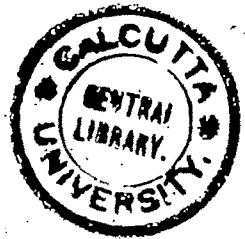


CU12-H03422-11-0298416



11

AMERICAN SOCIOLOGICAL REVIEW
Volume 38 Number 1

	Coleman	Loss of Power	
Key	Monothetic and Polythetic Typologies	Kennedy	Minority Status and Fertility
Idleton	Christian Beliefs and Anti-Semitism	Kohn Schooler	Occupation and Psychological Functioning
Glock Stark	Comment and Reply	Goldman	Managerial Mobility Motivations
Summen	Political Orientations Among Children	Blume Sinclair	The Reward System in Science
Downing L. L.	Income and Veteran Status	Hastings Robinson	Entry into First Marriage
	Hopkins	Political Overconformity and Mobility	

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit at least two copies, and retain a copy for your own files. Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—double-spaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated with reference. ["... have claimed that this is so (but see Jones, 1952:99 for a conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. ["... as many have noted (Johnson, 1942; Perry, 1947; Lindquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–66.

1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

293416

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

FEBRUARY, 1973

VOLUME 38 NUMBER 1

301.05

Vol. 38 Nos. 1-6

AM 35

FEB - DEC

1973

ARTICLES

Loss of Power.....	JAMES S. COLEMAN	1
Monothetic and Polythetic Typologies and Their Relation to Conceptualization, Measurement and Scaling.....	KENNETH D. BAILEY	18
Do Christian Beliefs Cause Anti-Semitism?.....	RUSSELL MIDDLETON	33
Do Christian Beliefs Cause Anti-Semitism?—A Comment.....	CHARLES Y. GLOCK AND RODNEY STARK	53
Response.....	RUSSELL MIDDLETON	59
The Development of Political Orientations Among Black and White Children.....	ANTHONY M. ORUM AND ROBERTA S. COHEN	62
Income and Veteran Status: Variations Among Mexican Americans, Blacks and Anglos.....	HARLEY L. BROWNING, SALLY C. LOPREATO AND DUDLEY L. POSTON, JR.	74
Minority Group Status and Fertility: The Irish.....	ROBERT E. KENNEDY, JR.	85
Occupational Experience and Psychological Functioning: An Assessment of Reciprocal Effects.....	MELVIN L. KOHN AND CARMI SCHOOLER	97
Managerial Mobility Motivations and Central Life Interests.....	DANIEL R. GOLDMAN	119
Chemists in British Universities: A Study of the Reward System in Science.....	S. S. BLUME AND RUTH SINCLAIR	126
A Re-examination of Hernes' Model on the Process of Entry into First Marriage for United States Women, Cohorts 1891-1945.....	DONALD W. HASTINGS AND J. GREGORY ROBINSON	138
Political Overconformity by Upwardly Mobile American Men.....	ANDREW HOPKINS	143

Editor: JAMES F. SHORT, JR.*Deputy Editors:* LOIS B. DEFLEUR AND LEE FREESE

Associate Editors: THEODORE R. ANDERSON, CHARLES BOWERMAN, ERNEST Q. CAMPBELL, WILLIAM R. CATTON, JR., LAMAR T. EMPEY, DORIS R. ENTWISLE, EDGAR G. EPPS, REYNOLDS FARLEY, BLANCHE GEER, NORVAL D. GLENN, ALLEN GRIMSHAW, TRAVIS W. HIRSCH, LEWIS M. KILLIAN, EDWARD O. LAUMANN, GARY T. MARX, MARSHALL W. MEYER, GWYNN NETTLER, CHARLES B. PERROW, SEYMOUR SPILERMAN, ARTHUR STINCHCOMBE, DONALD J. TREIDMAN, HARRIET ZUCKERMAN.

Copy Editor: LORETTA ANAWALT*Office Manager:* SHERRY BYE

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, news and announcements, employment bulletin, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The American Sociological Review is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; institutions and libraries, \$20; non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

8

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: MIRRA KOMAROVSKY
Vice-President: RAYMOND W. MACK
President-Elect: PETER M. BLAU
Vice-President-Elect: MATILDA W. RILEY
Secretary: J. MILTON YINGER
Past-President: WILLIAM J. GOODE
Executive Officer: OTTO N. LARSEN (ex officio)

Members-at-Large: HUBERT M. BLALOCK, JR., ORVILLE G. BRIM, ROSE LAUB COSER, JAMES A. DAVIS, JACK P. GIBBS, JOSEPH GUSFIELD, SUZANNE KELLER, KARL F. SCHUESSLER, RITA JAMES SIMON, JEROME H. SKOLNICK, RUTH H. USEEM, WALTER L. WALLACE.

(Articles in the REVIEW are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

February, 1973, ASR

■ This issue begins the 38th volume of ASR. It features new theoretical and empirical treatments of old sociological questions and a few new ones. And with it we officially welcome the following newly appointed Associate Editors: William R. Catton, Jr., Doris R. Entwisle, Blanche Geer, Norval D. Glenn, Gary T. Marx, and Marshall W. Meyer. My thanks to each for past, present, and future help in the enterprise. With the exception of Bill Catton, who only recently moved from New Zealand to his present post at the University of Wyoming, this group has been reviewing manuscripts on a regular basis for several months, since confirmation of their appointments by the Publications Committee.

■ The lead article in this issue is by **James S. Coleman**, Professor of Social Relations at Johns Hopkins. For the past four years, Coleman has been engaged in studying fundamental aspects of relationships between individual actors and purposively organized collective bodies (corporate actors). The paper published herein is from that larger study.

■ The second article is by **Kenneth D. Bailey**, Assistant Professor of Sociology and Program Director of the Population Research Program, Survey Research Center at the University of California, Los Angeles. His article is the third in a series on qualitative and quantitative methods of typology construction. His current research includes a comparative study of cluster analysis techniques and a variety of population studies.

■ The third article in this issue, by **Russell Middleton**, begins an exchange concerning the relationship between Christian beliefs and anti-semitism. Middleton is Professor of Sociology at the University of Wisconsin, Madison, and is a member of the staff of the Institute for Research on Poverty. He is currently engaged in research on factors related to self-esteem among black and

white men in Milwaukee and rural North Carolina and among the Kipsigis and Kikuyu of Kenya. He is also analyzing the social and social psychological impact of the three-year negative income tax experiments in several cities in New Jersey and Pennsylvania and in rural counties in North Carolina and Iowa. Middleton's article is commented on by **Charles Y. Glock** and **Rodney Stark**, co-authors of earlier work examining the relationship between Christian beliefs and anti-semitism. Glock and Stark are, respectively, Professor of Sociology at the University of California at Berkeley, and Associate Professor of Sociology at the University of Washington.

■ Co-authors of the next article are **Anthony M. Orum** and **Roberta S. Cohen**. Orum is Associate Professor of Sociology at the University of Texas (Austin). He is interested in various aspects of the political socialization experiences of young black and white children and identification and synthesis of the intellectual and empirical foundations of political sociology. Roberta S. Cohen is a doctoral candidate at the University of Illinois (Urbana). Her dissertation research concerns family influences in the political socialization process.

■ **Harley L. Browning** is Professor of Sociology and Director of the Population Research Center, University of Texas at Austin. He is co-author with Jorge Balan and Elizabeth Jelin of a forthcoming book, *Men in a Developing Society: Geographic and Social Mobility in Monterrey, Mexico*. Currently he is engaged in cross-national study of the sectoral transformation of the labor force, particularly the emergence of the "service economy." **Sally Cook Lopreato** is a teaching assistant and doctoral candidate in the Sociology Department of the University of Texas at Austin. She is engaged in research on the formalization of classical theory and on various aspects of social mobility. **Dudley L. Poston, Jr.** is Assistant Professor of Sociology and Associate Director of the Population Research Cen-

(Continued on p. 148)



AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

APRIL, 1973

VOLUME 38 NUMBER 2

ARTICLES

Bureaucracy and Modernization in China: The Maoist Critique.....	MARTIN KING WHYTE	149
On the Use of the Mass Media for Important Things.....	ELIHU KATZ, MICHAEL GUREVITCH, AND HADASSAH HAAS	164
Technology and Household and Configuration in Urban Africa: The Bassa of Monrovia.....	W. PENN HANDWERKER	182
Social Change, Migration and Family Interaction in Brazil.....	BERNARD C. ROSEN	198
New Directions in the Study of Community Elites.....	EDWARD O. LAUMANN AND FRANZ URBAN PAPPI	212
The Ecological Approach in Measuring Community Power Concentration: An Analysis of Hawley's MPO Ratio.....	JAMES M. WILLIAMS	230
Some Methodological Issues in Cohort Analysis of Archival Data... ..	KAREN OPPENHEIM MASON, WILLIAM M. MASON, H. H. WINSBOROUGH, AND W. KENNETH POOLE	242
Psychological Reductionism, Methodological Individualism, and Large Scale Problems.....	MURRAY WEBSTER, JR.	258
Symbolic Interaction as a Pragmatic Perspective: The Bias of Emergent Theory..	JOAN HUBER	274
Erratum		284

Editor: JAMES F. SHORT, JR.

Deputy Editors: LOIS B. DEFLEUR AND LEE FREESE

Associate Editors: THEODORE R. ANDERSON, CHARLES BOWERMAN, ERNEST Q. CAMPBELL, WILLIAM R. CATTON, JR., LAMAR T. EMPEY, DORIS R. ENTWISLE, EDGAR G. EPPS, REYNOLDS FARLEY, BLANCHE GEER, NORVAL D. GLENN, ALLEN GRIMSHAW, TRAVIS W. HIRSCH, LEWIS M. KILLIAN, EDWARD O. LAUMANN, GARY T. MARX, MARSHALL W. MEYER, GWYNN NETTLE, CHARLES B. PERROW, SEYMOUR SPILERMAN, ARTHUR STINCHCOMBE, DONALD J. TREIMAN, HARRIET ZUCKERMAN.

Copy Editor: LORETTA ANAWALT

Office Manager: SHERRY BYE

Executive Officer: OTTO N. LARSEN

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The American Sociological Review is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; institutions and libraries, \$30, non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: MIRRA KOMAROVSKY

Vice-President: RAYMOND W. MACK

President-Elect: PETER M. BLAU

Vice-President-Elect: MATILDA W. RILEY

Secretary: J. MILTON YINGER

Past-President: WILLIAM J. GOODE

Executive Officer: OTTO N. LARSEN (ex officio)

Members-at-Large: HUBERT M. BLALOCK, JR., ORVILLE G. BRIM, ROSE LAUB COSER, JAMES A. DAVIS, JACK P. GIBBS, JOSEPH GUSFIELD, SUZANNE KELLER, KARL F. SCHUESSLER, RITA JAMES SIMON, JEROME H. SKOLNICK, RUTH H. USEEM, WALTER L. WALLACE.

(Articles in the REVIEW are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

April, 1973, ASR

■ With this issue we hope to end the tardiness which has marked recent issues of ASR. We have moved up our deadlines and with the cooperation of publisher Henry Quellmalz (always assured), our readers, authors, and the editor (regrettably not always assured), and our capable staff, we are determined to get ASR to our readers in the month of publication. My sincere apologies for past lapses.

■ The issue begins with five articles reporting sociological critiques and findings from non-U.S. settings. The first in this series is **Martin King Whyte's** "Maoist Critique" of bureaucracy and modernization theory. Whyte is Assistant Professor and Associate Chairman of the Department of Sociology, and an Associate of the Center for Chinese Studies at the University of Michigan. He is currently involved in a cross-cultural study of the relative status of women and in research on contemporary family customs and family change in the People's Republic of China.

■ **Elihu Katz** is Director of the Communications Institute at the Hebrew University, Jerusalem, recently on leave as Simon Research Fellow at the University of Manchester. His collaborators, **Michael Gurevitch** and **Hadassah Haas** are also associated with the Hebrew University, and with the Israel Institute of Applied Social Science. Their paper reports data "On the Use of the Mass Media for Important Things" based on a survey conducted in Israel. All three coauthors are involved in communications research, with emphasis on mass media.

■ The third article in this series is by **W. Penn Handwerker**, Assistant Professor of Anthropology at California State University, Humboldt. His principal research interests are food and population problems in tropical Africa, and household and kinship structure. He is currently analyzing and writing up data on the production and sales of food crops in the Republic of Liberia.

■ **Bernard C. Rosen**, Professor of Sociology at Cornell University, continues his research on social change in Brazil. Currently on leave in England, and writing a monograph on this subject, he also expects to conduct research on family structure and socialization in middle and lower class families in London.

■ The last of these five papers is by **Edward O. Laumann**, presently Visiting Professor at the University of Chicago, and **Franz V. Pappi**, on leave from the Zentralarchiv für empirische Sozialforschung at the Universität zu Köln. Laumann is in the data analysis phase of a study of a West German community, involving four surveys of various population elements, including the community elite, a cross-section of the community and of their nominated friends, and of senior research personnel at the natural science research institute. Pappi is working on a monograph concerning methodological problems in the analysis of aggregated census and survey data. He is also interested in political sociology, especially electoral behavior. The two will again join forces in Germany for the summer, to continue their community study.

■ Laumann and Pappi's study of elites is followed by **James M. Williams'** research on the ecological approach to measuring community power concentration. Williams is Assistant Professor of Sociology at the University of Wisconsin, Eau Claire. He is currently engaged in a demographic study of the American Indian. Fertility and mortality trends from 1900 to the present are being identified using published data, and a small survey has been completed on a reservation in Wisconsin to gather information on fertility, contraception, migration, mortality, health, economic conditions and family life.

■ Other papers in this issue treat a variety of fundamental methodological and theoretical topics and important substantive areas. Authors and their interests follow.

(Continued on p. 284)



AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

JUNE, 1973

VOLUME 38 NUMBER 3

ARTICLES

- Causal Models and Social Indicators: Toward the Development of Social Systems Models.... JAMES G. ANDERSON 285
- Trends in the Occupational Mobility of U.S. Men, 1962-1970..... ROBERT M. HAUSER AND DAVID L. FRATHERMAN 302
- On Urban Alienations and Anomie: Powerlessness and Social Isolation.... CLAUDE S. FISCHER 311
- Education and Prejudice or Education and Response-Set?..... MARY R. JACKMAN 327
- The Invocation of Legal Norms: An Empirical Investigation of Durkheim and Weber..... B. C. CARTWRIGHT AND R. D. SCHWARTZ 340
- On Phenomenological Sociology JAMES L. HEAP AND PHILLIP A. ROTH 354
- Social Problems, Problematic Situations, and Quasi-Theories..... JOHN P. HEWITT AND PETER M. HALL 367
- Ministerial Roles and Social Actionist Stance: Protestant Clergy and Protest in the Sixties.... HART M. NELSEN, RAYTHA YOKLEY AND THOMAS MADRON 375

COMMENTS

- On Hummon's Mathematical Formulation of Blau's Theory of Differentiation in Organizations..... FOSTER G. DIECKHOFF 387
- Comment on Williams' "The Ecological Approach in Measuring Community Power Concentration"..... AMOS H. HAWLEY 390
- Reply to Hawley JAMES M. WILLIAMS 391
- Comments on "Hall's Professionalism Scale: An Empirical Reassessment" JOHN W. FOX AND JOHN A. VONK 392
- Reply to Fox and Vonk WILLIAM E. SNIZEK 395
- Erratum 396

Editor: JAMES F. SHORT, JR.

Deputy Editors: LOIS B. DEFLEUR AND LEE FREESE

Associate Editors: THEODORE R. ANDERSON, CHARLES BOWERMAN, ERNEST Q. CAMPBELL, WILLIAM R. CATTON, JR., LAMAR T. EMPEY, DORIS R. ENTWISLE, EDGAR G. EPPS, REYNOLDS FARLEY, BLANCHE GEER, NORVAL D. GLENN, ALLEN GRIMSHAW, NEIL W. HENRY, TRAVIS W. HIRSCHI, LEWIS M. KILLIAN, EDWARD O. LAUMANN, GARY T. MARX, MARSHALL W. MEYER, GWYNN NETTLER, CHARLES B. PERROW, SEYMOUR SPIELMAN, ARTHUR STINCHCOMBE, DONALD J. TREIMAN, HARRIET ZUCKERMAN.

Copy Editor: LORETTA ANAWALT

Office Manager: SHERRY BYE

Executive Officer: OTTO N. LARSEN

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The American Sociological Review is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; institutions and libraries, \$30, non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Changes of address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: MIRRA KOMAROVSKY

Vice-President: RAYMOND W. MACK

President-Elect: PETER M. BLAU

Vice-President-Elect: MATILDA W. RILEY

Secretary: J. MILTON YINGER

Past-President: WILLIAM J. GOODE

Executive Officer: OTTO N. LARSEN (ex officio)

Members-at-Large: HUBERT M. BLALOCK, JR., ORVILLE G. BRIM, ROSE LAUB COSER, JAMES A. DAVIS, JACK P. GIBBS, JOSEPH GUSFIELD, SUZANNE KELLER, KARL F. SCHUESSLER, RITA JAMES SIMON, JEROME H. SKOLNICK, RUTH H. USEEM, WALTER L. WALLACE.

(Articles in the REVIEW are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

June, 1973, ASR

■ This is a "short" issue, necessarily so because we have exceeded our page allocation for the past two issues. Articles and comments in the issue encompass an exceptionally broad range, however, providing good end-of-the-academic-year reading and much food for thought. Article authors, and their research interests and activities follow.

■ **James G. Anderson** is Associate Professor of Sociology and Industrial Engineering at Purdue University. He is currently Associate Director of the Health Services Research and Training Program in the Department of Sociology. His research involves the development of causal models to describe the organization of health care and patient behavior.

■ **Robert M. Hauser and David L. Featherman** are Associate Professors of Sociology and Rural Sociology, respectively, at the University of Wisconsin, Madison. They are collaborating in replication and extension of the "Occupational Changes in a Generation" survey. Hauser is also working with William H. Sewell in his panel study of the achievement of Wisconsin high school graduates, and he is interested in methodological issues in the use of causal models in sociology. Featherman has a strong interest in the social and economic achievement of women and in the relationship between labor force activities and achievement of spouses and their marital adjustments. His earlier work in this area is reflected in a book recently released, co-authored with O. D. Duncan and Beverly Duncan, *Socioeconomic Background and Achievement*.

■ **Claude S. Fischer**, a recent Harvard Ph.D., is a Lecturer in the Department of Sociology at the University of California, Berkeley. He is also associated with the Institutes of Urban and Regional Development, and Human Development. He is especially interested in the social psychology of urban life, social conflict and alienation, and in systems and network analysis.

■ **Mary R. Jackman** is Assistant Professor of Sociology at Michigan State University. The present paper reflects her continuing interest in the social-psychological consequences of social stratification. Currently she is engaged in further analysis of the structure and determinants of ethnic attitudes, and an examination of alternative models of subjective class identification.

■ **Bliss C. Cartwright** is a post-doctoral fellow in the Russell Sage Program in Law and Society at Yale Law School. He is interested in quantitative, longitudinal studies of legal institutions, with particular emphasis on relating functions of the courts to changing patterns of litigation. He is collaborating with K. Ikeda and R. Lempert on an analysis of legal interventions in Hawaii public housing projects, focussing on the effects of maximum income rules on mobility behavior. He has begun groundwork for an archival study of legal and structural changes in state supreme courts, 1870-1970. **Richard D. Schwartz** is dean of the Law School, State University of New York at Buffalo. A sociologist by training and self-definition, he describes his current research as participant observation of interdisciplinary conflict and cooperation in the development of an institution of higher education.

■ **James L. Heap and Phillip A. Roth** are doctoral students at the University of British Columbia interested in phenomenology and ethnomethodology. Heap is doing research on reality construction through documents and is editor of a recently published volume, *Everybody's Canada*. Roth is doing fieldwork in rural British Columbia on the construction of oral history accounts.

■ **John P. Hewitt** is Associate Professor of Sociology at the University of Massachusetts, Amherst. As part of a broader interest in collective behavior, he is especially concerned with the prob-

(Continued on p. 397)

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

AUGUST, 1973

VOLUME 38 NUMBER 4



ARTICLES

- Classic on Classic: Parsons' Interpretation of Durkheim.....WHITNEY POPE 399
- Axiomatic Theory, Informative Value of Propositions, and "Derivation Rules of Ordinary Language".....SIAMAK MOVAHEDI AND RICHARD H. OGLES 416
- Functional Alternatives and Economic Development: An Empirical Example of Permanent Employment in Japan.....ROBERT E. COLE 424
- Education and Mobility: From Achievement to Ascription.....BARBARA JACOBSON AND JOHN M. KENDRICK 439
- Group Disorders in the Public Schools.....PAUL RITTERBAND AND RICHARD SILBERSTEIN 461
- System Size and Ruling Elites.....BRUCE H. MAYHEW 468
- Size and Ruling Elites: Effects of System Growth on Power Structures.....MAXIMILIAN H. VONBROEMBSSEN AND LOUIS N. GRAY 476
- System Size and Structural Differentiation in Formal Organizations: An Alternative Baseline Generator.....DAVID A. SPECHT 479
- Causal Chain Models for the Socioeconomic Career.....JONATHAN KELLEY 481

COMMENTS

- "Ordinal Regression?" A Comment.....K. I. MACDONALD 494
- "Hardship and Collective Violence in France": A Comment.....CHARLES N. HALABY 495
- How to Get From Here to There.....DAVID SNYDER AND CHARLES TILLY 501
- Exchange as Symbolic Interaction: For What?.....CARRELL W. ABBOTT, CHARLES R. BROWN AND PAUL V. CROSBIE 504
- On the Relification of Paradigms: Reply to Abbott, Brown, and Crosbie..PETER SINGELMANN 506
- Comments on Hope's Mobility and Fertility Paper.....MILES E. SIMPSON 509

Editor: JAMES F. SHORT, JR.

Deputy Editors: LOIS B. DEFLEUR AND LEE FREESE

Associate Editors: THEODORE R. ANDERSON, CHARLES BOWERMAN, ERNEST Q. CAMPBELL, WILLIAM R. CATTON, JR., LAMAR T. EMPEY, DORIS R. ENTWISLE, EDGAR G. EPPS, REYNOLDS FARLEY, BLANCHE GEER, NORVAL D. GLENN, ALLEN GRIMSHAW, NEIL W. HENRY, TRAVIS W. HIRSCHI, LEWIS M. KILLIAN, EDWARD O. LAUMANN, GARY T. MARX, MARSHALL W. MEYER, GWYNN NETTLE, CHARLES B. PERROW, SEYMOUR SPILERMAN, ARTHUR STINCHCOMBE, DONALD J. TREIDMAN, HARRIET ZUCKERMAN.

Copy Editor: LORETTA ANAWALT

Office Manager: SHERRY BYE

Executive Officer: OTTO N. LARSEN

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The American Sociological Review is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; institutions and libraries, \$20; non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of Address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: MIRRA KOMAROVSKY

Vice-President: RAYMOND W. MACK

President-Elect: PETER M. BLAU

Vice-President-Elect: MATILDA W. RILEY

Secretary: J. MILTON YINGER

Past-President: WILLIAM J. GOODE

Executive Officer: OTTO N. LARSEN (ex officio)

Members-at-Large: HUBERT M. BLALOCK, JR., ORVILLE G. BRIM, ROSE LAUB COSER, JAMES A. DAVIS, JACK P. GIBBS, JOSEPH GUSFIELD, SUZANNE KELLER, KARL F. SCHUESSLER, RITA JAMES SIMON, JEROME H. SKOLNICK, RUTH H. USEEM, WALTER L. WALLACE.

(Articles in the REVIEW are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

August, 1973

■ Articles in this issue range from assessment of classical theoretical statements and formal theoretical rules to mathematical models and empirical research on a variety of topics. It concludes with unusually pithy exchanges between authors and critics.

■ **Whitney Pope** is Assistant Professor of Sociology at Indiana University. He has just completed a monograph on Durkheim's *Suicide* and is conducting further research on this topic. With a variety of co-authors, he is also engaged in assessments of the contributions of Tocqueville, Weber, Freud, and Parsons. **Siamak Movahedi** is Assistant Professor of Sociology at the University of Massachusetts, Boston. His major areas of interest are sociology of science and methodology. Currently he is working on probabilistic logic and its application to sociological methodology. His collaborator is **Richard Ogles**, Professor of Sociology at Tulsa University. His current work is in philosophy of social science, especially methodological criteria for the assessment of claims regarding cumulative knowledge in sociology. He is also studying the effects of concentration of economic and political power in advanced industrial societies.

■ **Robert E. Cole** is Associate Professor of Sociology and Associate of the Center for Japanese Studies at the University of Michigan. The article presented in this issue grows out of prior research on Japanese workers published in *Japanese Blue-Collar: The Changing Tradition* (University of California Press). He is currently studying work history patterns in Detroit and Yokohama based on samples of their respective labor forces, and he is writing a book on theories of social change.

■ **John M. Kendrick** and **Barbara Jacobson** are Assistant Professors at Yale and at Herbert H. Lehman College, City University of New York, respectively. They continue to collaborate on the

Puerto Rican project from which their article is drawn. In addition, Kendrick is studying ways in which colonialization and modernization have shaped patterns of conflict and accommodation between Anglos and Chicanos in the Southwestern U.S., and Jacobson is examining the impact of open admissions in CUNY.

■ **Paul Ritterband**, Associate Professor, City College of the City University of New York, has just completed a monograph on international education and migration. He is now working in two areas: (a) The Ethnic Politics of the Public Schools, (b) Jewish Learning in American Universities. **Richard A. Silberstein** is a sociology graduate student at Columbia University. He is studying the relationship between student, teacher, and administrator characteristics and educational achievement. Current interests include methodology and computer utilization in data analysis.

■ Three articles in this issue concern implications of system size. Among their authors, **Bruce H. Mayhew**, Associate Professor of Sociology at Temple University, is currently working on a theory of structural complexity in social systems. **M. H. von Broembsen** is Assistant Professor of Sociology and Assistant Director of the Social Research Center, and **Louis N. Gray** is Associate Professor of Sociology, both at Washington State University. Their research interests are in mathematical sociology, social power, and behavioral sociology. They are engaged in further research on models of differentiation and on effects of mass media stimuli. The primary areas of interest of **David A. Specht**, Assistant Professor of Sociology at Iowa State University, include formal models of social phenomena, applied statistics, and data management and processing. Specifically, he is investigating the use of probability models in the ecological analysis of residential patterns.

(Continued on p. 512)

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

OCTOBER, 1973

VOLUME 38 NUMBER 5

ARTICLES

- A Cross-Species Comparison of Status in Small Established Groups.....ALLAN MAZUR 513
A Behavioral Model of Man: Propositions and Implications.....
.....JOHN H. KUNKEL AND RICHARD H. NAGASAWA 530
Some Developmental Interpersonal Dynamics Through Childhood.....H. W. SMITH 543
Disturbance in the Self-Image at Adolescence.....
.....ROBERTA G. SIMMONS, FLORENCE ROSENBERG AND MORRIS ROSENBERG 553
An Interpretation of the Relation Between Objective and Subjective Social Status.....
.....MARY R. JACKMAN AND ROBERT W. JACKMAN 569
A Theory of Middleman Minorities.....EDNA BONACICH 583
The Potential for Residential Integration in Cities and Suburbs: Implications for the Busing
ControversyALBERT I. HERMALIN AND REYNOLDS FARLEY 595
The Causes of Racial Disorders: A Grievance-Level Explanation.....
.....WILLIAM R. MORGAN AND TERRY NICHOLS CLARK 611
Attitude and Action: A Field Experiment Joined to a General Population Survey.....
ROBERT BRANNON, GARY CYPHERS, SHARLENE HESSE, SUSAN HESSELBART, ROBERTA KEANE,
.....HOWARD SCHUMAN, THOMAS VICCARO AND DIANA WRIGHT 625
Voluntary Associations and Minority Status: A Comparative Analysis of Anglo, Black, and
Mexican Americans..J. ALLEN WILLIAMS, JR., NICHOLAS BABCHUK AND DAVID R. JOHNSON 637

Editor: JAMES F. SHORT, JR.

Deputy Editors: LOIS B. DEFLEUR AND LEE FREESE

Associate Editors: THEODORE R. ANDERSON, CHARLES BOWERMAN, ERNEST Q. CAMPBELL, WILLIAM R. CATTON, JR., LAMAR T. EMPEY, DORIS R. ENTWISLE, EDGAR G. EPPS, REYNOLDS FARLEY, BLANCHE GEER, NORVAL D. GLENN, ALLEN GRIMSHAW, NEIL W. HENRY, TRAVIS W. HIRSCH, LEWIS M. KILLIAN, EDWARD O. LAUMANN, GARY T. MARX, MARSHALL W. MEYER, GWYNETH NETTLER, CHARLES B. PERROW, SEYMOUR SPILERMAN, ARTHUR STINCHCOMBE, DONALD J. TREIMAN, HARRIET ZUCKERMAN.

Copy Editor: LORETTA ANAWALT

Office Manager: SHERRY BYE

Executive Officer: OTTO N. LARSEN

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The American Sociological Review is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; Institutions and Libraries, \$20; non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of Address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: MIRRA KOMAROVSKY

Vice-President: RAYMOND W. MACK

President-Elect: PETER M. BLAU

Vice-President-Elect: MATILDA W. RILEY

Secretary: J. MILTON YINGER

Past-President: WILLIAM J. GOODE

Executive Officer: OTTO N. LARSEN (ex officio)

Members-at-Large: HUBERT M. BLALOCK, JR., ORVILLE G. BRIM, ROSE LAUB COSER, JAMES A. DAVIS, JACK P. GIBBS, JOSEPH GUSFIELD, SUZANNE KELLER, KARL F. SCHUESSLER, RITA JAMES SIMON, JEROME H. SKOLNICK, RUTH H. USEEM, WALTER L. WALLACE.

(Articles in the Review are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

October, 1973

■ This issue begins with examination of fundamental propositions concerning status among homo sapiens and lower species, and a behavioral model of man, and extends to considerations of basic socialization processes, stratification, and minority-majority attitudes and behaviors.

■ **Allan Mazur** is Associate Professor of Sociology at Syracuse University. His research interests include biological aspects of social behavior and social aspects of science and technology. In the latter area he is completing a comparative study of controversies over fluoridation and nuclear power plants. **John H. Kunkel** is Professor of Sociology at the University of Western Ontario. His major interest is in the area of social change and development. At present he is analyzing the role of models of man and society in the study of social problems and the guidance of social change. **Richard H. Nagasawa** is Assistant Professor of Sociology at Arizona State University. He is interested in social psychology and labeling aspects of deviance. At present he is working on theory construction, particularly the quantification and utilization of axiom systems.

■ Two socialization papers are authored by **H. W. Smith** and coauthored by **Roberta G. Simmons**, **Florence Rosenberg** and **Morris Rosenberg**, respectively. Smith, Assistant Professor of Sociology at the University of Missouri-St. Louis, currently is involved in research on the reliability and validity of observational measures. He is also engaged in experimental study of the social effects of marijuana intoxication, and he has continuing research interests in developmental social psychology. Simmons, Associate Professor of Sociology and Psychiatry, University of Minnesota, Florence Rosenberg, Assistant Professor of Sociology, the American University, and Morris Rosenberg, Research Sociologist, National Institute of Mental Health, have been interested in the sociology of the self-concept for a number of years. Simmons is currently study-

ing the self-image of kidney transplant patients and their family decision-making processes, as well as collaborating with Florence Rosenberg on analysis of the self-concept of school-age boys and girls. Morris Rosenberg is investigating the self-concept effects of the dissonant context.

■ The remainder of the articles in this issue touch upon one or another aspect of stratification or minority-majority relations. Authors and their research interests and activities follow.

■ **Mary R. Jackman**, Assistant Professor of Sociology at the University of Michigan, is currently engaged in research on the structure and determinants of attitudes toward ethnic and political groups. **Robert W. Jackman**, Assistant Professor of Political Science at Michigan State University, has research interests in political sociology and public opinion, and is currently completing a study of cross-national variations in income equality and social welfare.

■ **Edna Bonacich** is Assistant Professor of Sociology at the University of California, Riverside. Her principal interest is in developing a general theory of ethnic and race relations that focuses on class conflict rather than on racial or ethnic differences, per se. Current topics of study include the abolitionist movement and the Japanese Americans as a middleman minority.

■ **Albert I. Hermalin** is Associate Professor of Sociology and a Research Associate of the Population Studies Center, University of Michigan. In addition to research on the family and economic status of blacks, he is engaged in research on the evaluation of fertility trends and the role of the family planning programs in Taiwan. His collaborator, **Reynolds Farley**, is Associate Director of the Population Studies Center and Associate Professor of Sociology at the University of Michigan. His research interests focus upon the demog-

(Continued on p. 647)

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

DECEMBER, 1973

VOLUME 38 NUMBER 6

ARTICLES

Some Problems in Role Analysis	MIRRA KOMAROVSKY	649
The Politics of Birth Practices: A Strategic Analysis	KAREN E. PAIGE AND JEFFERY M. PAIGE	663
A Dip in Deaths Before Ceremonial Occasions: Some New Relationships Between Social Integration and Mortality	DAVID P. PHILLIPS AND KENNETH A. FELDMAN	678
The Internal Stratification of the Working Class: System Involvements of Auto Workers in Four Countries	WILLIAM H. FORM	697
Social Networks and Voting: The Resurrection of a Research Agenda	CARL A. SHEINGOLD	712
The Assembling Process: A Theoretical and Empirical Examination	CLARK MCPHAIL AND DAVID L. MILLER	721
Values and Violence: A Test of the Subculture of Violence Thesis	SANDRA J. BALL-ROKEACH	736
Environment, Technology and the Administrative Intensity of Manufacturing Organizations	JOHN HENRY FREEMAN	750
Two Sources of Error in Ecological Correlations	JOHN L. HAMMOND, JR.	764
Non-Random Exogenous Variables in Path Analysis	DAVID L. KLEMMACK, THOMAS A. LEGGETTE AND LAWRENCE S. MAYER	778

COMMENTS

Comments on Models for the Socioeconomic Career	DAVID L. FEATHERMAN	785
History, Causal Chains and Careers: A Reply	JONATHAN KELLEY	791
Comment on "Symbolic Interaction as a Pragmatic Perspective: The Bias of Emergent Theory"	HERBERT BLUMER	797
Reply to Blumer: But Who Will Scrutinize the Scrutinizers?	JOAN HUBER	798
Comment on "Role Differentiation"	PETER J. BURKE	801
Reply to Burke	GORDON H. LEWIS	802
Patterns of Female Intergeneration Occupational Mobility: A Comment	NATALIE ROGOFF RAMSDØY	806
Patterns of Female Intergeneration Occupational Mobility: Response to Ramsdøy	PETER Y. DEJONG, MILTON J. BROWER AND STANLEY S. ROBIN	807
Reply to Simpson	KEITH HOPE	809

INDEX	814
-------------	-----

Editor: James F. Short, Jr.

Deputy Editors: Lois B. DeFleur and Lee Freese

Associate Editors: Theodore R. Anderson, Charles Bowerman, Ernest Q. Campbell, William R. Catton, Jr., LaMar T. Empey, Doris R. Entwisle, Edgar G. Epps, Reynolds Farley, Blanche Geer, Norval D. Glenn, Allen Grimshaw, Neil W. Henry, Travis W. Hirschi, Lewis M. Killian, Edward O. Laumann, Gary T. Marx, Marshall W. Meyer, Gwynn Nettler, Charles B. Perrow, Seymour Spilerman, Arthur Stinchcombe, Donald J. Theiman, Harriet Zuckerman.

Copy Editor: Loretta Anawalt

Office Manager: Sherry Bye

Executive Officer: Otto N. Larsen

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, Washington State University, Pullman, Washington 99163.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

The *American Sociological Review* is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office. Copyright © 1973 American Sociological Association.

Annual membership dues of the Association, including subscription: Member, \$30; Student Member, \$15; Associate, \$20; Student Associate, \$10; International Associate \$12.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15; institutions and libraries, \$20; non-member students, \$7. Single issues, \$4.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of Address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

AMERICAN SOCIOLOGICAL ASSOCIATION

Members of the Council

Officers of the Association for the Year 1973

President: Mirra Komarovsky
Vice-President: Raymond W. Mack
President-Elect: Peter M. Blau
Vice-President-Elect: Matilda W. Riley
Secretary: J. Milton Yinger
Past-President: William J. Goode
Executive Officer: Otto N. Larsen (ex officio)

Members-at-Large: Hubert M. Blalock, Jr., Orville G. Brim, Rose Laub Coser, James A. Davis, Jack P. Gibbs, Joseph Gusfield, Suzanne Keller, Karl F. Schuessler, Rita James Simon, Jerome H. Skolnick, Ruth H. Useem, Walter L. Wallace.

(Articles in the REVIEW are indexed in the *Social Sciences and Humanities Index*, the *Public Affairs Information Service*, *Psychological Abstracts*, *Sociological Abstracts*, *Ayer's Guide*, *University Microfilms*, and *Abstracts for Social Workers*.)

ITEMS

December, 1973

■ This issue is special for a number of reasons. It is a presidential address issue, and MIRRA KOMAROVSKY is a very special person. We are pleased to present her presidential address, "Some Problems in Role Analysis." In the 1930's and 1940's Komarovsky was among the first to study systematically urban participation in voluntary associations. Her later work has focused primarily on marriage and sex roles, in both college-educated and blue-collar strata.

■ The issue is special, also, because it marks the opportunity to thank publically some 456 special readers who helped us during 1973 and seven Associate Editors whose three year terms expire with this issue. The latter include: Theodore R. Anderson, Ernest Q. Campbell, LaMar T. Empey, Allen Grimshaw, Edward O. Laumann, Arthur Stinchcombe, and Donald J. Treiman. The former are listed on the immediately preceding pages. My most humble and sincere thanks to each of these colleagues who have served the discipline so well.

■ We also mark the end of our second volume, which happily coincides with the recent announcement that Morris Zelditch, Jr. will succeed me as editor of ASR. I am personally gratified that Buzz Zelditch has accepted the Council's appointment. We look forward to working with him toward the orderly transfer of the editorial functions, and we look forward to July, 1974, when he will begin receiving manuscripts for evaluation.

■ In the meantime, back to the present issue and

forward to 1974. Authors of other articles, and their research interests and activities follow:

■ KAREN E. PAIGE is Assistant Professor of Psychology at the University of California, Davis. Her research interests include the social origins of beliefs about women and particularly about women's reproductive behavior. She is currently developing social and psychological indicators of changing beliefs about women in the United States. JEFFERY M. PAIGE is Assistant Professor of Sociology at the University of California, Berkeley. His research interests include the politics of conflict and revolutionary change, and he is currently engaged in a cross-national study of rural class structure and agrarian revolutions in emerging nations. Both Paiges are also Research Associates in the Berkeley Survey Research Center. Their paper in this issue reports part of a collaborative cross-cultural study of the effects of political conflict on the status and reproductive behavior of women in stateless societies.

■ DAVID P. PHILLIPS is Assistant Professor and KENNETH A. FELDMAN is Associate Professor in the Department of Sociology, State University of New York at Stony Brook. Their paper in this issue is part of a continuing investigation by Phillips of the relationships between social life and mortality. Phillips is currently analyzing electronic records of six million death certificates in order to investigate 1) the effect of psychological and social factors on diseases not previously thought to be psychosomatic,

(Continued on page 813)

LOSS OF POWER

JAMES S. COLEMAN

*Johns Hopkins University**American Sociological Review* 1973, Vol. 38 (February):1-17

Berle and Means show that modern organizations separate the rights inherent in property or other resources into benefit rights and usage rights. To increase benefits, natural persons in society give over usage rights, that is direct control over actions, to corporate bodies through investments or membership. In so doing, they have gained these benefits at the price of loss of power. This paper presents methods for calculating the extent of loss of power in society, and discusses some means that persons use to regain power.

I WOULD like to begin with a very simple basis for a theory of social action.¹ There are actors and events, and actors are related to events in two ways: control of actors over events, and consequences of events for actors. Further, actors are purposive, in that they exercise control to achieve outcomes beneficial to them. Thus we can think of the actor's control over events as his resources, and the differential benefits he receives from an event's outcome

as his interests. Then an event is any occurrence over which some actor has some control and in which some actor has interests. An actor is any person who has interests in events, some resources, and the ability to use those resources to implement his interests.

At this point, we must consider what entities in the real world correspond to actors thus defined. The answer is not as straightforward as it might seem. If we look to law, for example, we find that there are two kinds of persons in the eyes of the law, having resources, interests, and the ability to use those resources: natural persons, like you and me, and juristic persons, which are corporate bodies of various sorts. They include business corporations, churches, cities, trade associations, professional associations, trade unions, clubs, and so on. Before the law, juristic persons may be plaintiffs or defendants in various types of suits. They require special legal principles for various reasons, not least of which is the fact that they have no physical corpus. Their resources can always be traced back to real persons, though the chain may be long and complex.

Furthermore, juristic persons have a long history in law. In the thirteenth and fourteenth centuries, the law was beginning to recognize churches and towns as "fictional persons" as they were then termed, able to buy, sell, and own property, and as "persons" before the court. The legal theory that

¹ The work discussed in this paper is pursued at greater length (though without the formal models) in a forthcoming monograph [Coleman, (1973b)]. The problems and ideas in this paper are drawn from two sets of sources: work in the formal analysis of collective decisions through mathematical methods [see Coleman (1973a)], and work in substantive examination of the power that members of organizations are able to exercise toward the organization itself. Work of the first type is to be found primarily in journals of economics and political science. It is reviewed and summarized in A. K. Sen (1970). Steven Brams (1968) has used a formal approach similar to the one in this paper for somewhat different purposes. Work of the second type is best exemplified by Berle and Means' (1940) study of owners' power in corporations, and by Robert Michel's study of membership power in the Social Democratic Party in Germany (1949). Earlier work in which I had a part (Lipset, Trow, and Coleman, 1956) studied a trade union that appeared to deviate from the general pattern of little power by members.

I am grateful to Arthur Stinchcombe for insightful comments on an earlier draft of this paper.

has developed since then, especially in the nineteenth century, has ever increased their substantive existence, accommodated ever more varieties of purposive organizations as juristic persons, and, at least until this century, freed them more and more from the state.

One might object that such legal theory is relatively recent, and the social processes involving natural persons existed long before such juristic persons and will exist long after they have vanished. That view is too short-sighted. Only in the later middle ages, the eleventh, twelfth, and thirteenth centuries, did persons like you and me come to be regarded as "persons before the law."² For example, certain aspects of the law concerning villeins in bondage to their lord in thirteenth century England were developed by analogy to the law for domesticated animals. On the continent, villeins were even more fully regarded as chattels of the lord. Furthermore, such villeins constituted the major part of the population.³

A theory of social action consisting of purposive actors controlling events would not have been appropriate even for natural persons in that time. People like you and me had fixed estates in a hierarchy, with actions circumscribed by their estate. Emancipation from fixed estates, with rights to establish and break relationships and to control one's direction of life, was the first step in cracking open the closed societies of the middle ages. The next step was developing the right to join together and form corporate persons with rights of their own. These corporate rights constituted further emancipation from a hierarchical structure or the state, because corporate persons could and did exercise power vis-a-vis the state. In early days, this power was exercised by boroughs, the landed corporations; later, it has been most used by business corporations and trade unions.

It appears, then, both reasonable and ap-

propriate to let the analytic definition of actors carry us where it may, and not automatically identify actors with natural persons. When we do this, a number of fruitful questions arise that otherwise would not. I wish to examine one such question in this paper: How is power in society divided between these two types of actors? If we put all natural persons on one side, and all juristic persons, the corporate actors, on the other, how is power divided between these two sides? How much control over events of consequence does each side have? And how has that balance shifted over time?

Much care is needed in addressing such a question, lest corporate actors and their power vanish before our eyes—for ultimately they derive their resources from natural persons.

Yet we are not deceived in regarding a skyscraper as a tangible manifestation of corporate power, or the outcome of a wage negotiation as a battle between two corporate actors. Clearly, our senses tell us that corporate actors, not natural persons, hold a large amount of control over events of interest. If our theory does not confirm our senses in this, something is likely wrong with the theory rather than with our senses.

A person yields direct control over his resources to a corporate actor by joining it, or investing resources in it. The resources may be money (e.g., a financial investment, or membership dues), the right to act as his agent (e.g., in a trade union, the right to negotiate a wage contract), or time and effort. He may be stockholder, union member, professional association member, club member, or citizen of a town and a nation; and the resources he invests will differ in different cases. But in every case, he gives up control of resources. As Leonardo da Vinci, a solitary man, wrote, "When you're all alone, the whole world belongs to you; when you're with another, only half is yours." By giving up control, one expects to receive a greater gain than would be possible by using the resources personally. Even for so-called non-voluntary membership in corporate actors, such as a trade union in a closed shop, or at the extreme, a nation, this implicit calculation of relative benefit exists: would he be as well off by withdrawing his resources (i.e., his membership) from the corporate body as

² See Pollock and Maitland (1898:418) who refer to Bracton, the principal legal theorist of thirteenth century England.

³ See Pollock and Maitland (1898:432) who estimate that "the greater half of the rural population" in thirteenth century England was unfree. Since the rural population in pre-industrial societies accounts for over 90% of the total population, this indicates that the majority of the population had the status of serf or villein.

he is in maintaining it? The only difference lies in the fact that "non-voluntary" corporate bodies have become so by increasing the costs of non-membership, through legal and other means. The closed shop, for example, greatly increases the costs to a worker of non-membership in the union covering his occupation.

This yielding of control over the use of his resources to a corporate actor suggests a division of rights with regard to resources. One is the right of use, and the other is the right of ultimate ownership, including the right of withdrawal, and the right to benefit from the use of resources. I will call these *usage rights* and *benefit rights* respectively. Others have made a similar distinction: Berle and Means, for example, suggest that with the advent of large corporations, the concept of private property is no longer adequately descriptive. They suggest that the rights inherent in this power have been separated into two: "active property," which is the power-in-use of this property, and "passive property," which includes all the residual rights of ownership, including the right of withdrawal and the right to receive benefits from use of the property. This is precisely the distinction intended here, but extended to cover resources other than private property, such as wage negotiation rights and other rights a member of an organization might yield control of to an organization of which he is a member.

The right to use resources is what is ordinarily meant by power, while the right to benefit is the right to gain from that exercise of power. The structure of modern society often places these two sets of rights in the hands of different actors: corporate actors often hold the usage rights, or power, while persons often hold the benefit rights.

Now in general, people have found this alienation of usage rights or power beneficial—simply because the combined resources of a number of persons, used corporately, ordinarily have more power to gain benefits than when used separately. Thus if natural persons have lost power in modern society through the alienation or investment of resources in corporate actors, this does not imply that they are "worse off." Indeed, it suggests that in terms of material benefits, they are better off; they would not otherwise continue such investments. In fact, studies

have shown that people with heavy investments (through membership, etc.) in corporate actors are better off than those who are more isolated. But they have given over power to corporate actors to this end, and thus reduced their power to act autonomously. Their situation is a little like that of a rich child who because of his wealth receives large material benefits, but has no control of his resources. The child, of course, has no rights of withdrawal, while the person does. (The effectiveness of those rights is discussed in a later section.) My question, however, is not about benefits, but power: What has happened to natural persons' power to act in modern society, with its greatly expanded corporate structure?

When a person decides to yield control of his resources to a corporate body (e.g., to pay union dues or obey union strike calls), he expects to gain the greater power of combined resources. The decision is between acting independently with more freedom or collectively with more power.

In formulating a decision rule for the corporate body, the same problem arises. If its actions require unanimity, each member has full control, for his resources cannot be used in ways he does not like. Without his consent to act, the collectivity is paralyzed. If, however, the resources of the corporate actor can be committed at the will of one or a few, the corporate actor may be quite powerful; but a member can do little to prevent actions he does not like.

The Probability of Preventing Corporate Action

When an actor yields usage rights over resources, he gains a share of control over the collected resources of the corporate actor. The conditions of this transaction are ordinarily embodied in constitutions, and the form of control is ordinarily a vote in collective decisions that control the corporate actor. Thus, if his share of control is effective, he has lost nothing in casting his resources with the corporate actor. Nevertheless, one seldom in this world gains something for nothing; and it is useful to examine more suspiciously his share of control. Just what in the corporate actor does he control?

Work on measurement of power has treated this question. Shapley and Shubik (1954) developed a measure of power in a collectivity, based on the fraction of (ordered) coalitions of which the person is a member. For various reasons, this measure is not appropriate; but Coleman (1971), and Rae and Taylor (1971) have developed appropriate and equivalent modifications of it, by considering only unordered coalitions; I will use Coleman's approach here. In it, any decision rule gives rise to three calculations: the corporate actor's power to act; a member's power to prevent action; and a member's power to initiate action. With a symmetric decision rule like a majority rule, the power to prevent and the power to initiate action are identical. Since I will examine only majority decision rules here, I need examine only one, the power to prevent corporate action.

As indicated above, if the decision rule is asymmetric, as is a unanimity rule or a rule which allows any member to commit the corporate actor, the corporate actor's power to act may be very small (a unanimity rule) or very large (a single member rule). But if we consider only majority rules, we can examine the effect of the corporate actor's size on the members' control of it—that is, on the probability that a member could prevent corporate action if he chose to.

I will develop here a simple measure of a member's constitutional power to prevent corporate action. It is simply the fraction of all those voting patterns that lead to corporate action, in which the member can, by changing his vote, prevent corporate action. To illustrate, consider a corporate actor with three members. There are eight possible voting patterns in which the individuals are distinguishable, ranging from +++ to ---. Four give a positive outcome and lead to corporate action, while four lead to corporate inaction. Of the four which lead to action there are two (+ + - and + - +) in which the person on the left could block corporate action by changing his vote. Thus, the measure of his power to prevent corporate action is 2/4, or 0.5. Furthermore, if each of these four patterns is assumed equally likely, 0.5 is also his *probability* of preventing corporate action. Assuming equally likely patterns is equivalent to assuming that each is voting

independently and casts a positive vote with a probability of 1/2. This assumption is, of course, not intended to match reality, but to be of use for measuring the constitutionally-determined power, in the absence of knowledge about members' voting probabilities.

It is important to recognize that this measure is not a measure of the power of members toward each other, but the power of each in controlling the corporate actor. Thus each of the members in a three-person group with a majority decision rule has a power of 0.5 to control the actions of the corporate actor, i.e., the group. These numbers cannot be added, for adding two such numbers would imply the combined voting of two members in a coalition; and the power of a coalition is not the same as the sum of the power of the members.⁴

If this sounds like a peculiar way of describing reality in a three-person group, it sounds less peculiar when the collectivity is a thousand in size. There the idea of power of the individual vis-a-vis power of the corporate actor accords more with common sense, because we see the thousand-person collectivity as an actor, taking actions that can be little affected by any individual member. They can, perhaps, be greatly affected by coalitions of many individuals; but the dilemma remains for the individual; he must yield control of his vote to the coalition to gain the power of coalition. For the thousand-member corporate actor, the power of the individual members is, using the same considerations expressed in the three-man case, down to .025.⁵

More generally, for a corporate actor of size n , the power of any individual member is about $.8/\sqrt{n}$, and the power of the corporate actor in any action is approximately

⁴ For further analysis of the power of coalitions in decision-making collective bodies, see Coleman (1971) and (1970a).

⁵ The calculation is carried out as follows: There are $\frac{(n-1)!}{\left(\frac{n-1}{2}\right)! \left(\frac{n-1}{2}\right)!}$ combinations of the other

$n-1$ members in which he could prevent action by changing his positive vote to negative (assuming n is odd). Assuming all these are equally likely, and using Stirling's approximation, we get power equal to $.8/\sqrt{n}$. This same index is given by Bartholemew and Bassett (1971:120).

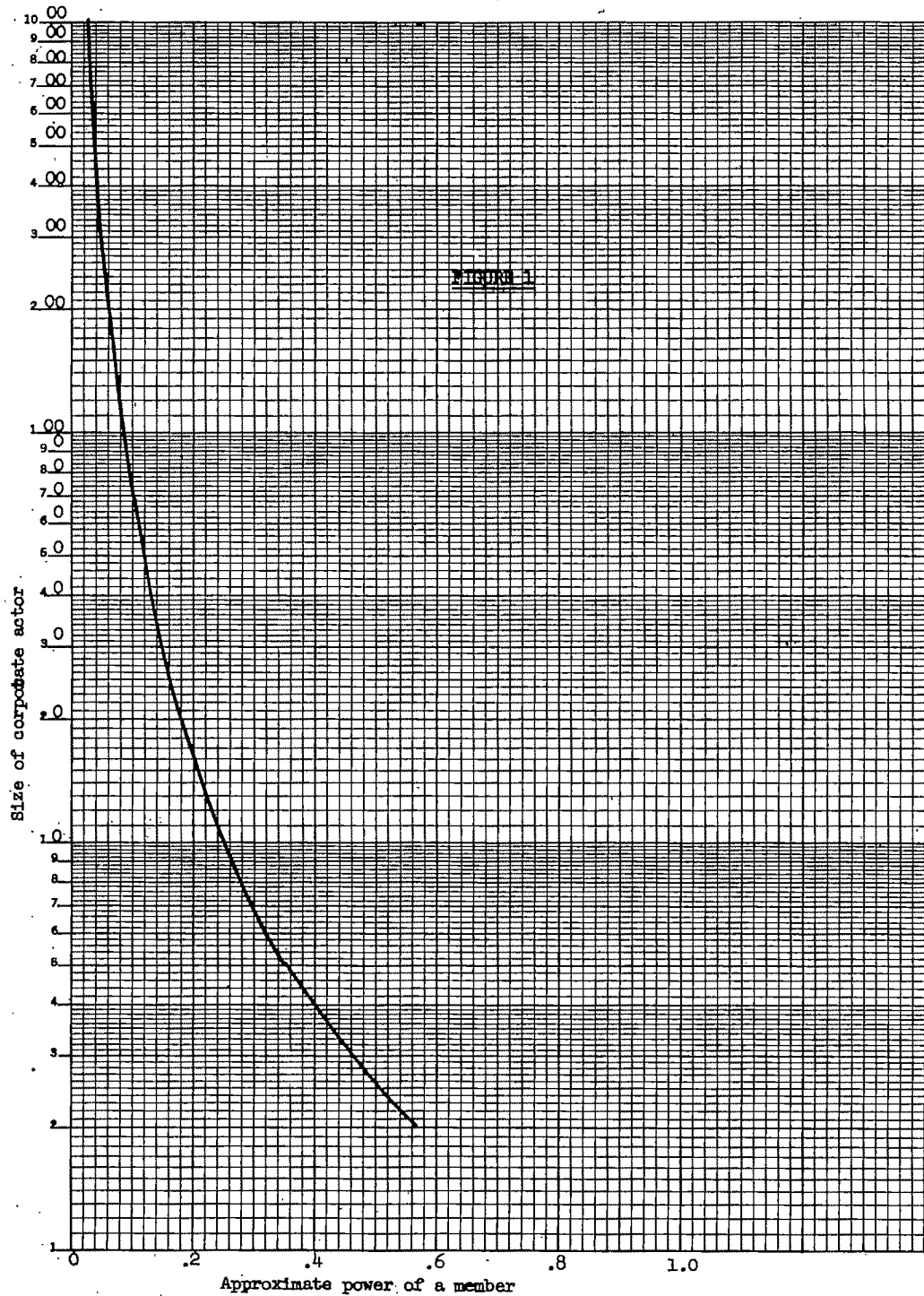
$1 - .8/\sqrt{n}$. Thus if I_n is the power of an individual in a corporate actor of size n , then

$$I_n = .8/\sqrt{n} \quad (1)$$

The power of the individual member as it

varies for corporate actors from size two to one thousand is given in Figure 1.

It is evident, then, that an individual's power declines sharply as he joins with



others to form corporate actors, with those actors carrying out actions for him. The power of the corporate actor relative to its environment may be sufficiently great to better realize his interests (i.e., benefit rights) than he could alone. Indeed, this is why a rational individual will join a collectivity and yield to it his individual control over events.

The Power of an Individual in a World with Corporate Actors

All natural persons in society have given over some portion of their control over events to corporate actors of various sizes. When a man marries, then, if we assume an equal share of control in family decisions, he has yielded half his control over certain actions to the corporate actor, the married couple. When he forms a close bond with another person and engages in activities jointly, he has given up half his control to the corporate actor which consists of the two. When he takes up residence in a state, and subjects himself to its laws, he yields control over a number of activities to the state, and retains only a tiny fraction of his power to affect the state's actions.

If we add up all the events in which he has interest, and weight them by the interest each holds for him, we can assess the amount of control he retains over his actions, if we know the size of the corporate actors to which he has yielded control. (We are assuming, of course, that he holds one vote under a majority decision rule. The principle holds under other conditions as well, though the calculations are less simple.) If the corporate actors in which he has invested resources are indexed by j , then the power that he retains over resources invested in corporate actor j can be denoted by p_j . This power was shown in the preceding section to be approximately $.8/\sqrt{n}$, when group j is of size n . His interests in each event, resource, or activity over which corporate actor j has control may be denoted by x_{ij} , for event i controlled by corporate actor j , with the sum of x_{ij} over all events equal to 1. This expresses his relative interest in different events, so that if he had total control over each event, he would realize 1.0 of his interests. Then the measure of his individual

control over events is simply the sum of the product $x_{ij}p_j$ over all events i in corporate actor j and over all corporate actors j :

$$I = \sum_{j=1}^m \sum_{i=1}^{n_j} x_{ij}p_j, \quad (2)$$

where I is his power ($0 \leq I \leq 1$), n_j is the number of events controlled by corporate group j , and m is the number of corporate actors in which he has made an investment. Equation (2) is a general equation when his power in group j is unspecified; but if his power is specified as indicated above, the equation for his power can be written in more specific form. His power in a group of size k , under the assumptions given earlier, is simply $.8/\sqrt{k}$. Thus if a certain fraction, x_k , of all his interests are located in activities controlled by corporate actors of size k (that is, x_{ij} is summed over all activities and all corporate actors j of size k), then his power over activities of interest to him is approximately given by:

$$I \approx \sum_{k=1}^{\infty} \frac{.8x_k}{\sqrt{k}} \quad (3)$$

This gives a measure of power as a function of his distribution of interests over activities controlled by different size corporate actors. The power decreases if the corporate actor which controls certain activities grows in size. It decreases also if his interests shift onto activities controlled by larger corporate actors.

Figure 1 shows the way his power decreases with the increase in size of corporate actors. The decrease in control depends greatly on the region of the curve from which a person starts. If his control is initially vested in himself or in very small corporate actors, then an increase in the size of actors has great impact. If large corporate actors already control activities that interest him, even large increases will reduce his power very little.

The power of corporate actors in this world of two kinds of persons can be obtained from equation (3), by summing over all persons, and subtracting from r , where r is the total number of persons. For if there were no corporate actors, if each natural person fully controlled those activities of interest to him, then each would have a power of 1. Thus, what we want to

know is how that total power of r is divided between corporate actors and real persons. To make the measure independent of population size, we can normalize back to 1, dividing through by r . Note that I deliberately treat corporate actors and real persons asymmetrically, paying attention only to the interests of real persons. The relevant equation for the power of corporate actors, in a population of n natural persons, with m corporate actors, of size n_j (where $j = 1, \dots, m$), is:

$$C_c = 1 - \frac{1}{r} \sum_{h=1}^r \sum_{j=1}^m \sum_{i=1}^{n_j} x_{ijh} p_{jh}, \quad (4)$$

or specifying as in equation (3)

$$C_c = 1 - \frac{1}{r} \sum_{h=1}^r \sum_{k=1}^{\infty} \frac{.8x_{kh}}{\sqrt{k}}. \quad (5)$$

The amount of power held in society by corporate actors of a given size k is given by a subset of the terms from equation (5):

$$\begin{aligned} C_k &= 1 - \frac{1}{r} \sum_{h=1}^r \frac{.8x_{kh}}{\sqrt{k}} \\ &= 1 - \frac{.8}{\sqrt{k}} \frac{1}{r} \sum_{h=1}^r x_{kh}, \quad (6) \end{aligned}$$

or if $\bar{x}_k (= \frac{1}{r} \sum_{h=1}^r x_{kh})$ is defined as the

average over all real persons of the importance of activities in which control is held by corporate actors of size k , C_k becomes:

$$C_k = 1 - \frac{.8\bar{x}_k}{\sqrt{k}}. \quad (7)$$

If \bar{x}_k is held constant, we see that C_k increases rapidly as size increases. Thus if the size of a collectivity increases, and it continues to deal with the same set of activities in which real persons have unchanged interest, corporate power goes up at the expense of individual power.

This still does not get very far toward estimates of power held by corporate actors and real persons in a given society. Such a measure may not be important because cor-

porate actors are here defined broadly to include all actors created by a collective decision that binds two or more parties. Thus it includes a person's actions carried out with only one other person. What may be more important is the proportion of societal control held by a particular corporate actor, such as the state.

I will not provide such estimates here, but will turn instead to the question of what long-term trends and short-term fluctuations appear to be occurring in the values of C_k .

Mass Communication and the Drift of Interests Toward Activities Controlled by Larger Corporate Actors

Most interests of most persons are controlled in their immediate vicinity, by themselves and others near them. Most of our interests concern matters immediately at hand, satisfying our physical wants. Thus it should be clear that in any discussion of changes in control, a large portion of interests are controlled by the person himself or jointly with one other.

However, apart from these activities, social processes have shifted interests toward activities controlled by larger corporate actors. The major process of this sort is mass communications, which has vastly extended the social horizons of most persons. Persons focus attention on national and international events through the mass media that was once focused on state and local events. Northerners in America focused attention on racial strife in the South through the media. Yet the objective level of strife in itself would not have generated as much interest in the absence of modern media. Television may give Swedish youth a strong interest in the Vietnam War (which was controlled by corporate actors they have no control over) though their parents as youths directed their attention to matters closer at hand. In short, communication processes focus attention on events controlled by ever larger corporate actors. These are supplemented by other changes that widen horizons, such as travel and the increased leisure of most persons that allows interests to expand. But the principal process appears to be communication through the mass media. There is little evidence to suggest that the

trend will be reversed. Added to this trend is a second which will be discussed below.

The Increasing Size of Corporate Actors

The preceding process tends to weight heavily more distant activities and events controlled by larger corporate actors. But in addition, other processes tend to place a given activity under the control of large corporate actors. Such changes are changes in social organization, not merely changes in the awareness of distant activities. They take the form of amalgamations, concentrations of ownership, consolidations of schools, the increase in city sizes, the increase in size of firms. Again, the trend appears long-term, though less irreversible than the trend in mass communications.

Both changes, that is, the development of interests in new activities controlled by large actors, and the increase in size of actors controlling a given activity, decrease the individual's power. This appears to be due to general secular trends that show no signs of abating.

Besides such trends, there are short-term fluctuations as well. The above measures indicate that whenever important public events occur, individuals experience a loss of power qua individuals. A recent pair of studies suggests that this loss of power is subjectively experienced. Julian Rotter (1971:37) developed a test of "internal control vs. external control," in which some persons tested express a sense of control over their own future, while others see themselves as subject to the whims and circumstances of their environment. Rotter administered this test to a large sample of college students in 1962, and again in 1971. In 1962 there was much less attention on public affairs (before large-scale civil rights activities, before the Vietnam War, before environmental problems were seen as very important) than in 1971. The measures above would predict, from this fact alone, that individuals would experience a lower sense of internal control in 1971 than in 1962. And this is what Rotter found. In 1971, the average score was 11 on a scale from 1 (fully "internal") to 29 (fully "external"). Rotter reports that in 1962 about 80% of college students had scores closer to the "internal" extreme than this.

This of course is only one piece of suggestive evidence that a subjective sense of control is affected by fluctuations in the size of the corporate actor controlling events important to persons. It is particularly suspect because these events appear to be poorly controlled by the corporate actors. What such tests would show during a period characterized by important public events in conjunction with a strong sense of corporate unity, such as occurred in the United States during World War II or Israel during and after the 1967 war, is not known. But more of this later. Now I will ask what are the psychological consequences of such matters.

Psychic Consequences of Lack of Control, and Strategies for Regaining Control

Much evidence argues that persons have a fundamental psychological need to control events that affect them, and that loss of such control creates psychological distress. Perhaps the most compelling evidence relates to physical pain. Numerous experiments show that subjectively experienced pain is not merely an automatic response of the nervous system, but derives from the loss of control over one's body that a physical trauma implies. When there is no sense of having lost control, pain does not develop (Buytendijk, 1962). The kinds of psychological distress Alvin Toffler points to in *Future Shock* (1970) do not result from the speed of change as Toffler supposes, but from inability to predict and control that change. Nevertheless, the general secular trend toward greater interest in events controlled by corporate actors, which leads to ever-decreasing individual control, suggests a parallel trend toward increasing psychological distress due to reduced control over events.

What strategies can persons use to cope with such psychological distress? Examination of the terms on the right of equation (1) suggests what strategies a person may use to increase his power. One is to change his set of interests x_{ij} , reducing all those for which his control over corporate actor j , p_j , is low, and increasing those for which p_j is high. In general, this will mean reducing interest in public affairs, because for them p_j is inexorably low. In effect, it implies all that is meant by the psychological

term, withdrawal: a withdrawal from interest in public affairs, from collective action, and a withdrawal of resources from large corporate actors. It means a reinvestment of resources in activities one can directly control.

A second strategy is to attempt to increase control over corporate actors, that is, to increase p_j for the corporate actor j controlling those activities and events for which one's interests are great (i.e., for which the x_{ij} 's are large). Although the expected power of an individual over the actions of a corporate actor of size k is $.8/\sqrt{k}$, this may be increased through attempts at gaining control. However, since gain in control of an event by one person means loss by another, this strategy will not generally be successful. Success for some must be offset by failure for others.

There is a third strategy suggested by the articulated structure that some corporate bodies take. This can be described as breaking a single stage decision process for a corporate actor down into several stages. Political systems do this through representation and other corporate bodies through committee structures. Such substructures are introduced for purposes not relevant to the present discussion. What we wish to know, however, is whether they increase a person's power over events. Intuitively, the answer would appear to be no, since the same number of persons have ultimate control over the same events through the corporate actor. Nevertheless, the system is not one in which control of all individuals adds to 1.0, but one in which the control of an individual plus the control of the corporate actor add to 1.0. Might not the multiple-stage process affect the relative control of the individual and the corporate actor over the action?

The answer is yes, it does, but in the direction of decreasing the individual's control, and increasing the corporate actor's. The approximation $I = .8/\sqrt{n}$ indicates that this is so. For suppose a decision process is divided into two stages, with m subgroups of equal size, n/m , in the first stage, and a group of m representatives in the second. Then the individual's control in the first is approximately $.8/\sqrt{n/m}$, and in the second the control of each representative is $.8/\sqrt{m}$.

His control is then his control in the first stage in determining the position of his representative, times his representative's control in the second stage. This is:

$$I_2 = \frac{.8}{\sqrt{\frac{n}{m}}} \cdot \frac{.8}{\sqrt{m}} = \frac{.64}{\sqrt{n}}, \quad (8)$$

which is less than his control in the single-stage decision. Using exact calculations with the binomial distribution does not change this result.

Coalition Formation

Another strategy remains: forming a coalition, that is, a corporate person within the larger corporate person. This coalition will then have greater power in the corporate body than one would himself. If it is large enough, half the size of the corporate actor, plus one, then with a majority decision rule it can fully control the corporate person.

This strategy appears to be quite useful. It has one catch: as the coalition becomes larger and more powerful within the corporate actor, an individual's power within it declines. The other coalition members are not passive partners, but themselves expect to have a voice in the coalition's action. Thus the same kind of problem has been created within the smaller corporate actor, the coalition, that exists with the larger corporate actor. As the coalition gains power by increasing in size the individual loses power within it.

Yet the widespread use of this strategy in social life, constructing coalitions to fight internal battles in corporate bodies, suggests that the strategy may increase one's power, even though it is limited by the above considerations. A moment's reflection indicates that this must be true. If one can form a coalition consisting of a bare majority, and then exercise his vote within that coalition, his power is that of a person in a corporate body only half as large. In reference to Figure 1, if he is trying to control a corporate actor of size one hundred with his one vote, his power to do so is about .08. But if he forms a coalition of fifty-one members, his power is that of a person in a corporate

actor of fifty-one persons, about .11. Thus forming a bare-majority coalition is obviously beneficial under the above assumptions (majority decision rule and independent voting with probability of 1/2), together with the added assumption that the coalition's action is also determined by a majority vote within it.

This then raises the question of whether this is the best sized coalition one could form to increase one's power. It turns out not to be. In fact, a much smaller coalition is better. It has less control over the corporate actor, but he has more control over it. A person wants a coalition of such size that the product of the probability of his controlling the coalition and the probability of the coalition's controlling the corporate actor is maximum. A general rule of thumb can be used to express the size of a smaller body that gives an individual the maximum power for controlling a larger corporate body which contains it. It is this: If the larger corporate body is of size n , and the coalition size is n_1 , then the optimum coalition size, n_1^* is approximately

$$n_1^* \approx 1.4 \sqrt{n}, \quad (9)$$

for values of n at least up to 10,000. The calculations are carried out in the appendix, and the value of n_1 which gives maximum control is shown in Figure 2, along with control in the absence of a coalition. The formulas for control through a coalition are also given in the appendix, as equation (5) and equation (6).

For values of n_1 that are not too large (not larger than about $.5\sqrt{n}$), then it is possible to estimate the power of the coalition vis-a-vis the corporate body. This is done by use of equation (1), multiplying the right hand side by n_1 : $I = .8n_1/\sqrt{n}$. This allows an estimate of the control exercised by an individual through a coalition. Within the coalition itself, equation (1) applies directly to the individual's control over the coalition. Thus the approximation for the overall measure of control through a coalition is

$$I_{n_1} \approx \frac{.8n_1}{\sqrt{n}} \cdot \frac{.8}{\sqrt{n_1}} \approx .64 \sqrt{\frac{n_1}{n}} \quad (\text{for } n_1 \leq 0.5 \sqrt{n}) \quad (10)$$

For values of n_1 larger than $0.5\sqrt{n}$, equation (1) will give too large an estimate for an individual's control through a coalition. Since the optimal value of n_1 is about $1.4\sqrt{n}$, this means that equation (10) can only be used to estimate I_{n_1} for values of n_1 considerably below the optimum. Even so, comparison of equation (10) with equation (1) shows that I_{n_1} is about $.8\sqrt{n_1}$ times the size of I when the coalition is small enough so that equation (10) holds. More generally for optimum values, n_1^* , Figure 2 shows the degree to which control through a coalition is greater than that without a coalition.

If the voters have some degree of interdependence, so that they do not all vote independently as assumed by the binomial distribution, then the coalition size which gives maximum control is larger. This degree of interdependence can be measured by the variance of the corporate actors' actions, taken over a sequence of votes. If this variance is σ^2 , then the degree of interdependence, q , is $\frac{n\sigma^2 - 0.25}{n}$.⁶ When there is some interdependence, the optimum value of n_1 is given as

$$n_1^* \approx 1.4 \sqrt{\frac{n}{1-q}}, \quad (11)$$

as shown in the appendix. This will always give a value of n_1^* greater than the value of n_1^* from equation (9), so long as there is some interdependence, i.e., $q > 0$.⁷

In sum, the fruits of this strategy of coalition formation are considerable, for they show that an individual can greatly increase his power vis-a-vis a corporate actor, through joining a set of others. The optimum size of this set is below the size that would completely control the corporate actor, a somewhat surprising result. However, this optimum size must increase when other members of the corporate actor are interdependent.

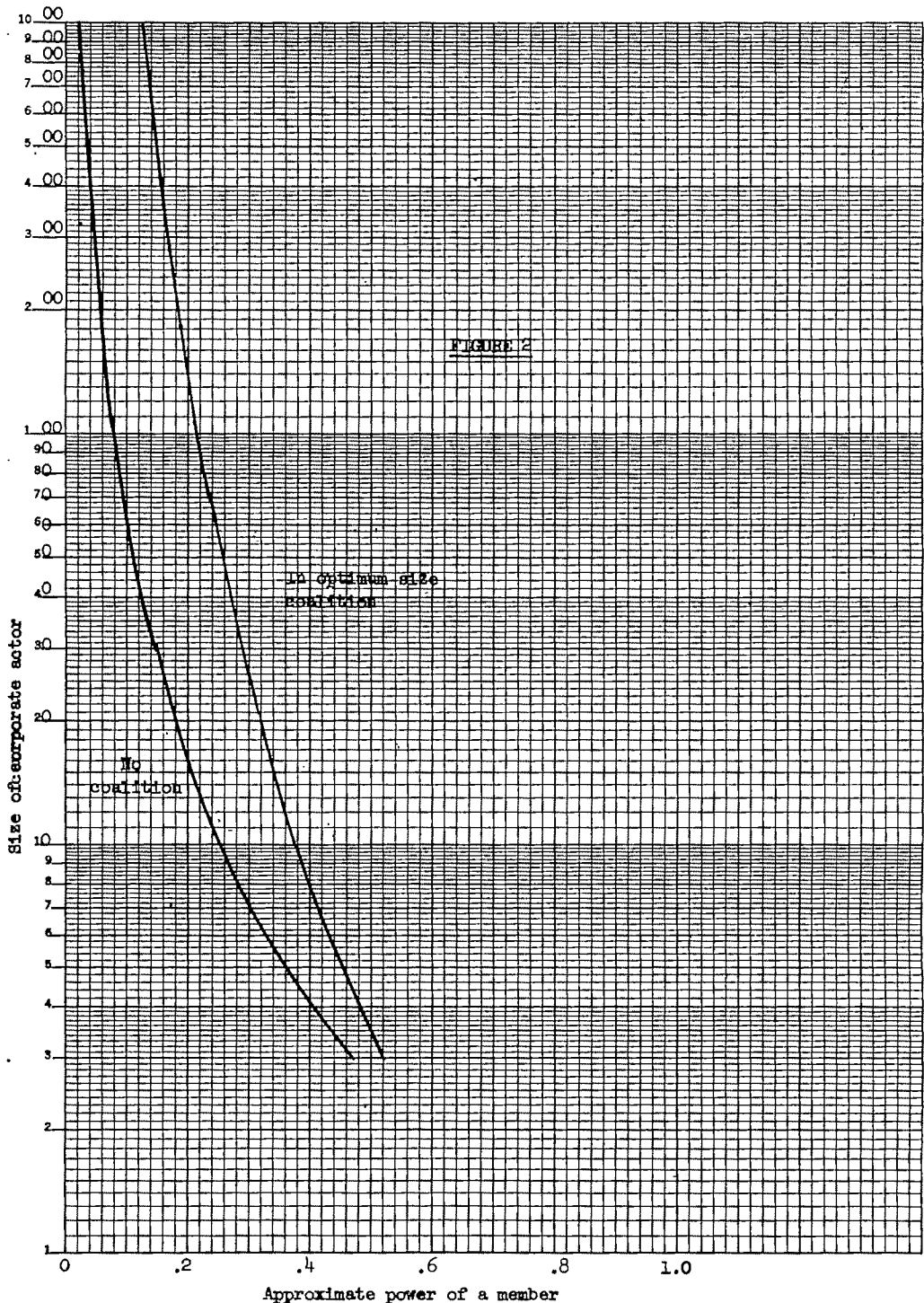
Yet when all is said and done, the individual's power over the resources he places

⁶ This may be found, from calculations in the appendix, by the fact that q is a function of n and n' , and n' is a function of σ^2 .

⁷ The calculations for finding control of the corporate actor are given in the final footnote of the appendix.

under the control of a large corporate actor remains very small. Thus it is fair to say that coalition formation is a beneficial strategy for controlling a corporate actor, but is not a satisfactory means for the individual to recover power lost in the corporate world.

One consistent, recurrent set of research results seems, on the surface, to contradict my general thesis that yielding usage rights over one's resources to corporate actors reduces his objective control over events, and his subjective sense of control. This is re-



search showing that the wider the range of group membership of a person, the more he feels in control of events that concern him, and the more objective control he seems to have through those intermediate corporate actors (see for example, Kornhauser, 1959; Arendt, 1951; Rotter, 1966). But the research needed to determine whether this research contradicts or confirms my own remains to be done. If a person has a given portion of his interests in public affairs at various levels, and has invested resources in an intermediate corporate actor, such as a political or civic group, that group acts as a coalition to exert power over the larger corporate actor. Another person with the *same fraction* of his interests in public affairs at each level, who has not yielded usage rights of his resources to an intermediate group, lacks the coalition power of that group. As this section shows, that power is not negligible.

Only the person with a smaller fraction of interests in public affairs or a smaller fraction beyond the immediate locality [cf., equation (2)], should have a greater sense of control than one with interests broadly extended. Even here, among those with interests confined to local affairs, a person who has invested resources in a corporate body should experience the power benefits of coalition, and show a greater sense of control than one without affiliations. In short, to test this thesis, it is necessary to control on interests, comparing only persons whose interests span the same set of events. For persons with different interests but similar group affiliations, the theory predicts that the person with narrow, local interests will show the greater sense of control.

Withdrawal of Investment

One option that has not been discussed is the withdrawal of investment. Other authors have pointed out that fundamentally a member can exercise power toward a corporate actor in two ways: control and withdrawal. Hirschman (1970) has captured these two modes in the title of a book that discusses them, *Exit, Voice and Loyalty*. Withdrawal or the threat of it can be an important means of recapturing power from a corporate actor or limiting usage rights by the constant threat of withdrawal. However,

this threat is powerful only insofar as such a withdrawal will not result in a person's loss of benefits.

In a highly competitive market structure, such as the capital market, corporate actors are forced to compete for funds; thus an investor can withdraw, invest in another corporate actor, and receive nearly the same benefits. Though he does not control the day-to-day actions of the corporation, he can indirectly control it through the competitiveness of the capital market, which forces the corporate actor to attend to his interests.

If the economic and social costs of moving residence are low so that communities must compete for residents, withdrawing one's resources from a city is a serious enough threat to constitute effective power. If the cost of changing schools is low (does not, for example, require change of residence), then the threat of doing so constitutes effective power. If withdrawing from a union does not mean reduced income or working conditions, then the union member has effective power vis a vis the union, limiting the usage rights of the corporate actor.

But as these last three examples suggest, in many cases, a market for resources does not exist; and it would be costly for someone to withdraw his resources. Often a residential move is costly (and in some countries, people must have a permit to move to a city), a change of schools requires moving residence or paying for private school, quitting a union often means losing a job. In such situations, the right of withdrawal is not an important means of control. When, in fact, such withdrawal is costly, the corporate actor can divert the benefits from usage rights to itself, bringing about its own growth without benefit to members.⁸

⁸ Sometimes the usage rights, or power held by the corporate actor, are at the disposal of one man, as an agent, with few constraints. In other cases, that power is held by some set of persons who act as checks on one another. In the former case, the agent (the "director," "manager," "president," etc.) can if he desires use the power to divert the benefits to himself, or to his office. The examples of this in trade unions, business firms, and political office are numerous enough to indicate that the former conditions obtain in various types of corporate actors.

Thus one can gain power by investing in corporate actors which operate in highly competitive private markets. This is, of course, not a general solution, because the competitiveness of markets differs greatly for different types of resources. Nevertheless, countries differ strikingly in the competitiveness or pluralism of intermediate corporate actors. One way to describe the flights from some Socialist countries, where intermediate corporate actors far more commonly monopolize certain areas of action, is to say that those leaving are attempting to gain power through entering a social system in which the cost of withdrawing resources from corporate actors is not so great.⁹

Why do Individuals Give Up Control?

As my exposition suggests, individuals in society, natural persons, show a general and continual loss of power to corporate actors. These juristic persons include the state, which has special privileges before the law. What remains to ask is why individuals have yielded this power—especially when the loss of control appears to have negative psychological consequences.

Perhaps we can answer this question by looking back at the thirteenth through the seventeenth centuries, when the feudal structure was breaking up, when natural persons were coming to have rights, that is, powers, before the law, and when the early corporations were conceived in the form of boroughs and churches. At that time, the corporation and the trust were outgrowths of the newly-won "natural rights" of persons, extending their powers vis-a-vis the power of the state or the king. In effect, the law's recognition of boroughs

as legal persons, the extension of this recognition to non-landed corporations, and the endowing of trusts with the powers of persons, legitimated the idea that an individual could extend his powers through combination. The increase in power by coalition-formation was brought about through this recognition. The nineteenth and twentieth centuries have witnessed a vast extension of these possibilities. Today we find ourselves confronted by a world of corporate actors to whom much of our sovereignty has gone.

Yet we continue to yield control. I think two central processes bring this about. One is indicated by the motives suggested above: the desire to augment our power vis-a-vis even larger corporate bodies through creating others, a process which is called the creation of "countervailing power" in modern discourse. Such a process leads to an escalation that has its natural ending only when all our sovereignty lies in the hands of two competing corporate actors, each holding half the world's power. Obviously, such an ending is not pleasant to contemplate. Other conceivable structural configurations could develop, though I will reserve their consideration for later work.¹⁰

The second motive is different. Men sometimes yield their usage rights to a corporate actor in hopes of greater gain, as when individuals invest economic resources in a company (in exchange for negligible control over the company's resources). This motive is even clearer in bond purchases and loans, where the investor yields full control of his money for a greater return than he might reap by using the money directly.

When a person joins a guild, union, or professional association, he yields control over certain actions (as well as monetary fees) expecting to gain thereby. When a man yields sovereignty in marrying, he anticipates greater satisfaction than his own independent use of sovereignty would bring.

More subtle examples are those corporate actors which persons invest in and sacrifice for with no tangible returns. These range

An example of how such concentration of power in the hands of one person is shown for the Pressman's Union as described by Lipset, Trow, and Coleman (1956): the president of the union had sufficient power in his office to gain a change in the rules of the union that sharply limited the power of the large locals (his principal source of opposition) relative to the small locals (which he could easily control).

⁹ An interesting incident occurred in relation to this paper. An earlier draft (without the above paragraph) was submitted for publication to a work edited by American, Soviet, and other editors. The Soviet editors felt it necessary to reject the paper because of the sensitivity of the topic in their country.

¹⁰ One is suggested by possibilities outlined in Coleman (1970b), a work which envisions temporary transfer of portions of sovereignty by persons, who have a continual option of withdrawing it and placing it elsewhere.

from the sports team in an elementary school, to the nation for which men may give their lives. Such investments pay intangible but directly experienced rewards in the sense of pleasure a person experiences in being on a winning team or the sense of collective well-being he experiences when his nation does well. These gratifications are obviously greater than the also evident distress a person experiences when such a corporate actor fails. For if this were not so, the existence of team sports, in which every winner must create its corresponding loser, could not be explained.

The psychological processes which lead to gratification or distress through a corporate actor's success or failure are beyond the scope of our examination. It is sufficient to recognize their existence and to make a few observations about them. First of all, it may be useful, for developing an appropriate conception of society, to conceive of the very psychological self of a person as being partitioned among various corporate actors, with a portion of the self remaining with him. Then, just as persons have yielded control of economic resources to corporate actors, they have yielded control of "inalienable" personal resources as well. In doing so, they can expect a satisfaction greater than that they would experience without the psychic investments, but also the possibility of dissatisfaction or distress. Alleviating or reversing this distress lies beyond them, because they have yielded personal control. It is this, I suspect, which creates a special degree of frustration—though again the question is a psychologically complex one and lies outside my competence. We can observe, however, that in defense the self does move to regain control by withdrawing the psychic investment. As Adam Smith (1759) said one time in a treatise on sympathy for which he was better-known in his lifetime than for *The Wealth of Nations*, men find it much easier to have sympathy for a prince than a beggar. Or in modern idiom, "Everyone loves a winner."

There is one corporate actor, well-known through history, in which such psychic investments are carried to their limit. This is the communal group, in which the members invest their total selves. The literature

on such groups shows the continual battle of the corporate self against the individuals' recurrent tendencies to recover a portion of their selves. Processes of self-incrimination, group criticism, and confession are the most common that the corporate body uses to combat this recurrent danger.¹¹ The use of these same processes in some totalitarian states (U.S.S.R., China) indicates that such attempts by corporate actors to control the self are not confined to small communities. We are nearly returned full cycle to the middle ages which lacked not only the concept of juristic persons with rights before the law, but also the concept of the natural person, with inalienable rights.¹²

Half a dozen independent couples were living on a hillside not far out of town in independently established households with no group organization, but with informal networks. They found they had to institute more formal organization and rules, however, in response to the problem of what to do with their dogs! Each of these couples had at least one dog, and each was engaged in raising their own garden vegetables. The dogs, as dogs are wont to do, proceeded to tear up the gardens. This gave rise to the first formally called meeting of the entire group, and further organizational efforts then proceeded to tackle other problems in common.

The study of these psychological investments and withdrawals constitutes an important direction of work in the search for means by which natural persons can come to live successfully in a world populated by corporate actors. We cannot live without these artificial persons in our midst. But we have yet to discover how we can live with them.

REFERENCES

- Arendt, Hannah
1951 *The Origins of Totalitarianism*. New York: Harcourt Brace.
Bartholemew, D. J. and E. E. Bassett
1971 *Let's Look at the Figures*. Baltimore: Penguin Books.

¹¹ See Benjamin D. Zablocki (1967) for a perceptive examination of these processes among the Bruderhof, a religious community in contemporary U.S. See also Connor (1972) for a study of these processes in the Soviet Union.

¹² In commenting on a draft of this paper, James Short gave a contemporary example of the particular form of withdrawal described in the above paragraph and the subsequent reconstruction of a corporate actor:

- Berle, A. A. and G. C. Means
1940 *The Modern Corporation and Private Property*. New York: Macmillan.
- Brams, Stephen J.
1968 "Measuring the concentration of power in political systems." *American Political Science Review*, Vol. LXIII:2 (June):461-75.
- Buytendijk, F. J. J.
1962 *Pain, Its Modes and Functions*. Chicago: University of Chicago Press.
- Coleman, James S.
1970a "The benefits of coalition." *Public Choice* 8 (Spring):45-61.
1970b "Political Money." *American Political Science Review* 64:4 (December):1074-87.
1971 "Control of collectivities and the power of a collectivity to act." In B. Lieberman (ed.), *Social Choice*. London: Gordon Breach.
1973a *The Mathematics of Collective Social Action*. Chicago: Aldine-Atherton.
1973b *Power and the Structure of Society*. New York: W. W. Norton.
- Connor, Walter D.
1972 "The manufacture of deviance: the case of the Soviet purge, 1936-38." *American Sociological Review* (August):403-13.
- Hirschman, A. O.
1970 *Exit, Voice and Loyalty: Responses to Decline in Firms, Organizations, and States*. Cambridge: Harvard University Press.
- Kornhauser, William
1959 *The Politics of Mass Society*. Glencoe: Free Press.
- Lipset, S. M., M. A. Trow and J. S. Coleman
1956 *Union Democracy*. Glencoe: Free Press.
- Pollock, Sir Frederick and Frederic W. Maitland
1898 *The History of English Law*, second ed., Vol. 1, Cambridge, England: Cambridge University Press.
- Rae, Douglas and M. Taylor
1971 "Decision rules and policy outcomes." *British Journal of Political Science* 1:71-90.
- Rotter, Julian B.
1966 "Generalized expectancies for internal versus external control of reinforcement." *Psychological Monographs* 80:1.
1971 "External control and internal control." *Psychology Today* 5 (June):37-59.
- Sen, A. K.
1970 *Collective Choice and Social Welfare*. San Francisco: Holden-Day.
- Shapley, L. S. and M. S. Shubik
1954 "A method for evaluating the distribution of power in a committee system." *American Political Science Review* 48:787-92.
- Smith, Adam
1759 *The Theory of Moral Sentiments*. Edinburgh: Printed for A. Millar and A. Kincaid and J. Bell.
- Toffler, Alvin
1970 *Future Shock*. New York: Random House.
- Zablocki, Benjamin D.
1971 *The Joyful Community*. Baltimore: Penguin Books.

APPENDIX

CALCULATION OF POWER TO CONTROL A CORPORATE ACTOR THROUGH A COALITION

The power to control a corporate actor consists of the power to block positive action and the power to convert negative action to positive action. When this power is exerted through a coalition, it is necessary to consider both the power of the coalition to control the corporate actor, and the power of the individual to control the coalition.

The probability that the coalition cannot block a positive vote is of course zero if the coalition is greater than half the size of the total corporate actor. If it is not that large, the probability that it will not block the action is the probability that the number of positive votes, i , among the other $n - n_1$ voters, is equal to or greater than $(n+1)/2$, that is, a majority. If votes are cast among the other $n - n_1$ voters with probability $1/2$ and independently, then this is given by the binomial probability,

$$j = \frac{n - n_1}{n + 1} \sum_{j = \frac{n + 1}{2}}^{n - n_1} \frac{(n - n_1)!}{j!(n - n_1 - j)!} \left(\frac{1}{2}\right)^{n - n_1}$$

The binomial variance of i is in this case $0.25(n - n_1)$, so that the normal approximation to this quantity is the probability that a standardized random variable z (mean 0 and variance 1) will exceed the mean by an amount equivalent to $i \geq (n+1)/2$. The standardized random variable is

$$z = \frac{i - \frac{n - n_1 + 1}{2}}{\sqrt{0.25(n - n_1)}}$$

$$\text{Thus when } i = \frac{n + 1}{2}, z = \frac{n_1}{\sqrt{n - n_1}}$$

The normal approximation to the probability that the coalition will not block a positive vote is thus the probability that z in a standardized normal distribution is greater than $n_1/\sqrt{n - n_1}$. Thus the normal approximation to the probability that the coalition can block a positive vote is the probability that z is greater than zero and less than $n_1/\sqrt{n - n_1}$. If z is less than zero, the vote will be negative without the coalition's blocking it; if it is greater than $n_1/\sqrt{n - n_1}$, it will be positive in spite of the coalition's nega-

tive vote.¹ The probability of blocking a positive vote is $\Pr\{0 < z < n_1/\sqrt{n-n_1}\}$.

The probability that the coalition will be able to convert a negative vote into a positive one is the probability that less than half of the non-coalition members favor the action [the probability that $i < (n-n_1+1)/2$, or in the normal approximation, the probability that $z < 0$], but that at least $(n+1)/2 - n_1$ of them favor it (i.e., the probability that $i \geq (n+1)/2 - n_1$). The binomial probability is the sum

$$\sum_{j=(n+1)/2-n_1}^{(n-n_1-1)/2} \frac{(n-n_1)!}{j!(n-n_1-j)!} \left(\frac{1}{2}\right)^{n-n_1}$$

The standardized normal approximation to the lower limit is

$$z = \frac{\frac{n+1}{2} - n_1 - \frac{n-n_1+1}{2}}{\sqrt{0.25(n-n_1)}} = -n_1/\sqrt{n-n_1}$$

Thus the normal approximation to the probability that the coalition will control the collectivity through converting a negative vote to a positive one is $\Pr\{-n_1/\sqrt{n-n_1} < z < 0\}$. Because the standardized normal distribution is symmetric around $z=0$ (or the binomial with probabilities of $1/2$ is symmetric around the middle outcome), this is the same probability as $\Pr\{0 < z < n_1/\sqrt{n-n_1}\}$. Thus the total probability of the coalition's controlling the collectivity, which I will call p_1 , is

$$p_1 \approx 2\Pr\{0 < z < n_1/\sqrt{n-n_1}\}, \quad (1)$$

where n is the size of the collectivity, and n_1 is the size of the coalition. For values of n and n_1 that are small, the exact binomial probability should be used instead:

$$p_1 = 2 \sum_{j=(n-n_1+1)/2}^{(n-n_1-1)/2} \frac{(n-n_1)!}{j!(n-n_1-j)!} \left(\frac{1}{2}\right)^{n-n_1} \quad (2)$$

¹ The probability of preventing a positive action on the part of the collectivity, given that there would be positive action with the coalition's positive vote, is $\Pr\{0 < z < n_1/\sqrt{n-n_1}\} | \Pr\{z > 0\}$; and since in a normal distribution $\Pr\{z > 0\} = 1/2$, this is $2\Pr\{0 < z < n_1/\sqrt{n-n_1}\}$. For the binomial probability, it is

$$i = \frac{\frac{n-1}{2}}{\sum_{j=(n-n_1+1)/2}^{(n-n_1-1)/2}} \frac{(n-n_1)!}{j!(n-n_1-j)!} \left(\frac{1}{2}\right)^{n-n_1}$$

$$i = \frac{\frac{n-n_1}{2}}{\sum_{j=(n-n_1+1)/2}^{(n-n_1-1)/2}} \frac{(n-n_1)!}{j!(n-n_1-j)!} \left(\frac{1}{2}\right)^{n-n_1}$$

The denominator equals $1/2$, so that the required probability of preventing action is twice the numerator.

The individual's probability of controlling the coalition can be calculated in exactly the same way, where the size of the "coalition" within the coalition is 1, and the size of the remaining portion of the coalition is n_1-1 . Thus if we call this probability p_2 , it is

$$p_2 \approx 2\Pr\{0 < z < 1/\sqrt{n_1-1}\}, \quad (3)$$

where he is of size 1 and the coalition including him is of size n_1 .² The exact binomial probability is only one term in the binomial series, since he will convert the coalition from positive to negative only if it has a bare majority.³

$$p_2 = \frac{(n_1-1)!}{\left(\frac{n_1}{2}\right)! \left(\frac{n_1-1}{2}\right)!} \left(\frac{1}{2}\right)^{n_1-1} \quad (4)$$

Thus his overall power to control the corporate actor through forming a coalition is equal to the product $p_1 p_2$. If p is that power, then the normal approximation gives

$$p \approx 4\Pr\{0 < z < n_1/\sqrt{n-n_1}\} \Pr\{0 < z < 1/\sqrt{n_1-1}\}. \quad (5)$$

The exact binomial probability is given by the product of the right hand sides of eq. (2) and eq. (4).

$$p = 2 \frac{(n_1-1)!}{\left(\frac{n_1}{2}\right)! \left(\frac{n_1-1}{2}\right)!} \left(\frac{1}{2}\right)^{n_1-1} \sum_{j=(n-n_1+1)/2}^{(n-n_1-1)/2} \frac{(n-n_1)!}{j!(n-n_1-j)!} \left(\frac{1}{2}\right)^{n-n_1} \quad (6)$$

To find the size of coalition that maximizes this power, it is necessary to find the value of n_1 that maximizes $p_1 p_2$ as a function of n . (The

² This probability can be approximated, as indicated in eq. (1) of the text, by $.8/\sqrt{n_1}$. Similarly, the probability of the coalition controlling the corporate actor, p_1 , can be approximated by $.8n_1/\sqrt{n}$, an approximation that holds well when $n_1 < .5\sqrt{n}$. Thus the product, $p = p_1 p_2$, may be approximated by $p \approx \frac{.8}{\sqrt{n_1}} \cdot \frac{.8n_1}{\sqrt{n}} = .64 \sqrt{\frac{n_1}{n}}$, but only when n_1 is less than about $.5\sqrt{n}$. When n_1 increases beyond this point, this equation will give too high a value for p . As later analysis shows, $0.5\sqrt{n}$ is below the optimum value of n_1 .

³ Since n_1 is assumed even, we will assume that exact equality of positive and negative votes is decided by toss of a fair coin. This means that he will block action in only half the cases in which $i = n_1/2$, since the other half will be negative anyway. He will convert a negative vote to positive in half the cases when $i = n_1/2 - 1$, which gives a term in the binomial series equal to that in eq. (4). The sum of these two, each multiplied by $1/2$, gives the right hand side of eq. (4).

calculations could be carried out equally well by considering the probability of winning the election, rather than the probability of control. The probability of winning in the coalition is $0.5 + p_1/2$, and the probability of winning in the general election is $0.5 + p_1/2$ given that one won in the coalition, or $0.5 - p_1/2$, given that one lost in the coalition. Thus the overall probability of winning is $(0.5 + p_1/2)(0.5 + p_2/2) + (0.5 - p_1/2)(0.5 - p_2/2)$, or $0.5 + p_1 p_2/2$. Thus maximizing the probability of winning involves maximizing the product, $p_1 p_2$.

In order to find the size of n_1 , say n_1^* , that maximizes $p_1 p_2$, numerical analysis can be carried out for various values of n . It turns out that n_1^* is a simple linear function of \sqrt{n} ,

$$n_1^* \approx 1.4\sqrt{n}, \quad (7)$$

for values at least to $n = 10,000$.

If the other individuals in the corporate actor or in the coalition are not acting independently, but are acting with some mutual influence, then this calculation of course does not hold. If, however, the variance of the votes of the corporate actor over a series of issues can be estimated, then it is possible to consider the effective number of independent voters there are in the corporate actor. If the calculated variance of the proportion positive is σ^2 , then this is equated to the variance of a binomial distribution with probability $1/2$ and n' independent votes, where n' is treated as a variable to be calculated: $\sigma^2 = \frac{0.25}{n'}$, or $n' = 0.25/\sigma^2$. In that case, n' should take the place of n in eq. (7), and the left hand side of eq. (7) would be in that case the optimum number of *independent* coalition members, which can be labelled $n_1'^*$. If the coalition members are interdependent in the same degree as the remaining collectivity members, then one calculates the fraction of an independent person represented by each actual person, where the fraction, f , is n'/n , or $\frac{0.25}{n\sigma^2}$.

Then the actual coalition size is $\frac{1}{f} n_1'^*$. Thus where there is interdependence within the corporate actor and within the coalition, the optimum size of coalition is

$$\begin{aligned} n_1^* &= \frac{1}{f} n_1'^* \\ &= \frac{1}{f} 1.4 \sqrt{\frac{0.25}{\sigma^2}} \\ &= \frac{n\sigma^2}{0.25} 1.4 \sqrt{\frac{0.25}{\sigma^2}} \end{aligned}$$

$$\begin{aligned} &= 1.4n \sqrt{\frac{0.25}{\sigma^2}} \\ &= \frac{1.4n\sigma}{.5} \\ &= 2.8n\sigma \end{aligned} \quad (8)$$

Eq. (8) can be put in another form. If the variance estimate is used to calculate n' , the number of independent voters equivalent to the actual number, n , of voters in the corporate actor, then substituting for σ in eq. (8) gives

$$n_1^* = \frac{1.4n}{\sqrt{n'}} \quad (9)$$

Or if the degree of interdependence, $1 - \frac{n'}{n}$, is defined as q , then the equation for the optimum coalition size becomes

$$n_1^* = 1.4 \sqrt{\frac{n}{1-q}} \quad (10)$$

For example, if the actual corporate actor consists of one hundred members, but the standard deviation of the proportion positive in a series of votes is .138, this means that the corporate actor is voting as if there are thirteen independent voters. Using eq. (8), the optimum size of the coalition is

$$\begin{aligned} n_1^* &= 2.8 \cdot 100 \cdot .138 \\ &= 38.9 \end{aligned}$$

Or using eq. (9) gives

$$n_1^* = \frac{1.4 \cdot 100}{\sqrt{13}} = 38.9.$$

This compares to $n_1^* = 14$ for the case where all voters are voting independently (by use of eq. (7) with $n = 100$, or eq. (8) with $\sigma = \sqrt{0.25/n} = .05$, or eq. (9) with $n' = 100$).⁴

⁴Thus the optimum coalition size is larger if the individuals are acting interdependently. The increase in size depends on the degree of interdependence, as eq. (10) indicates. If the degree of interdependence is such that the number of independent voters in the corporate actor is less than about 7.8, then the optimum size coalition is one that is completely controlling, that is, $(n+1)/2$, as use of eq. (9) with $n_1^* = n/2$ shows.

The equation for finding the amount of control of an individual through a coalition, when there is interdependence of amount q , is, in place of eq. (5),

$$\begin{aligned} p &\approx 4\Pr\{0 < z < n_1/\sqrt{(n-n_1)(1-q)}\} \times \\ &\Pr\{0 < z < 1/\sqrt{(n_1-1)(1-q)}\}. \end{aligned}$$

MONOTHETIC AND POLYTHETIC TYPOLOGIES AND THEIR RELATION TO CONCEPTUALIZATION, MEASUREMENT AND SCALING *

KENNETH D. BAILEY

University of California, Los Angeles

American Sociological Review 1973, Vol. 38 (February):18-33

Past studies of the relationship between conceptualization and measurement in typology construction have depended upon the over-simplified heuristic-empirical (H-E) distinction. This paper presents the classification-identification and monothetic-polythetic distinctions. These are used not only to construct an extension of the H-E distinction (heuristic, empirical, classical and reduced classical types), but also to examine previously neglected relationships between types, scales, and forms of property-space reduction. Specifically, I show that all heuristic types and all categories of simple order scales (e.g., Guttman scales) are monothetic, while all empirical types and categories of partial order scales (e.g., Likert scales) are polythetic. I also show that a partial order scale can be constructed from a monothetic typology through Lazarsfeld's arbitrary numerical reduction, and a simple order scale can be constructed from a monothetic typology through functional reduction. Four characteristics of Weber's rational administration (as presented by Udy, 1958) are used to construct examples of the various typologies and scales.

TYPOLOGIES differ greatly, not only in content, but in form. The whole issue of typology construction and use is vast and complicated. Sociologists have come to no common ground about what a typology is. The typological literature runs the gamut from qualitative to quantitative (cf. Parsons, 1937; Weber, 1949; Hempel, 1952; Watkins, 1952; McQuitty, 1957; Martindale, 1959; Bloombaum, 1964; Capecchi, 1966; McKinney, 1966; Stinchcombe, 1968; Blalock, 1969; and Lopreato and Alston, 1970).

Many sociologists consider typologies to be theoretically derived sets of concepts. Others consider them to be derived from measurement, and useful primarily for condensing data from large samples. These disparate views can best be reconciled by constructing a typology of typologies which includes both conceptualization and measurement.

A number of typologies of typologies have been constructed. Three important ones are by Hempel (1952), Capecchi (1966), and McKinney (1966). Hempel lists three types of types: (1) classificatory; (2) extreme; and (3) ideal. Capecchi's typology includes seven dimensions:

(1) presence or absence of time-space links; (2) abstractness-non-abstractness; (3) normativity-non-normativity; (4) major-minor complexity; (5) presence-absence of relations between the variables; (6) major-minor evidence of the variables; and (7) level of quantification of the variables. McKinney includes six dimensions in his typology: (1) ideal-extracted; (2) general-specific; (3) scientific-historical; (4) time-less-time-bound; (5) universal-local; and (6) generalizing-individualizing.

All these typologies are valuable, but none resolves the conceptualization versus measurement argument. However, all three deal with the problem (though not in sufficient detail), in the heuristic-empirical (H-E) distinction. This distinction is represented by Hempel's ideal (H) versus classificatory (E) types; Capecchi's abstract (H) versus non-abstract (E) types; and McKinney's ideal (H) versus extracted (E) types.

Winch's (1947) heuristic-empirical dichotomy deals solely with the conceptualization-measurement problem. According to Winch (1947:68), heuristic types are conceptually derived, and do not have empirical examples. Empirical types, on the other hand, result solely from data analysis, without prior conceptualization.

The heuristic-empirical distinction is oversimplified, and tends to widen rather than narrow the gap between conceptualiza-

*Preparation of this paper was partially supported by UCLA Academic Senate Research Grant #2884. I am indebted to George A. Miller and to the *Review's* referees for their helpful comments on an earlier draft of this paper.

tion and measurement. This oversimplification can be combatted by distinguishing the operation of classification from the operation of identification, and by distinguishing between monothetic and polythetic types. The first goal of this paper is to present these two distinctions, and combine them to form a typology of typologies (shown to be an extension of the H-E dichotomy), that bridges the gap between conceptualization and measurement. The second goal is to show the relationship between typologies and scaling. The latter is a neglected topic in sociology.

CLASSIFICATION AND IDENTIFICATION

Typology construction and use consists of two analytically distinct operations which are often not distinguished. These operations are: (1) classification and (2) identification or assignment. Failure to distinguish between them can cause confusion. Classification is the ordering of concepts into groups (or sets) on the basis of their relationships, that is, of their associations by contiguity, similarity, or both (adapted from Sokal and Sneath, 1963:3). Thus defined, classification is the name of either the process or the end result (Sokal and Sneath, 1963:3). A multi-dimensional classification is a typology. A type concept is one cell of a typology. Classification is the generic term.

Classification is conceptualization. It must be distinguished from identification, or the assignment of empirical objects to the several cells of an established classifi-

cation (cf. Sokal and Sneath, 1963:58). Identification requires measurement, classification does not. Identification is accomplished by measuring a particular specimen on every variable of the typology, and assigning it to the type concept or cell which displays its particular configuration of values on all variables.

Blalock (1968) distinguishes between a theoretical or thinking language in which we conceptualize, and an operational language in which we specify ways to measure. Classification uses the former, identification uses the latter.

Figure 1 illustrates the operations of classification and identification. Figure 1a shows classification or conceptualization alone, without identification or assignment. This is easily recognizable as Winch's (1947:68) heuristic typology, best exemplified by the ideal type. An example is Weber's ideal type of "economic man," a hypothetical case that is extreme on all dimensions (he is perfectly rational, perfectly self-interested, and concerned only with material interests). The ideal type is well known and does not require extended discussion here. For further discussion see Parsons (1937) and Weber (1949).

Figure 1b shows data analysis on the empirical level without benefit of prior conceptualization. This is easily recognizable as Winch's (1947:68) empirical typology. All types derived from a purely descriptive analysis of data are empirical, as are all those derived through Q factor analysis or through cluster analysis using obverse

Figure 1. Three Important Combinations of Classification and Identification*

	Heuristic (a)	Empirical (b)	Classical** (c)
Classification	X ↓	Y ↑	Z ↓
Identification	None	Y'	Z'

* Lack of a superscript indicates a type concept resulting from classification. A superscript indicates an empirical example resulting from identification. The arrow is drawn from the operation performed first to the one performed second.

** Not named in H-E distinction.

correlations among persons. Examples of empirical typologies are: (1) Kretschmer's (1925) typology of body types; (2) Butler and Adams' (1966) typology of delinquent girls derived through Q factor analysis; and (3) Bruce and Witt's (1971) typology of cities derived through cluster analysis.

It is clear that the heuristic and empirical types do not exhaust the possibilities of Figure 1. Often typologists are more interested in the situation of Figure 1c. Here a typologist first constructs a type concept or entire typology in his mind, and then sets out for the field to see how many specimens he can find to fit each cell. This is the classical procedure, familiar to every sociologist in the form of a fourfold or larger statistical table. We call Figure 1c the classical typology.¹

Such typologies are often used by statisticians to discover relationships among variables. The typologists' main goal is to determine which cells are empirically null. Sociologists owe a debt of gratitude to Stinchcombe (1968:47) for pointing out that the potentially large typology with many empirically null cells is the most fundamentally important typology. He says:

In summary, then, typologies have two radically different functions in scientific theory, one of which is fundamental, the other of which is just a convenience. In the first case a typology is a statement that a large number of variables have only a small number of combinations of values which actually occur, with all other combinations being rare or nonexistent. This results in a radical improvement in scientific theory.

Notice that the typology which Stinchcombe says is the most important of all

¹ Construction of such a typology, of course, requires some prior knowledge of the general phenomena studied, but does not require actual drawing of the sample as does the empirical typology of Figure 1b. Empirical typologies are often quite unique to the particular sample and very difficult to generalize. An investigator who is entirely ignorant of the phenomena to be studied may still construct a classical typology. However, it is less likely to be adequate. Construction of any typology, whether heuristic, empirical, or classical, requires prior selection of the component dimensions. The difference is that in heuristic and classical typologies the particular configuration of values defining each cell can be ascertained prior to data analysis. This is not possible in the empirical case.

is that of Figure 1c, the classical.² Notice also that such typologies are completely left out of the H-E distinction since they are both conceptual and empirical. Such typologies cannot be labeled simultaneously both heuristic and empirical without destroying the mutual exclusiveness of the H-E distinction, thus making it no more useful than a classification by sex which allows persons to be considered both male and female simultaneously. Obviously our extension is warranted.

In order for empirical examples to exist we cannot define the classical type as extreme on all values, as is the ideal type. We must define it as composed of variables capable of taking on a full range of values. This is the tactic recommended by Parsons (1937:601-24). He advocated study of the general analytic properties underlying ideal types, and analysis of the entire range of possible combinations. This is basically the same as Lazarsfeld's (1937) process of substruction. Udy (1959) provided an example. He extended seven of Weber's rational-bureaucratic characteristics to form variables. By correlating these variables he divided the original ideal type into two types: (1) rational (four variables); and bureaucratic (three variables). For further discussion see Blalock (1969:32-3).

Much of the ideal type's appeal comes from its usefulness as a model. By destroying its extreme nature we are perhaps rendering it less useful as a heuristic device. The constructed type has been developed in an attempt to retain the generality of the ideal type while gaining the specificity of the empirical type. McKinney (1966:26) says:

In summary, the Becker-McKinney model of constructed type is an ideal type shorn of any purely fictional qualities, firmly grounded in the particularities of actual situations, and constituted by attributes that are empirically discoverable.

The constructed type can have empirical

² Stinchcombe's typology is clearly classical because: (1) in a heuristic typology you cannot tell which combinations actually occur (which cells have empirical examples); (2) in an empirical typology you cannot tell if only a few combinations exist out of many, because not all potential cells are examined.

examples assigned to it (although they are rare). If the operations of classification and assignment are both carried out, the constructed type is our classical type. If only classification is carried out, it is our heuristic type.

Taken together, Figures 1a, 1b, and 1c constitute an extension of the H-E dichotomy which exhausts all combinations of classification and identification.³ Nevertheless, our extension of the H-E distinction is not complete. To complete it we must examine the monothetic-polythetic distinction.

MONOTHETIC AND POLYTHETIC TYPOLOGIES

Monothetic and polythetic typologies differ chiefly in their requirements for the identification of specimens. A typology is monothetic if possession of a unique set of features is both necessary and sufficient for identifying a specimen as belonging to a particular cell of the typology (adapted from Sokal and Sneath, 1963:13). That is, each feature is necessary and the set is sufficient. The necessary features can be stipulated prior to measurement and assignment of specimens.

In contrast, a polythetic typology is constructed by grouping together those individuals within a particular sample which have the greatest number of shared features. No single feature is either necessary or sufficient (Sokal and Sneath, 1963:14). The objects or specimens are grouped so as to maximize overall similarity within each group. The degree of overall similarity on all variables can be determined only through measurement. Thus, polythetic types can-

not be constructed prior to measurement.⁴

The formal definition of a polythetic group (which he calls "polytypic") is presented by Beckner (1959:22):

A class is ordinarily defined by reference to a set of properties which are both necessary and sufficient (by stipulation) for membership in the class. It is possible, however, to define a group K in terms of a set G of properties f_1, f_2, \dots, f_n in a different manner. Suppose we have an aggregation of individuals (we shall not as yet call them a class) such that:

1) Each one possesses a large (but unspecified) number of the properties in G .

2) Each f in G is possessed by large numbers of these individuals;

and

3) No f in G is possessed by every individual in the aggregate.

A class is called fully polythetic if all three conditions are fulfilled, and polythetic if only the first two are fulfilled.

Each f is a property or attribute that can be coded present or absent, or 1 or 0. In variables containing several categories, each category is an f , and can be coded 1 or 0 as in dummy variable regression analysis. For example, if each of N variables contained n categories, the number of f s would be $N \times n$. However, each individual will possess only one f on each variable, because at any one time he has only one value of the variable. If he scores 0 on the variable he will possess no f s on it.

Notice that when Beckner says a class is ordinarily defined by a set of necessary and sufficient properties, he is speaking of a monothetic class. Notice also that he says that necessity and sufficiency are attained by stipulation. This simply means that we define a class as having these—and only these—properties, and assign only individuals possessing them to the class.

The polythetic class is a compromise to empirical reality. As noted by Stinchcombe above, we want any empirical typology (or classical typology after assignment) to contain as few cells as possible. Achieving parsimony is one of the chief reasons for assigning specimens to a typology.

³ Additional combinations can be formed only by dividing Figure 1b into two types

Classification	None	Y_2
	↑	↑
Identification	Y_1'	Y_2'
	(b_1)	(b_2)

one with no conceptual referent (b_1); and one whose conceptual referent is defined only as a direct mental image or perception of the specimen (b_2). The former represents identification without classification. However, it is debatable whether one can study a group of empirical specimens without having a conceptual image of them. Thus, it is debatable whether division of (b) into (b_1) and (b_2) is empirically possible even though it is analytically possible. Winch's (1947) empirical type seems to include both without distinguishing between them.

⁴ This statement must be clarified. The notion of a polythetic class has two aspects: (1) heterogeneity; (2) minimum heterogeneity (maximum homogeneity). Only (2) cannot be achieved prior to measurement.

298416

If we insist on monotheticism, it will be a miracle if all empirical specimens fall into only one of a few types, because the empirical world is simply too diverse and imperfect. For example, if we are assigning on the basis of 121 variables as Stephenson (1953:129) did in constructing his empirical typology of introversion-extroversion, it will be a miracle if even a few specimens are identical in value on all 121. Millions of potential cells can be formed from 121 variables. For example, only twenty-five dichotomous variables yield 2^{25} or 33,554,432 cells. Each pair of specimens will probably differ on at least one f , so that if we assign them to the same class this class will be polythetic.

The monothetic-polythetic distinction does not depend on the reliability of measurement. It might seem from the definitions of monotheticism and polytheticism that we would define properties as necessary (and thus the type as monothetic) if they could be reliably measured. Properties that could not be reliably measured would not be used singly, but only together, with none being necessary.

Notice, though, that a monothetic type can be defined at the conceptual level prior to measurement. Unreliable or otherwise inadequate measures would hinder the operation of assignment, but would not affect the definition of monotheticism. Conversely, reliable measures for all variables would not change a polythetic type to monothetic. All variables are measured for each individual during the operation of assignment, regardless of whether the individual is assigned to a monothetic or polythetic type; and reliability of measurement affects efficacy of assignment, but not the definition of the type.⁵

⁵ The only requirement for constructing either monothetic or polythetic typologies is that all dimensions be nominal variables of at least two categories (or one category coded present or absent). If one can discriminate between two categories of a concept he may use that concept as a dimension in his typology. If concepts are so ambiguous that they fail to meet even these minimal criteria, then they cannot be used in typology construction.

Some sociologists use very crude dichotomies such as authoritarian-nonauthoritarian. If the borderline between the two categories is so arbitrary that two or more raters cannot agree whether a particular case is authoritarian or nonauthoritarian,

The goal of typology construction is to construct types which are as perfect as possible. At the conceptual level, this means conceptual purity, and thus lack of ambiguity. At the empirical level this means a group of homogeneous objects. The monothetic type is the perfect type on both levels.

Polythetic types are also constructed to be as homogeneous as is empirically possible, usually by means of cluster analysis or Q factor analysis. All clustering methods yield empirical types. These methods are many and diverse, and cannot be described in detail here. Briefly, they often use obverse correlations (correlations between two persons rather than two variables), and seek to form types in which persons or objects within one type are more similar to (have higher correlations with) each other than with persons in another type. This creates types which are as perfect as the imperfection of the empirical data will allow.

Clustering methods can group individuals on the basis of hundreds of items. In fact,

then obviously measurement is too crude for typology construction. With such crude measurement, the f s are so ambiguously delimited that the monothetic-polythetic distinction is threatened. Obviously such concepts are unsuitable for computing statistics, or for any serious field work.

Careful examination often reveals such a crude dichotomous concept to be an alleged unidimensional scale whose scalability has not been proven. Examples of such dichotomized scales are: rational *versus* nonrational; formal *versus* informal; conformity *versus* nonconformity; and bureaucratic *versus* nonbureaucratic. Often such a scale can be made suitable for typology construction by decomposing it, and using the various component items as dimensions, rather than using the entire scale as a single dimension.

For example, in Table 1 we used four items out of an organizational rationality scale to construct a typology, rather than using rationality as one dimension. These four items can be scored present-absent. They are comparatively unambiguous and comparatively easy to operationalize (although ambiguities do exist, see Udy, 1958). If adequate scales can be assumed, then several may be combined to construct higher order typologies.

Selecting variables for constructing typologies remains highly subjective. Further discussion of this topic is beyond the scope of this paper. More work needs to be done on both the theoretical and pragmatic considerations which govern the selection of variables. Generally, we wish to select those variables which will make our polythetic types as homogeneous as possible. These will be variables that are highly intercorrelated.

Q factor analysis requires several times as many variables as individuals. As Fleiss and Zubin (1969:238) note, Q factor analysis seems to maximize the number of variables. Consequently, the resulting typologies can contain millions of potential cells. However, no quantitative method currently in use yields all potential types. Only a few types are produced, and they are necessarily polythetic. For further discussion of clustering see Sokal and Sneath (1963), Ball (1965), and Friedman and Rubin (1967).

The monothetic-polythetic distinction is trivial in the univariate case, and may not be very important when one has only a few dimensions and a few specimens to be assigned. But for the multivariate case (e.g., with 121 variables as in Stephenson's work), it is indispensable. The monothetic definition does not allow a typologist to group together several specimens that clearly belong together, but differ on only one or two of perhaps 121 variables; the polythetic does.

CLASSIFICATION-IDENTIFICATION AND MONOTHETICISM-POLYTHETICISM COMBINED

The classification-identification and monothetic-polythetic dimensions are correlated. A typology formed as a conceptual exercise before measurement is undertaken will be monothetic. Conversely, a typology formed by measurement and data analysis without prior conceptualization will be polythetic. This means that heuristic typologies are monothetic; empirical typologies are polythetic.⁶

⁶ The reasons why heuristic typologies are monothetic and empirical typologies are polythetic should now be clear. A monothetic type is more homogeneous than a polythetic type. Or, in other words, a monothetic heuristic type is less ambiguous than a polythetic one. We are free to create polythetic heuristic types if we desire ambiguous concepts, but usually we desire unambiguous concepts, and so create monothetic heuristic types. Also, we have no assurance that a heuristic type is polythetic, because we have no way to determine whether Beckner's condition (2) is fulfilled (see Footnote 4). In fact, Beckner's definition of polytheticism clearly assumes measurement and assignment.

We also construct empirical types to be as homogeneous as possible, but complete homogeneity is rare for typologies with many variables. Monothetic types are often called "artificial" because

Classical typologies are originally monothetic before assignment. After assignment they may be revised to various degrees of polytheticism, depending on the specific data being analyzed. This is analogous to the process of revising a hypothesis on the basis of empirical findings.⁷

For examples of monothetic and polythetic groups consider Table 1, Table 2, and Table 3. Both Stinchcombe (1959) and Udy (1959) found that Weber's ideal type concept of bureaucracy could be divided into two type concepts of (1) rational; and (2) bureaucratic administration. Further, Udy (1958) found in a study of twenty-five nonindustrial production organizations that

they seldom appear as naturally occurring groups (see Sokal and Sneath, 1963:170-1). Thus, for all practical purposes, the polythetic heuristic and monothetic empirical types are empirically null in Table 4 below.

⁷ It should now also be clear that a classical typology will be monothetic before assignment (for the same reason that a heuristic typology is monothetic). It can be converted to polytheticism only by increasing the conceptual ambiguity of each type. After assignment it is possible to delimit the maximally homogeneous polythetic types. When assigning specimens to a classical typology, we must seek to achieve the goals of typology construction as stated by Stinchcombe and quoted above: reduction of a large number of possible cells to only a few cells which contain specimens, with all other cells null.

The problem is that these few remaining cells described by Stinchcombe are monothetic, as a careful reading of his discussion will show (Stinchcombe, 1968:43-5). A perfect classical typology according to Stinchcombe's description would be one in which all specimens fall into only two or three monothetic cells out of millions. Unfortunately this event occurs rarely, if ever. It requires extremely high intercorrelations among all dimensions of the typology.

The most practical goal of classical typology construction is to assign specimens to the minimum number of cells each of which is maximally homogeneous. This excludes two extremes which are incompatible with Stinchcombe's definition of a good classical typology: (1) the entire sample of specimens assigned to a single type; (2) each specimen assigned to its own individual type (random or uniform distribution). The former achieves parsimony at the expense of monotheticism; the latter achieves monotheticism at the expense of parsimony.

The only realistic alternative is to compromise: to sacrifice monotheticism somewhat, and sacrifice parsimony somewhat. The goal is to construct a set of polythetic types which is parsimonious enough to satisfy the typologist, with each type also being homogeneous enough to satisfy him.

Table 1. A Sixteen-cell Monothetic Heuristic Typology Based on Weber's Four Dimensions of Rational Administration as Presented by Udy (1958)*

Variable 3: Performance Emphasis	Variable 1: Compensatory Rewards			
	Present (1)		Absent (0)	
	Variable 2: Specialization		Variable 2: Specialization	
	Present (1)	Absent (0)	Present (1)	Absent (0)
Present (1)	(1,1,1,1)	(1,0,1,1)	(0,1,1,1)	(0,0,1,1)
Variable 4: Segmental Participation				
Present (1)	1	2	3	4
Absent (0)	(1,1,1,0)	(1,0,1,0)	(0,1,1,0)	(0,0,1,0)
	5	6	7	8
Absent (0)	(1,1,0,1)	(1,0,0,1)	(0,1,0,1)	(0,0,0,1)
Variable 4: Segmental Participation				
Present (1)	9	10	11	12
Absent (0)	(1,1,0,0)	(1,0,0,0)	(0,1,0,0)	(0,0,0,0)
	13	14	15	16

* The four-variable score for each monothetic cell is determined by coding each variable as 1 if present, and 0 if absent. For example, the score for Cell 1 is (1,1,1,1). If a specimen has any other configuration, it cannot be assigned to Cell 1.

the four dimensions of the rational type formed a Guttman scale.

Table 1 and Table 2 are based on these four rational dimensions. As presented by Udy (1958) they are: (1) compensatory rewards (allocation of rewards from higher to lower positions); (2) specialization; (3) performance emphasis (rewards proportional to performance); and (4) segmental participation (participation based on contractual agreement). Udy presents these four characteristics as "present *versus* absent" variables. For further discussion see Blau and Scott (1962:205-10).

Table 1 shows classification only, without measurement, identification, or any sort of data analysis. Table 2 shows raw empiricism, or measurement and data analysis only, without prior type concept formation or conceptualization of any kind.

Each cell in Table 1 is monothetic. If each of the four variables is coded 1 for present or 0 for absent, then a particular empirical specimen (production organization) can be assigned to a particular cell (type concept), only if it possesses the exact configuration of 1s and 0s required by that cell. For example, if the variables are scored in this order (compensatory rewards, specialization, performance emphasis, segmental participation), Cell 1 requires a score of (1, 1, 1, 1). If a specimen scores (1, 1, 1, 0) or any other configuration, it cannot be admitted. The typology of Table 1 is clearly monothetic.

Now consider the hypothetical organizations of Table 2. The presence or absence of all four variables is indicated for each organization. If we disregard Table 1, we see that even in lieu of prior conceptualiza-

Table 2. Polythetic Empirical Types of Organizational Rationality Formed without Prior Classification or Conceptualization (all organizations are hypothetical)

Organization	Score ¹	Type 1 ² (Rational)	Type 2 ³ (Nonrational)
A	(1,1,1,1)	X	
B	(1,0,1,1)	X	
C	(1,1,1,0)	X	
D	(0,0,0,0)		X
E	(0,0,0,1)		X
F	(0,1,0,0)		X

¹The score for each organization is determined by coding each of Udy's four rationality variables as 1 if present, and 0 if absent, as in Table 1.

²Type 1 is polythetic because Organizations A, B, and C do not possess common values on all four variables. Type 1 is not fully polythetic because A, B, and C do possess common values on variables 1 and 3.

³Type 2 is polythetic because Organizations D, E, and F do not possess common values on all four variables. Type 2 is not fully polythetic because D, E, and F do possess common values on variables 1 and 3.

tion the organizations fall quite clearly into two empirical types. Type 1 is essentially rational, since all organizations in it possess at least three of the four characteristics. Type 2 is essentially nonrational, since all of the organizations in it lack at least three of the four characteristics. But notice that no two organizations are identical within either type. Each differs from others within the type on at least one variable. Thus, the two types do not meet the definition of monotheticism, and must be polythetic.

Type 1 exhibits a score of 1 for all organizations on variable 1 and variable 3; but either 1 or 0 on variables 2 and 4 (1, 1 or 0, 1, 1 or 0). Thus, it is not fully polythetic. Similarly, Type 2 exhibits (0, 1 or 0, 0, 1 or 0) and is not fully polythetic. A fully polythetic type will exhibit (1 or 0, 1 or 0, 1 or 0, 1 or 0).

Table 1 is a heuristic typology.⁸ Table 2 is an empirical typology. If the specimens of Table 2 can be assigned to Table 1, then

⁸It should be noted that not all heuristic types take the form of Table 1 (i.e., a full table showing intersections of particular levels of a number of interrelated variables). Individual types such as hypothetical, ideal, or imaginary types may all qualify as heuristic. The only requirement is that they be types formed conceptually and in lieu of actual data measurement.

we have a classical typology, as in Table 3. Notice that this typology remains monothetic, but that each specimen occupies its own cell. No two specimens fall into the same cell. The specimens occupy monothetic Cells 1, 2, 5 (rational); and Cells 12, 15, 16 (nonrational). No real grouping has occurred. We began with six organizations and we end with six types. No parsimony has been achieved as in Table 2, where all six specimens were condensed into only two types. Parsimony can be achieved only by reducing the classical typology.

Reduction From Monotheticism to Polytheticism

The number of cells in a monothetic classical typology can be reduced through Lazarsfeld's (1937) three forms of reduction. Two of these forms (pragmatic and arbitrary numerical) result in polythetic types. We call the resulting typologies "reduced classical." The types remaining after functional reduction are monothetic unless the cell boundaries are widened, or unless one of the other two forms of reduction is performed simultaneously. For this reason we do not distinguish functionally reduced classical typologies from nonreduced classical typologies. We call them both classical.

Table 3. Classical Typology (Monothetic): Formed by Assigning the Specimens of Table 2 to the Monothetic Typology of Table 1.

Variable 3: Performance Emphasis	Variable 1: Compensatory Rewards			
	Present (1)		Absent (0)	
	Variable 2: Specialization		Variable 2: Specialization	
	Present (1)	Absent (0)	Present (1)	Absent (0)
Present (1)	Organization A	Organization B		
Variable 4: Segmental Participation	(1,1,1,1)	(1,0,1,1)	(0,1,1,1)	(0,0,1,1)
Present (1)	1	2	3	4
Absent (0)	Organization C			
Variable 4: Segmental Participation	(1,1,1,0)	(1,0,1,0)	(0,1,1,0)	(0,0,1,0)
	5	6	7	8
Absent (0)				Organization E
Variable 4: Segmental Participation	(1,1,0,1)	(1,0,0,1)	(0,1,0,1)	(0,0,0,1)
Present (1)	9	10	11	12
Absent (0)			Organization F	Organization D
Variable 4: Segmental Participation	(1,1,0,0)	(1,0,0,0)	(0,1,0,0)	(0,0,0,0)
	13	14	15	16

First consider pragmatic reduction. In Table 3 we can collapse contiguous Cells 1, 2, and 5 into one polythetic type; and Cells 12, 15, and 16 into another. The two resulting types are respectively the same rational and nonrational types formed in Table 2. This collapsing procedure is pragmatic reduction (Lazarsfeld, 1937:128). Pragmatic reduction converts monothetic cells to polythetic by creating fewer cells, each of which has wider boundaries and admits more diverse specimens. If we collapse on all dimensions (so that no unique *f* is possessed by all individuals), then the type becomes fully polythetic. If we do not collapse on all dimensions, individuals share values on the dimensions not collapsed, so the type is polythetic rather than fully polythetic.

The second form of reduction is arbitrary numerical. Lazarsfeld (1937:128) provides

an example. He says that in constructing an index of housing conditions, one might weight plumbing without either central heat or a refrigerator equal to the other two without plumbing. Coding existence of an attribute by 1 and lack of it by 0, and taking variables in this order (plumbing, central heat, refrigerator), Lazarsfeld is saying that (1, 0, 0) = (0, 1, 1). As an analogous example in Table 3, we could let (1, 0, 0, 0) = (0, 1, 1, 1), or Cell 14 = Cell 3. Together these two cells form a polythetic type. An organization belongs to this type whether it is assigned to Cell 14 or Cell 3.

Arbitrary numerical reduction is tantamount to folding over the typology, and making formerly distant types equal. This procedure is often quite useful in index construction and scaling, as will be shown below.

Lazarsfeld's (1937:128) functional re-

duction consists of discarding from the typology all empirically null, and thus unnecessary cells. It can be used to simplify the analysis by removing the entire middle and two corners of the typology in Table 3, that is Cells 3 and 4, 6-8, 9-11, and 13 and 14. The remaining cells are not empirically null, and thus cannot be discarded. The typology labelled fundamental by Stinchcombe is clearly the result of functional reduction.

Functional reduction is a true method of reduction. It actually reduces the size or area of the typology, as contrasted with pragmatic reduction and arbitrary numerical reduction, which reduce the number of cells, but not the amount or area of property-space analyzed. Functional reduction is only feasible where the dimensions of the typology are correlated (see Lazarsfeld, 1937:128). For further discussion of reduction see Bailey (1972).

The Extended H-E Typology

We have now discussed four types of typologies: (1) heuristic; (2) empirical; (3) classical; and (4) reduced classical. By combining the two dimensions of classification-identification and monotheticism-polytheticism we can create a six-celled typology of typologies (shown in Table 4) which includes these four types plus two empirically null cells. Notice that the typology of Table 4 includes the original H-E distinction. This leads us to suspect that our two dimensions were latent and unnamed dimensions in the H-E typology, so that our typology represents an extension through Lazarsfeld's (1937:132-8) process of substruction.

MONOTHETICISM-POLYTHETICISM AND SCALING

Little has been written on the relationship between typologies and scaling (although Lazarsfeld's 1937 discussion of reduction does indicate a desire to achieve unidimensionality; see page 129). Discussion of this relationship is essential for understanding how the monothetic-polythetic distinction applies to single test items (single observable indicators) and overall test scores (scales). We will limit our dis-

Table 4. Extended Typology Based on the Classification-Identification and Monothetic-Polythetic Distinctions¹

	Monothetic	Polythetic
Classification only	Heuristic	Null ²
Identification, then Classification	Null ²	Empirical
Classification, then Identification	Classical	Reduced Classical

¹Monotheticism and polytheticism are mutually exclusive concepts. A class cannot be simultaneously monothetic and polythetic. Thus, there is not a third column in the typology. However, classification and identification are separate research operations both of which can be performed on the same typology. Thus, there is a third row. There would be a fourth row if one included identification without classification (Footnote 3).

²For discussion of why these cells are generally null, see Footnote 6.

cussion to unidimensional scaling. The relationships between typologies and multidimensional scales are interesting, but beyond the scope of this paper. For discussion of multidimensional scaling see Coleman (1971).

In unidimensional scaling we try to combine all dimensions of the typology so as to form a scale of a single property (e.g., rationality, bureaucracy, authoritarianism, or alienation). We can attempt to establish either partial order or single order on the property.

Partial Order

Partial order is characterized by the existence of tie scores on the property. That is, there are two or more ways to make the same score on the property (Phillips, 1971: 221-2).

An example of partial order is provided by a common school examination, say in arithmetic. Assume that there are ten questions, and each counts ten points on the scale of arithmetic knowledge. Mary an-

swers the first three test items correctly, but misses the last seven. John misses the first seven, but correctly answers the last three. Each pupil scores thirty points. The scale is partial order because the two students made identical scores of thirty in completely different ways.

The chief problem with a partial order scale is the difficulty of insuring unidimensionality. When the same score is being made two different ways there is little guarantee that some of the items are not in fact measuring some other property (e.g., authoritarianism instead of rationality). The most famous method for constructing partial order scales is Likert scaling (cf. Murphy and Likert, 1937). The Likert procedure has built-in safeguards to maximize the probability that unidimensionality will be achieved (Phillips, 1971:222-3).

For an illustration of the relationship between a typology and a partial order scale reconsider Table 1. Remember that each of the sixteen cells is monothetic. There is a unique configuration of 1s and 0s required for admission to each cell. No two cells are alike. Each organization we study can be assigned to one and only one of the sixteen cells.

We want to form a partial order unidimensional scale of rationality from this typology. This requires tie scores. To obtain them we first code each present character as 1 and each absent character as 0, as before. We then assign a scale score to each of the sixteen monothetic cells. We obtain

each score by summing the 1s (summed rating). A cell's score is simply the number of 1s it contains.

Now we have partial order because there is more than one way to make a single score. For example, an organization that scored 0 on the first three variables and 1 on the last would have a scale score of 1 on rationality and would be assigned to Cell 12. An organization that scored 1 on the first variable and 0 on the last three would be assigned to Cell 14. This organization would also have a scale score of 1 on rationality. Since there are four items in the typology (and in the scale), there are five scores that can be obtained (0, 1, 2, 3, or 4). Table 5 shows the number of monothetic cells in each scale score type.

Notice that the distribution of cells shown in Table 5 is binomial, and the total number of combinations (cells) for the four items is 2^4 or 16. Type 1 (scale score 0) and Type 5 (scale score 4) are monothetic, containing only one cell each, because there is only one unique way to make a score of either 0 or 4 respectively. These are recognizable as the polar types. Types 2 through 4 are all obviously polythetic. For example, there are six different ways to make a scale score of 2 (i.e., six different cells form Type 3). These six include cells as disparate as Cell 6 (only compensatory rewards and performance emphasis present) and Cell 11 (only specialization and segmental participation present).

By considering only the sum total of 1s

Table 5. The Composition of Partial Order Scale Types as Constructed by Means of Lazarfeld's Arbitrary Numerical Reduction

Type Number	Scale Score	Number of Monothetic Cells Used in Constructing the Type	Location of These Cells in Table 1	Whether Resulting Type as Formed by Arbitrary Numerical Reduction Is Monothetic or Polythetic
1	0	1	Cell 16	M
2	1	4	Cells 8, 12, 14, 15	P
3	2	6	Cells 4, 6, 7, 10, 11, 13	P
4	3	4	Cells 2, 3, 5, 9	P
5	4	1	Cell 1	M
	$\bar{X} = 2$	Sum Total = 16 = 2^4		

for each cell, and failing to notice which specific items the 1s occurred on (the exact configuration of 1s and 0s), we are in effect performing Lazarsfeld's arbitrary numerical reduction. For example, in Type 3, Cell 4 has a score on the four items respectively of (0, 0, 1, 1), while Cell 13 has a score of (1, 1, 0, 0). Each of these configurations is unique in the typology, and each of these two cells is monothetic. But by scoring each as 2, we are destroying its uniqueness. We now merely have two different ways to make the same score of 2 on rationality, and thus to be admitted to Type 3. We are in effect saying that (0, 0, 1, 1) = (1, 1, 0, 0). This is clearly arbitrary numerical reduction (Lazarsfeld, 1937:128).

Simple Order

Partial order is a weak form of scaling. A stronger form is simple order. In a simple order scale there are no tie scores on the property (Phillips, 1971:221). Unlike partial order where there were six different ways to make a score of two on rationality, a simple order scale would permit only one unique way to make a score of two (or any other score). Since each score is unique, we can tell by looking at a person's score exactly how he answered all items. Simple

order scales provide a much stronger guarantee of unidimensionality than do partial order scales.

The most famous method for constructing simple order scales is Guttman's (1950) *scalogram analysis*. Our four item scale of rationality as it would appear in Guttman's cumulative format is presented in Table 6. Notice that the five Guttman scale scores are represented respectively by five different monothetic cells in Table 1 (Cells 1, 5, 13, 14, 16). Never can more than one cell be used to attain a single score as in partial order scaling.

As can be seen from Table 6, if an organization has a scale score of 0 (Type 1) it should possess none of the four characteristics. If it has a score of 1 (Type 2) it should possess only compensatory rewards. For a score of 2 (Type 3) it should possess compensatory rewards and specialization. A score of 3 (Type 4) is made by possession of compensatory rewards, specialization, and performance emphasis. Type 5 (scale score 4) is represented by possession of all four characteristics.

If the scale is perfect, all of the other eleven cells in Table 1 will be empirically null. If the scale is imperfect, some empirical specimens will fall into some of the other eleven cells (Cells 2-4, 6-8, 9-12, or

Table 6. The Composition of Simple Order Guttman Scale Types as Constructed by Means of Lazarsfeld's Functional Reduction

Type Number	Scale Score	Number of Cells Used in Constructing the Type	Location of These Cells in Table 1	Whether Type is Monothetic or Polythetic	Configuration of Characteristics	Example from Udy (1958:417)
					Characteristic CR ¹ S ² PE ³ SP ⁴ (1) (2) (3) (4)	
5	4	1	Cell 1	M	(1, 1, 1, 1)	Paiute
4	3	1	Cell 5	M	(1, 1, 1, 0)	Kaybles
3	2	1	Cell 13	M	(1, 1, 0, 0)	Lobi
2	1	1	Cell 14	M	(1, 0, 0, 0)	Crow
1	0	1	Cell 16	M	(0, 0, 0, 0)	Tibetans

¹CR = Compensatory rewards (Udy, 1958)

²S = Specialization (Udy, 1958)

³PE = Performance emphasis (Udy, 1958)

⁴SP = Segmental participation (Udy, 1958)

15). All organizations falling into cells that should be null must be considered scaling errors.

For example, if an organization possessed only one characteristic, but possessed number 3 (performance emphasis) instead of number 1 (compensatory rewards), then this organization would score (0, 0, 1, 0) instead of the expected (1, 0, 0, 0). It would be assigned to Cell 8 of Table 1. This is an error according to simple order Guttman scaling, which would predict that all organizations with a scale score of 1 should be assigned to Cell 14 of Table 1. If there are too many scaling errors we will be unable to reproduce the exact pattern of answers for a given organization accurately.

In his study of nonindustrial production organizations, Udy (1958) found only one Guttman scale error out of twenty-five cases. This occurred when the Samoans possessed only one characteristic, but possessed specialization instead of compensatory rewards. Thus, they must be assigned to Cell 15 in Table 1 instead of Cell 14. Examples of each scale score as selected from the twenty-four organizations scoring as predicted are: (1) scale score 0—Tibetans; (2) scale score 1—Crow; (3) scale score 2—Lobi (hunting); (4) scale score 3—Kaybles (construction I); and (5) scale score 4—Paiute (Udy, 1958:417).

Since all of the specimens should be assigned to only five cells out of sixteen, we can use Lazarsfeld's (1937:128) technique of functional reduction to eliminate the other eleven cells from the typology. The five cells that remain after functional reduction constitute a simple order Guttman scale of rationality. If we know an organization's Guttman scale score, we can tell which cell it is assigned to (each organization is assigned to one and only one cell). It is then an easy matter to reproduce its value on all items.

Partial and Simple Order Compared

It can now be seen that the definition of polytheticism is synonymous with the definition of partial order (more than one way to make a certain score or be assigned to a certain type). Similarly, the definition of monotheticism is synonymous with the definition

of simple order (one unique way to make a certain score or be assigned to a certain type). A partial order scale contains monothetic, polythetic, and fully polythetic types, while a simple order scale is composed solely of monothetic types.

If functional reduction can successfully be used to reduce a large number of cells to only a few, the resulting typology will fulfill Stinchcombe's requirements for the fundamental typology. Then monotheticism can be retained and the items may form a simple order scale.

But notice that if we have a large sample of specimens, and if many cells are null, the frequencies in the remaining cells must be large. Often we find instead that no cells are null, but that each cell frequency is low. To gain parsimony without discarding any specimens from our sample we need some way to exhaust the typology yet reduce the number of cells. Arbitrary numerical reduction yielding a partial order scale composed of polythetic categories meets our needs. Thus, we use it even though it provides less guarantee of unidimensionality than does simple order scaling.

After a unidimensional scale is constructed, it can always be further reduced through pragmatic reduction. This is true for both partial order and simple order scales. For example, we can call a score of 3 or more "high rationality," and a score of 2 or less "low rationality." This means collapsing together Types 4 and 5 (high) and Types 1, 2, 3 (low) of the partial order scale. This is a clear case of pragmatic reduction.

In the case of simple order, such dichotomizing creates two polythetic types by collapsing together the monothetic Cells 1 and 5 (high) and Cells 13, 14, 16 (low). The two resulting types are not fully polythetic. As shown in Table 6, the high type requires a score of 1 on the first three variables, but either 1 or 0 on the last (1, 1, 1, 1 or 0). The low type is (1 or 0, 1 or 0, 0, 0). If a researcher is lucky enough to achieve simple order scaling with few errors, it will generally be unwise to sacrifice monotheticism by dichotomizing. He has too much information to lose, and little parsimony to gain, at least compared to the original gain in parsimony achieved by reducing from sixteen cells to only five.

One has much less to lose by dichotomizing our partial order scale. Some of the scale types (e.g., Type 2 for scale score 1, and Type 4 for scale score 3) are already fully polythetic (see Table 5 and Table 1), with none of the four variables having a common score on all cells in the type. The two final types (high rationality and low rationality) will be fully polythetic, and will contain sixteen cells, thus exhausting the typology.

CONCLUDING REMARKS

The monothetic-polythetic distinction is very important because the process of typology construction and use includes both concept formation and data analysis. Excluding either operation simplifies the task greatly, but precludes construction of what Stinchcombe (1968:47) calls the fundamental typology—one that has many potential combinations of variables, but only a few that actually occur empirically.

It is a large task to both construct concepts and also analyze empirical data and assign specimens to the cells, especially, for large typologies with many variables and many categories in each. Both concept formation and data analysis are difficult specialities mastered by few. Each has its own special requirements.

First consider concept formation. Concepts are always defined monothetically. This is essential if they are to be even half-way unambiguous. It would be ridiculous, for example, to define the concept of upper class as being either high or low on education and either high or low on income. This definition is ambiguity personified, and such a polythetic concept is totally unacceptable. We say instead that the upper class is high on income and high on education. Low income and low education can then be defined as lower class.

Now consider assignment and data analysis. Assignments into monothetic cells may proceed smoothly if one has only a few variables and a few specimens. But typologies with only a few variables are often theoretically irrelevant, and may be the result of attempts at simplification. If one has many variables and many specimens, he will almost certainly be forced to reduce to polytheticism. Otherwise, he will find that he

has assigned only one or a few specimens to each of hundreds or even thousands of cells, with no real grouping. He will have about as much trouble summarizing this data as he would have had summarizing his original data, and could have saved himself the effort of constructing the monothetic typology. The only way he can attain parsimony is by reducing to polytheticism.

The great amount of imperfection in the empirical world makes retention of monotheticism during assignment an idealistic goal rather than a pragmatic criterion. Even characteristics long thought to be absolutely essential for a natural type are often found to be lacking in specimens that clearly belong to that type. For example, possession of red blood corpuscles was considered a defining characteristic of all vertebrates until some species of fish were discovered to lack them (Sokal and Sneath, 1963:13).

What is the typologist to do in such a case? The fish clearly are vertebrates in spite of the missing corpuscles. He has no choice but to assign them to the vertebrate category, making this category polythetic. The virtual impossibility of finding any single feature that is alone adequate for identifying a naturally occurring group emphasizes the importance of polythetic typology in empirical investigation.

Because of the difficulty involved in simultaneous classification and assignment, sociologists have never been completely successful in constructing large classical typologies of the sort recommended by Stinchcombe. Instead, history has shown two extremes. Sociologists of Weber's era concentrated on classification without assignment. By limiting their work largely to monothetic heuristic typologies in the form of verbal charts and ideal and polar types, they were able to avoid the problems inherent in the assignment of natural groups.

Contemporary typologists have often chosen the opposite extreme. The advent of computerization has resulted in the development of many quantified typological methods. The most notable of these are the many clustering methods (see Sokal and Sneath, 1963; Ball, 1965; Friedman and Rubin, 1967). Such methods always yield empirical types formed through assignment without

prior conceptualization. These natural types are almost invariably polythetic.

Future work will surely emphasize the further development of quantified clustering techniques. There will also be increasing development and use of newer techniques such as *smallest space analysis* (Guttman, 1968); and multidimensional scaling (Coleman, 1971).

The interface between such new techniques is not always well understood by sociologists. In addition, there is a continuing need to clarify the relationship between new techniques and more traditional typological and reduction procedures. Of particular interest is the unexplored relationship between typologies and multidimensional scales. Sociologists can better understand the relationships among all methods, both old and new, if they keep the monothetic-polythetic and classification-assignment distinctions clearly in mind.

REFERENCES

- Bailey, Kenneth D.
1972 "Polythetic reduction of monothetic property-space: the approaches of Lazarsfeld and Guttman." Pp. 83-111 in Herbert L. Costner (ed.), *Sociological Methodology* 1972. San Francisco: Jossey-Bass.
- Ball, Geoffrey H.
1965 "Data analysis in the social sciences." Proceedings, Fall Joint Computer Conference 27:533-60.
- Beckner, Morton
1959 *The Biological Way of Thought*. New York: Columbia University Press.
- Blalock, Hubert M., Jr.
1968 "The measurement problem: a gap between the languages of theory and research." Pp. 5-27 in Hubert M. Blalock and Ann B. Blalock (eds.), *Methodology in Social Research*. New York: McGraw-Hill.
1969 *Theory Construction: From Verbal to Mathematical Formulations*. Englewood Cliffs, New Jersey: Prentice-Hall.
- Blau, Peter M. and W. Richard Scott
1962 *Formal Organizations*. San Francisco: Chandler Publishing Company.
- Bloombaum, Milton
1964 "A contribution to the theory of typology construction." *The Sociological Quarterly* 5 (Spring):157-62.
- Bruce, Grady D. and Robert E. Witt
1971 "Developing empirically derived city typologies: an application of cluster analysis." *The Sociological Quarterly* 12 (Spring): 238-46.
- Butler, Edgar W. and Stuart N. Adams
1966 "Typologies of delinquent girls: some alternative approaches." *Social Forces* 44 (March):401-07.
- Capecchi, Vittorio
1966 "Typologies in relation to mathematical models." *Ikon*, supplementary number 58 (September):1-62.
- Coleman, James S.
1971 "Clustering in N dimensions by use of a system of forces." *Journal of Mathematical Sociology* 1 (January):1-47.
- Fleiss, Joseph L. and Joseph Zubin
1969 "On the methods and theory of clustering." *Multivariate Behavioral Research* 4 (April):235-50.
- Friedman, H. P. and J. Rubin
1967 "On some invariant criteria for grouping data." *Journal of the American Statistical Association* 62 (December): 1159-78.
- Guttman, Louis
1950 "The basis for scalogram analysis." Pp. 60-90 in Samuel A. Stouffer, *et al.*, *Measurement and Prediction*. Princeton: Princeton University Press.
1968 "A general nonmetric technique for finding the smallest coordinate space for a configuration of points." *Psychometrika* 33 (December):469-506.
- Hempel, Carl G.
1952 "Typological methods in the natural and social sciences." Proceedings, American Philosophical Association, Eastern Division 1:65-86.
- Kretschmer, Ernst
1925 *Physique and Character*. New York: Harcourt, Brace, and Company.
- Lazarsfeld, Paul F.
1937 "Some remarks on the typological procedures in social research." *Zeitschrift für Sozialforschung* 6:119-39.
- Lopreato, Joseph and Letitia Alston
1970 "Ideal types and the idealization strategy." *American Sociological Review* 35 (February):88-96.
- Martindale, Don
1959 "Sociological theory and the ideal type." Pp. 57-91 in Llewellyn Gross (ed.), *Symposium on Sociological Theory*. New York: Harper and Row.
- McKinney, John C.
1966 *Constructive Typology and Social Theory*. New York: Appleton-Century-Crofts.
- McQuitty, Louis L.
1957 "Elementary linkage analysis for isolating orthogonal and oblique types and typal relevancies." *Educational and Psychological Measurement* 17 (Summer):207-29.
- Murphy, Gardner and Rensis Likert
1937 *Public Opinion and the Individual*. New York: Harper.
- Parsons, Talcott
1937 *The Structure of Social Action*. Glencoe: The Free Press.
- Phillips, Bernard S.
1971 *Social Research: Strategy and Tactics*. Second Edition. New York: Macmillan.

- Sokal, Robert R. and Peter H. A. Sneath
1963 *Principles of Numerical Taxonomy*. San Francisco: W. H. Freeman.
- Stephenson, William
1953 *The Study of Behavior: Q-Technique and its Methodology*. Chicago: University of Chicago Press.
- Stinchcombe, Arthur L.
1959 "Bureaucratic and craft administration of production: a comparative study." *Administrative Science Quarterly* 4 (September):168-87.
- 1968 *Constructing Social Theories*. New York: Harcourt, Brace, and World.
- Udy, Stanley, Jr.
1958 "'Bureaucratic' elements in organizations: some research findings." *American Sociological Review* 23 (August):415-18.
- 1959 "'Bureaucracy' and 'rationality' in Weber's organization theory: an empirical study." *American Sociological Review* 24 (December):791-95.
- Watkins, J. W. M.
1952 "Ideal types and historical explanation." *British Journal for the Philosophy of Science* 3 (May):22-43.
- Weber, Max
1949 *The Methodology of the Social Sciences*. Translated and edited by Edward A. Shils and Henry A. Finch. Glencoe: The Free Press.
- Winch, Robert F.
1947 "Heuristic and empirical typologies: a job for factor analysis." *American Sociological Review* 12 (February):68-75.

DO CHRISTIAN BELIEFS CAUSE ANTI-SEMITISM? *

RUSSELL MIDDLETON

*University of Wisconsin, Madison**American Sociological Review* 1973, Vol. 38 (February):33-52

I examine Glock and Stark's contention that certain Christian religious beliefs are causally related to anti-Semitism, using data from a 1964 national survey. Religious orthodoxy proves to be uncorrelated with anti-Semitism at the zero-order. A path analysis reveals that the relationships in the causal sequence hypothesized by Glock and Stark are weak. Furthermore, the influence of religious orthodoxy, religious libertarianism, religious particularism, and religious hostility to the historic Jew is not expressed solely through the intervening step of religious hostility to modern Jews; the coefficients for the direct paths to anti-Semitism are in some cases sizable. The five religious belief variables taken together in a simple additive model account for approximately 15 percent of the variance in anti-Semitism. When socioeconomic status, a number of other social attributes, and a number of social psychological traits are held constant, however, the five religious belief variables account uniquely for only 2 percent of the variance in anti-Semitism. Even here one must be cautious in inferring a causal relationship, particularly since some of the religious measures may simply reflect a more general anti-Semitic ideology. A revised model is presented which includes socioeconomic status and social psychological variables.

IN light of the strong emphasis on brotherhood in Christian ethics, it is a paradox that many studies have found that those Americans who on various indices are more religious also tend to be more prejudiced against racial and ethnic minorities. After three decades of research attempting to specify the precise relationship of religiosity to prejudice, however, few firm conclusions can be drawn. In this paper I shall not attempt to resolve all the questions concerning the relationship of different dimensions of

religiosity to different types of prejudice. Instead I wish to focus on one of the more intriguing and theoretically plausible questions: Do certain religious beliefs "cause" anti-Semitism among Christians in the United States today? More precisely, I shall examine the causal theoretical model set forth in Glock and Stark (1966), the most ambitious and comprehensive study of this question published to date.

PREVIOUS RESEARCH

Historians have made a convincing case that the Christian church was largely responsible for the development of anti-Semitism in ancient and medieval times (Parkes, 1934, 1962, 1963; Isaac, 1956, 1964; Si-

* I am indebted to Robert M. Hauser, Seymour Spilerman, Hal H. Winsborough, David Mechanic, and William H. Sewell, who read an earlier version of this paper and responded with many helpful comments and suggestions.

mon, 1948; Trachtenberg, 1943). The evidence suggests that economic and other social factors were of relatively minor significance, at least initially. It was the theological picture of the Jew created in the New Testament, in the patristic literature, and in subsequent teachings of the church that apparently unleashed the virulent hatred and persecution of Jews in the western world.

If we grant that certain teachings of the Christian church may have played a causal role in anti-Semitism in earlier times, is it true that religion still does so today? By and large empirical studies of the relationship of religiosity and anti-Semitism do show that those who are more religious tend to be more highly prejudiced against Jews, but the findings have not been completely consistent.

Several studies have found that those who have no religious affiliation are less anti-Semitic than those who are Catholic or those who belong to major Protestant groups (Sanford, 1950; Levinson and Sanford, 1944; Rosenblith, 1949). Bettelheim and Janowitz (1950) in a study of 150 male army veterans in Chicago found no significant difference between those with and without religious affiliations, but their sample contained only twelve of the latter. Blum and Mann (1960) found that college students who belonged to religious clubs were more anti-Semitic than those who did not.

With regard to church attendance the results are less consistent. Some studies have shown a positive association between church attendance and anti-Semitism (Simpson, 1959; Vanecko, 1966), some have shown no association or an irregular pattern (Pettigrew, 1959; Sanford, 1950; Selznick and Steinberg, 1969), and at least one (Parry, 1949) has found that nonchurch-going Protestants are more anti-Semitic than church-going Protestants. Williams (1964) found a curvilinear pattern in Savannah and Steubenville, with those who seldom attended church more anti-Semitic than those who never attended or often attended. Neuwien (1966:199) found the reverse curvilinear pattern in a national sample of Catholic school children. Those who attended mass most often and least often were most anti-Semitic.

Other studies have found that anti-Semitism is positively correlated with an extrinsic or instrumental orientation to religion (Wilson, 1960; Frenkel-Brunswick and Sanford, 1945; Vanecko, 1966), with acceptance of the customs and teachings of organized religion (Kelly, Ferson, and Holtzman, 1958), and with the tendency to attach high importance to religion and the church (Sanford, 1950). On the other hand, Evans (1952) and Jones (1958) both found a $-.14$ correlation between anti-Semitism and the Allport-Vernon scale on religious values, and Prothro and Jensen (1950) found a $-.11$ correlation between anti-Semitism and favorable attitudes toward the church on the Thurstone-Chave scale. Finally, Rosenblith (1949) found that South Dakota college students who reported that they had not been influenced by religion in their upbringing were more tolerant toward Jews than those who had been influenced by religion; whereas O'Reilly and O'Reilly (1954) found no significant relationship between the two variables for Catholic college students in a southern city.

Many of these studies suffer from serious methodological defects, such as relying on inadequate samples and failing to use adequate controls in the analysis. Considering these methodological defects and the diversity of the findings, one cannot regard any propositions about the relationship of various dimensions of religiosity to anti-Semitism as firmly established. Even if we assume that certain ideological dimensions of religiosity are positively correlated with anti-Semitism, the question still remains whether the relationship is causal or spurious. Probably the most common view is that it is spurious—that religiosity and prejudice are common consequences of some third factor or set of factors. Thus, Dittes (1969:633) suggests that education and class differences may account for much of the correlation between prejudice and religion. Others maintain that it is a particular cognitive style, authoritarianism, or other psychodynamic personality factors which lead an individual to be both anti-Semitic and highly religious (e.g., Argyle, 1958: 91-2; Allport, 1966:451; Levinson, 1967: 1012; Greeley, 1967:1009).

Although Allport explicitly rejected the

notion that religious beliefs lead directly to prejudice (Allport, 1962:132, 1966:451), he conceded that in the past there have been plentiful supports for bigotry in the particularism common to most theological systems. He believed that the doctrines of revelation, election, and theocracy have played a causal role in prejudice in the past and that the peril exists even today, though the "pathogenic elements" in theology are disappearing. Generally, however, he insisted that the average churchgoer has only "vague intimations of theology" and that it is "farfetched" to search for the roots of prejudice in their theological context (Allport, 1966:449-50). Bettelheim and Janowitz have also expressed the view that in modern times religious appeals as a basis for persecuting the Jews have generally fallen flat. In their own study of Chicago army veterans they found references to religion almost totally absent when individuals gave reasons for disliking Jews (Bettelheim and Janowitz, 1950:166).

On the other hand, Rose (1963:21-2) believed that ancient differences in theological conceptions profoundly influence contemporary images of Jews. He recalled that as a boy he was beaten up by some Catholic neighbor children on the sole excuse that "You killed Our Lord."

There is, in fact, some evidence that the "pathogenic elements" in Christian theology have not declined as much as Allport suggested. Several studies of the content of Sunday school materials and other publications of church groups in the United States, France, Belgium, Switzerland, and Canada have shown that Jews are often portrayed in a highly negative fashion (Olson, 1963; Eakin, 1935; Brown, 1957; Démann, 1952). It is true that many Christian groups have been working vigorously of late to revise their educational materials and eliminate those elements which might contribute to anti-Semitism. In the absence of recent systematic studies, however, we do not know how effective these efforts have been in changing the pattern of religious instruction.

THE GLOCK AND STARK POSITION

It is Glock and Stark in their book *Christian Beliefs and Anti-Semitism* (1966) who have argued most forcefully that certain theological beliefs of Christians play a causal role in anti-Semitism even today. Before

collecting or analyzing any empirical data, they constructed a complex theoretical model of the causal relations between different sets of religious beliefs and secular anti-Semitism (Glock and Stark, 1966: 94-5). They argue that orthodox religious faith leads Christians to take a particularistic view of their religious status, which in turn leads them to hold the historic Jew responsible for the crucifixion of Jesus. Religious hostility to the historic Jew breeds in them religious hostility toward the modern Jew. If the individual accepts norms of religious libertarianism, however, the causal sequence may be partly broken at any of these earlier steps. In the final link of the causal chain those who view the modern Jew as an unforgiven crucifier being punished by God also tend to develop secular anti-Semitic beliefs. Furthermore, Glock and Stark theorize that orthodoxy, particularism, religious hostility toward the historic Jew, and religious libertarianism do not have a direct effect on secular anti-Semitism. Their effect is expressed only indirectly through the intervening variable of religious hostility toward modern Jews. This latter point they regard as crucial to their main thesis (Glock and Stark, 1966:132).

To test their theoretical model Glock and Stark sent mail questionnaires in 1963 to a random sample of members of predominantly white Christian church congregations in four counties along the western side of San Francisco Bay in northern California. A 67 percent return rate yielded a final sample of 2,886. Glock and Stark constructed indices for measuring each variable in their model and then used percentage tables to show the relationships of these variables. Unfortunately, the choice of cross tabulations as the basis for analysis made it impossible for them to test fully their theoretical model, for if multivariate tables are constructed with more than a few variables at once, either the number of cases quickly becomes too small for the exponentially increasing cells of the table, or the mind begins to boggle at the resulting complexity.

Glock and Stark present many tables showing relationships which are consistent with parts of their theoretical model. For the most important part of the analysis, however, they use composite indices which combine several measures of individual vari-

ables. They show that religious dogmatism (a composite index of orthodoxy, particularism, implication of the historic Jew in the Crucifixion, and religious anti-libertarianism) is positively associated with religious hostility toward modern Jews and with secular anti-Semitism. They show also that religious hostility toward modern Jews is positively associated with secular anti-Semitism, even when religious dogmatism is controlled. Religious dogmatism, however, is not associated with secular anti-Semitism when religious hostility toward modern Jews is controlled. Finally, religious bigotry (a composite index of religious dogmatism and religious hostility toward modern Jews) is associated with anti-Semitic beliefs and feelings and with a propensity to engage in anti-Semitic actions. The association with anti-Semitic beliefs remains even when controls are introduced, one at a time, for education, occupation, income, size of community of upbringing, region of upbringing, age, political views, sex, and Protestant-Catholic affiliation. On the other hand, religious bigotry is only slightly associated with anti-black prejudice.

Glock and Stark recognized that the people on the West Coast are probably less religious and more libertarian than those in most other sections of the United States. They were, however, able to include some of their most central questions in a national survey of the American adult population carried out by the National Opinion Research Center in October, 1964. Hence, they were able to replicate some of their findings on a nationwide basis. Their basic finding from the national survey is that the index of religious bigotry—an index similar to that used for the Bay Area sample—is positively associated with secular anti-Semitism. This association persists even when controls for race, Protestant-Catholic affiliation, sex, education, and region are introduced singly or in combinations. Glock and Stark found that among those they classified as high or medium high on anti-Semitism, 23 percent also had scores of four or five on the index of religious bigotry, and another 21 percent had scores of three on the index. From this they drew the following conclusions:

Conservatively, these findings would suggest that at least one-fourth of America's anti-

Semites have a religious basis for their prejudice, while nearly another fifth have this religious basis in considerable part. . . . An impressive proportion, no less than a fourth of American anti-Semitism is attached to religious sources. . . . Far from being trivial, religious outlooks and religious images of the modern Jew seem to lie at the root of the anti-Semitism of millions of American adults (Glock and Stark, 1966:205).

Since its publication *Christian Beliefs and Anti-Semitism* has received much attention—both appreciative and critical—in professional, religious, and general periodicals (e.g., Williams, 1967; Greeley, 1967; Levinson, 1967; Dittes, 1967; Strommen, 1967; Hadden et al., 1966; Furfey, 1966; Monas, 1966). Methodological criticism directed at this and similar work in this area has been so common that Stark (1970:151) was moved to comment that in his “admittedly cynical view” relatively little attention would have been paid to methodological questions if the relationships had been reversed—that is, if Christian commitment had been shown to have only “positive” effects.¹

In a recent work Stark and Glock, joined by Foster and Quinley (Stark et al., 1971) have attempted to answer their critics and at the same time make a parallel analysis of data collected in 1968 from a sample of 1,580 parish clergymen from the nine largest Protestant denominations in California. The bulk of the analysis once again proceeds with percentage tables and uses a composite index at certain critical points. Comparing results from the clergy sample with those from the Bay Area sample, the authors conclude that although the clergy are less likely to be anti-Semitic than the laity, there is an even better fit between the data and the theoretical model for the clergy than for the laity (Stark et al., 1971:72).

Some earlier critics of *Christian Beliefs and Anti-Semitism* maintained that the use of composite indices in the analysis had glossed over the underlying complexity of the theoretical model and made it impossible to test whether the model was correct.

¹ Stark et al. (1971:2) also question the motives of the critics. Of course, a criticism may be correct or incorrect quite apart from the critic's motivation. In any case there is no religious motivation underlying my own work on this topic.

They even suggested (Levinson, 1967:1010-11; Dittes, 1967:184; Furfey, 1966:558) that Glock and Stark had stacked the deck in favor of finding a correlation between "religious bigotry" and secular anti-Semitism by including some anti-Jewish religious items in the index for bigotry. To deal with these objections Stark et al. (1971:80-4) carried out a path analysis of the data from the Bay Area and clergy samples. In both cases they found that religious hostility toward modern Jews was the only variable that had a sizable coefficient for a direct path leading to secular anti-Semitism. In both cases religious hostility toward modern Jews was determined by particularism, by orthodoxy directly, and by orthodoxy working indirectly through particularism. Among the clergy, however, particularism played a less important role.

Stark et al. (1971:82) conclude that the path analyses provide strong confirmation of their theoretical model as originally developed: "The main paths in which we took theoretical interest and which were sustained by the tabular analysis produce sizable coefficients. Those we rejected—for example, all linkages between the various factors and anti-Semitism, except through religious hostility—yield very small or non-existent coefficients." This conclusion may come as a surprise to someone who has taken their earlier model seriously. First, the authors in their more recent analysis have dropped entirely one of their six original variables, religious libertarianism. They do not explain why. They do not even mention the variable in their summary of the earlier book, despite the fact that religious libertarianism had a prominent place in the theoretical model and a whole chapter was devoted to it. Second, the model specifies a sequence of paths from one variable to the next. Yet neither path analysis sustains the theory. The direct path from orthodoxy to religious hostility toward modern Jews, for example, is greater than the theorized indirect path through particularism. Furthermore, the causal sequence is almost entirely disconnected at the next step, religious hostility toward the historic Jew. For the laity it is clear that this variable is not caused by particularism, nor does it in turn cause religious hostility toward modern Jews. For

the clergy the path coefficients are only slightly higher. Stark et al. (1971:82) show surprise at this finding but curiously do not seem to regard it as a major deviation from their theoretical model.²

In this paper I wish to provide a complete test of the original Glock and Stark theoretical model. I shall thus reintroduce the missing variable—religious libertarianism. I am also skeptical of their assertions that their findings are impervious to the influence of control variables, and I shall introduce a number of controls simultaneously, including some social psychological variables not used by Glock and Stark. Finally, I shall present a first step toward what I believe is a more reasonable path model linking these variables.

METHODS

The data I shall analyze come from a nationwide survey on anti-Semitism among adults in the United States carried out by NORC in October, 1964.³ Using the national sample rather than Glock and Stark's Bay Area sample has both advantages and disadvantages. The chief advantage, of course, is that it is a national sample and the results are not distorted by any peculiar regional characteristics. The national survey also contained questions on many social psychological variables not included in the Bay Area survey. On the other hand, the national survey did not have as many questions dealing with religious beliefs, and therefore it is

² Glock and Stark (1966:211) did hedge at one point in their earlier work, however. They suggested that religious hostility to modern Jews could be significantly muted if the decide issue could be laid to rest, but they believed some link would probably still remain between particularism and religious hostility toward modern Jews because Jews remained outside the "true" faith.

³ The study was sponsored by the Anti-Defamation League of B'nai B'rith and was directed by the Survey Research Center, University of California, Berkeley. Sampling and interviewing were carried out by the National Opinion Research Center, University of Chicago. The interview schedule, details about sampling procedures, and basic results are published in Selznick and Steinberg (1969). I am indebted to the International Data Library and Reference Service of the University of California, Berkeley, and the Social Science Data and Program Library Service of the University of Wisconsin, Madison, for making the data available to me.

not possible to build indices or scales exactly equivalent to those constructed by Glock and Stark from the Bay Area data. There was enough correspondence between the two surveys, however, for Glock and Stark to consider the national data as basis enough for replicating their Bay Area findings, and one chapter of their book is devoted to an analysis of the national data. Stark et al. (1971), however, did not carry out a path analysis with the national data when they reanalyzed the Bay Area data.

For the national NORC survey probability methods were used in the sample selection down to the block level within some seventy-three primary sampling units. Within blocks interviewers followed a prescribed travel route from a random start to fill age, sex, and employment quotas. This deviation from strict probability procedures at the block level probably did not introduce a serious bias, since a comparison with the results of the 1964 Population Survey of the Bureau of the Census shows that the two samples had very similar distributions of key characteristics—education, income, race, age, and sex. Nevertheless, one should keep in mind this limitation, particularly when interpreting the results of tests of statistical significance.⁴

For the national survey NORC completed 1,976 interviews. In this paper I shall analyze the data from the 1,852 individuals who identified themselves as Protestants or Catholics.

I shall use the techniques of correlation, multiple regression, and path analysis to test the Glock and Stark theory. Using these techniques rather than cross tabulations does

raise certain measurement problems. The techniques do not permit missing observations, and some procedure must be used to supply the missing data. I have followed the essentially conservative procedure of substituting the integer value nearest the mean for each variable.⁵ Correlation and regression analysis also assume an interval level of measurement of variables—an assumption which can rarely be met for social psychological scales. I believe, however, that at our present level of methodological development the advantages of using correlation and regression techniques ordinarily greatly outweigh the pitfalls, even when one's measures do not constitute true interval scales.⁶

⁵ Glock and Stark (1966:249) say that they "entirely avoided making assumptions about how respondents might have answered" by simply excluding from the analysis all persons who failed to answer all the questions for a given index. They state that none of the evidence they examined suggested that this exclusion of cases biased the results, but it is by no means clear that reducing the representativeness of the sample in this way is less biasing than substituting sample means or medians for missing data. Furthermore, the attrition of cases might become unacceptably high when one goes beyond bivariate analysis to the simultaneous analysis of a number of variables. It is unlikely that the procedure I followed created much bias, since the mean percent of missing data for the questions used in the analysis for this paper was only 0.4 percent. I also constructed a 0-1 dummy variable to indicate whether or not data were missing for each item comprising the anti-Semitism and religious belief measures. Then all of the original items and the missing data dummy variables were factor analyzed together. The missing data dummy variables had loadings near zero for each of the factors represented by the original items, and their own high loadings tended to be scattered among seven other factors. This suggests, then, that there was little systematic bias in the nonresponse for these central variables. For a discussion of the effect of nonresponse on correlation results see Blau and Duncan (1967:Appendix F).

⁶ A number of authors have expressed some skepticism about the propriety of using statistics which assume interval measurement in such cases (Mayer, 1970, 1971; Hawkes, 1971; Vargo, 1971; Schweitzer and Schweitzer, 1971; Wilson, 1971). Other authors, however, have argued that these statistics tend to be better developed and are more powerful, sensitive, versatile, and interpretable than rank order or other nonparametric statistics; whereas the latter are not without dubious assumptions themselves (Abelson and Tukey, 1959; Borgatta, 1962, 1968; Labovitz, 1967, 1970, 1971; Boyle, 1970). Labovitz (1970) has found that changes in scale tend to have little effect on correlation coefficients except in extreme cases.

⁴ Because the sample was not drawn according to strict probability methods at the block level, Selznick and Steinberg (1969:xx) chose not to use tests of significance. In this study I shall report whether relationships are significant beyond the .05 level and shall leave it to the reader to decide whether tests of significance are meaningful in this instance. For a justification of the NORC quota sampling procedures see Sudman (1966). Sudman reports that sex, age, and employment status are reasonable predictors of availability for interviews. Hence, although the procedure is not unbiased, typically the bias is small, on the order of 3 to 5 percent. He concludes that it is reasonable to treat quota sampling in which the interviewer must follow a prescribed travel plan as a form of probability sampling.

To measure the basic dependent variable, secular anti-Semitism, I use the eleven-item anti-Semitism scale of Selznick and Steinberg (1966:22). The scale measures acceptance of certain traditional stereotyped beliefs about Jews. This scale taps only the cognitive elements of anti-Semitism and not the affective and conative, but this is probably not a major shortcoming in light of the findings of Woodmansee and Cook (1967) that cognitive, affective, and conative elements do not emerge as separate factors in prejudice, as has been commonly supposed. The Selznick and Steinberg anti-Semitism scale is similar to the six-item index used by Glock and Stark in analyzing the national data and includes three of their six items. I believe the Selznick and Steinberg measure is preferable, however, since it was derived from a factor analysis, and I find that it has a relatively high coefficient of reliability (Cronbach's $\alpha = .83$). The chief defect of the Selznick and Steinberg scale is that most of the items are worded in the same direction, so that agreement indicates anti-Semitism. The scale is thus probably contaminated by acquiescence response bias, but this is also a problem with the Glock and Stark anti-Semitism indices. Since acquiescence bias generally tends to be highest among those with little education, it becomes particularly important to control for socioeconomic status variables when examining the relationship of religious beliefs to anti-Semitism.

The independent and control variables I have used in this study are shown in Table 1. The specific interview questions from

Boyle (1970) has also employed a dummy variable technique in multiple regression analysis to assess the effects of assuming equal intervals in measurement and has concluded that the empirical dangers are not great. In the present study I used a similar approach. I calculated the multiple correlations of the eleven individual questions coded 0-1 which comprise the anti-Semitism scale with a series of dependent variables. I then compared the multiple correlations of the anti-Semitism questions with the correlations obtained with the anti-Semitism scale for the same dependent variables. The same procedure was followed with the questions comprising the measure of religious libertarianism. A comparison of the results of these two approaches shows a mean difference of only .029 between r and R and .011 between r^2 and R^2 . Hence, it seems unlikely that the assumption of equal intervals seriously distorts the results of the present study.

which the measures were constructed are identified in the second column, and the coefficients of reliability (Cronbach's α) for the multi-item scales are shown in the third column.⁷ Some of the coefficients are

⁷ The scale scores are sums of the individual item scores coded as follows. For all the measures "not ascertained" and "refusal" were coded with the integer nearest the mean value.

Anti-Semitism:

Each item: 1 if belief is accepted; 0 if not accepted or DK (don't know)

Religious orthodoxy:

Q53: 0 for replies 1, 2, 3, DK; 2 for replies 4, 5; 4 for reply 6

Q55, Q56: 0 for reply 3 and "There is no Devil, but there is evil"; 1 for reply 4; 2 for DK; 3 for reply 2; 4 for reply 1

Religious particularism:

Q54: 1 if no; 0 if yes, other, DK

Q60: 1 if Christians, Protestants, or Catholics; otherwise 0

Religious libertarianism:

Q7: 0 for reply 1 in Q7A or Q7B; 1 for reply 2 or DK

Q63A, Q63B: 0 if no; 1 if yes or DK

Q63C: 0 if yes; 1 if no or DK

Religious hostility to historic Jews: 1 for the Jews; otherwise 0

Religious hostility to modern Jews: 1 if yes; 0 if no or DK

Anomia:

Each item: 1 if agree; 0 if disagree or DK

Authoritarianism:

Each item: 1 if agree; 0 if disagree or DK

Anti-black prejudice:

Q10: 1 if yes; otherwise 0

Q13P, Q13Q: 1 if agree; 0 if disagree or DK

Q15: 1 if separate but equal schools; otherwise 0

Q16: 1 if yes; 0 if no or DK

Ethnocentrism:

Each item: 1 if agree; 0 if disagree or DK

Faith in people:

Q51-0: 1 if disagree; 0 if agree or DK

Q69: 1 if yes; 0 if no or DK

Anti-Catholic prejudice: 1 if agree; 0 if disagree or DK

Anti-poor prejudice:

Each item: 1 if agree; 0 if disagree or DK

Political conservatism:

Q9A: 1 if strongly approve or approve somewhat; otherwise 0

Q13A, Q13E: 1 if disagree; 0 if agree or DK

Q13B, Q13F, Q13G, Q13H: 1 if agree; 0 if disagree or DK

Psychic inadequacy:

Q68A, Q68D, Q68J, Q68K: 1 if yes; 0 if no or DK

Q68B: 1 if no; 0 if yes or DK

Q75: 1 if not too happy; otherwise 0

Cultural sophistication:

Each item: 1 if correctly identified as a writer; otherwise 0

low, particularly for the two- and three-item scales, but it was not possible to build more acceptable scales for these variables from the specific questions included in the survey.

To measure the religious belief variables I have with one exception used the same items as Glock and Stark in their analysis of the national data. To measure religious libertarianism Glock and Stark used two questions about willingness to permit atheists to hold public office and teach in the public schools. I have used these two items plus two others which ask whether an atheist should be barred from the Presidency and whether a book written by an atheist should be removed from a public library. For a multiple regression approach to analysis the composite measures of religious dogmatism and religious bigotry are not necessary, but I have included these variables for purposes of comparison with Glock and Stark's results. Religious dogmatism is a composite of four variables equally weighted: religious orthodoxy, religious particularism, religious anti-libertarianism, and religious hostility to the historic Jew. Religious bigotry is a composite of these four variables plus religious hostility to the modern Jew, all equally weighted. Unfortunately, two of the religious variables—religious hostility to the historic Jew and religious hostility to the modern Jew—are measured only by single dichotomous items. This means that the variance of these two measures is sharply curtailed, and the reliability of the measures is unknown.

For control variables I selected a number of measures (variables 8–28 in Table 1) from those significantly correlated with both secular anti-Semitism and at least one religious belief variable. The control variables include measures of socioeconomic status, other social attributes, and various social psychological traits. Occupation of head of household, region of upbringing, and marital status are treated as dummy variables in the regression analysis.⁸

The social psychological scales are derived for the most part from a factor analysis using a uniqueness rescaling procedure, which factors the correlation matrix after rescaling

with estimates of uniqueness.⁹ Both varimax and quartimax rotations were examined. Most of the scales are factorially distinct and are composed of items which have high loadings on a single factor. I was not able to satisfy this criterion fully in all cases, however. For example, the five authoritarianism items have relatively high loadings on two or perhaps three different factors. This is consistent with previous research which has shown that the F-scale is not unidimensional (Christie and Garcia, 1951; Camilleri, 1959; Lutterman and Middleton, 1970). This finding is not surprising, since the original scale (Adorno et al., 1950) was designed to measure some nine different characteristics or dimensions which theoretically comprise the authoritarian syndrome. The five F-scale items used in this survey were taken from Forms 40 or 45 of the original scale—two in revised form—and represent five of the nine original dimensional clusters.

Some of the authoritarianism items also have high loadings on the same factor as the three anomia items taken from the Srole (1956) scale. Because of the long research tradition associated with these two measures, however, I have chosen to construct them in the traditional way and to treat them as separate measures. Selznick and Steinberg (1969:156–69) argue that the F-scale, and to a lesser extent the anomia scale, measure primarily an unenlightened, unsophisticated cognitive style rather than some deeper underlying personality factor. To support their argument they cite the inverse correlation between education and scores on the F-scale and the anomia scale. For part of their analysis they even combine the F-scale items with some other items which reflect a kind of anti-intellectualism to form an index of simplism. In my own factor analysis, however, I found that the authoritarianism items had a different factor structure than the other simplism items. I believe, furthermore, that Selznick and Steinberg overemphasize the extent to which the F and anomia scales simply reflect educational differences. In analyzing the same data I find that the zero-order correlation between the F-scale and anti-Semitism is .378. With education controlled, the par-

⁸ For a discussion of the use of the dummy variable technique in multiple regression analysis see Suits (1957) and Cohen (1968).

⁹ For a description of the procedure see Wisconsin, University of (1969).

tial correlation is .315; with education, occupation, income, and cultural sophistication (ability to identify four famous authors) controlled, the partial correlation is still .302. The zero-order correlation between anomia and anti-Semitism is .387, and the additional controls reduce it only to .319.¹⁰ It is thus clear that the F and anomia scales do have some relationship to anti-Semitism quite independent of education or cognitive sophistication.

FINDINGS

Table 1 shows the zero-order correlations of the religious belief measures and secular anti-Semitism for the national sample. The most striking finding is the lack of any correlation between religious orthodoxy and secular anti-Semitism. Glock and Stark argue (1966:209) that orthodoxy is the source of the whole syndrome and that "clearly, without its orthodox base, the whole structure would collapse." The evidence from the national sample, however, shows that there is no basis for anti-Semitism in orthodoxy. It is true that orthodoxy is correlated with the other religious variables in the way that Glock and Stark predicted, though the coefficients are not high. The total of direct

and indirect effects of orthodoxy on anti-Semitism, however, is zero. In *Christian Beliefs and Anti-Semitism* Glock and Stark present no evidence on the relationship between orthodoxy and secular anti-Semitism, but Stark et al. (1971:127-8) report correlations of .075 for the Bay Area sample and .225 for the clergy. Thus, religious orthodoxy accounts for none of the variance in secular anti-Semitism for the national sample, less than 1 percent for the Bay Area sample of church members, and 5 percent for the sample of Protestant ministers in California. This seems a shaky base on which to build a whole causal sequence.

The zero-order correlations of the other religious belief measures with anti-Semitism are relatively small. The largest is the -.32 correlation between religious libertarianism and anti-Semitism, which accounts for approximately 10 percent of the variance. The composite measures, religious dogmatism and religious bigotry, do less well, accounting for 7 and 9 percent. It is obvious from the data in Table 1 that combining the individual religious belief variables to form the composite measures tends to hide much of the complexity of the data which have a direct bearing on Glock and Stark's theory.

As I suggested earlier, several critics have argued that Glock and Stark guarantee a correlation between religious bigotry and

¹⁰ $P < .05$ for all coefficients mentioned in this paragraph.

Table 1. Correlations of Anti-Semitism, Religious Variables, and Other Variables, for U.S. Protestants and Catholics.

Variables	Question Numbers**	Reli- abi- lity***	Anti- Semi- tism	Ortho- doxy	Parti- cular- ism	Liber- tarian- ism	Histo- ric Jews	Mod- ern Jews	Rel. Dogma- tism	Rel. Bigo- try
1. Religious orthodoxy	53, 55-56	.67	.00	----	.37	-.29	.10	.20	.56	.53
2. Religious particularism	54, 60	.37	.17	.37	----	-.36	.09	.22	.67	.64
3. Religious libertarianism	7, 63A-C	.75	-.32	-.29	-.36	----	-.10	-.23	-.66	-.63
4. Religious hostility to historic Jews	61		.14	.10	.09	-.10	----	.13	.63	.57
5. Religious hostility to modern Jews	62		.21	.20	.22	-.23	.13	----	.30	.61
6. Religious dogmatism			.27	.56	.67	-.66	.63	.30	----	.94
7. Religious bigotry			.30	.53	.64	-.63	.57	.61	.94	----
8. Education	70		-.27	-.09	-.17	.34	-.06	-.18	-.26	-.28
9. Family income	93		-.15	-.13	-.22	.26	-.05	-.13	-.25	-.25
10. Occupation head of household (dummy)	72A, 86A		.18*	.12*	.18*	.22*	.08*	.14*	.19*	.20*
11. Age	76		.23	-.02	.02	-.27	.09	.10	.16	.17
12. Size community reared	79		-.12	-.13	-.27	.24	-.03	-.10	-.25	-.25
13. Region in which reared or foreign-born (dummy)	78, 78A		.17*	.19*	.31*	.27*	.05*	.11*	.25*	.25*
14. Years lived in community	90		-.10	-.02	-.02	.12	-.03	-.04	-.07	-.08
15. Female=1, male=0	A-1		.11	.19	.09	-.12	-.01	-.01	.12	.10
16. Black=1, nonblack=0	A-2		.07	.07	.25	-.14	.01	.05	.18	.17
17. Organization member	50		-.07	-.03	-.04	.08	-.02	-.05	-.06	-.07
18. Marital status (dummy)	84		.08*	.05*	.07*	.14*	.02*	.03*	.09*	.09*
19. Anomia	51J-K, 51M	.54	.39	.03	.20	-.31	.10	.21	.26	.29
20. Authoritarianism	51A-E	.52	.38	.06	.12	-.34	.09	.18	.24	.27
21. Anti-black prejudice	10, 13P-Q, 15-16	.72	.36	.01	.08	-.32	.05	.14	.18	.20
22. Ethnocentrism	13L-M	.44	.32	.10	.14	-.37	.07	.13	.27	.27
23. Faith in people	51-0, 69	.40	-.30	.02	-.18	.23	-.06	-.17	-.18	-.21
24. Anti-Catholic prejudice	131		.28	.04	.13	-.22	.03	.18	.16	.20
25. Anti-poor prejudice	13C-D	.41	.18	-.03	.02	-.10	.01	.09	.04	.07
26. Political conservatism	9A, 13A-B, 13E-H	.68		.15	.01	.00	.06	.12	.05	.09
27. Psychic inadequacy	68A-B, 68D, 68J-K, 75	.58	.13	.03	.10	-.15	.01	.06	.11	.12
28. Cultural sophistication	17B, 17F, 17J, 17L	.71	-.24	-.09	-.20	.35	-.07	-.17	-.27	-.29

*Multiple R for dummy variable. Note that the sign of R is positive by definition. All coefficients without an asterisk are zero-order Pearsonian product-moment correlations. Where r is greater than .04, $p < .05$; where R is greater than .08, $p < .05$.

** Question numbers in interview schedule reproduced in Selznick and Steinberg (1966:Appendix A). ***Cronbach's α

secular anti-Semitism by including some anti-Jewish items—albeit in a religious context—in the index of religious bigotry. Partly to examine this argument and partly to determine whether the religious belief measures represent independent dimensions of religious belief, I submitted all of the items from the anti-Semitism scale and the religious belief measures to a factor analysis (uniqueness rescaling procedure with a varimax rotation). Items from the anti-Semitism scale have high loadings primarily on Factor I; those from the religious libertarianism scale have high loadings primarily on Factor II. Those two dimensions appear relatively independent of the others. Religious orthodoxy and religious particularism, however, are not independent of each other and have their highest loadings on the same factor. Religious hostility toward the historic Jew does not have a high loading on any factor, but the highest is on a relatively independent factor. Religious hostility toward the modern Jew has no high loadings, but its highest is on the “orthodoxy-particularism” factor. Apart from religious libertarianism, then, the various types of religious beliefs are not clearly differentiated. This result is generally consistent with most factor analytic studies of religious beliefs which tend to find one global, undifferentiated religiosity factor in heterogeneous, religiously unsophisticated populations (Dittes, 1969:610).

Among the religious belief questions, those tapping religious hostility to historic and modern Jews are clearly anti-Jewish; and the religious particularism question might be interpreted as anti-Jewish. Yet none of these items has a high loading on Factor I, the “secular anti-Semitism” factor. It appears that religious anti-Semitism and secular anti-Semitism are not more or less interchangeable measures of the same underlying phenomenon, and it is legitimate to examine whether religious and secular anti-Semitism are related. The problem lies in interpreting the meaning of the relationship and in considering how the other religious variables function in the causal process.

In connection with this controversy Levinson (1967:1010–11) maintains that the principal conclusion to be drawn from Glock and Stark's most important table (Table 50) is that religious anti-Semitism and secular anti-

Semitism are highly interrelated even with religious dogmatism held constant. On the basis of the limited data for the Bay area sample presented in the table he “predicts” that the correlation between religious hostility toward the modern Jew and secular anti-Semitism is about .6 and that if religious dogmatism were controlled, the partial correlation between religious and secular anti-Semitism would remain near .6. Thus, he argues against a three-step causal sequence and for the view that both religious and secular anti-Semitism simply reflect a more generalized anti-Semitic ideology.

My analysis of the national data shows a different pattern. There is a correlation of .21 between religious hostility toward the modern Jew and secular anti-Semitism, and this is reduced to .15 with religious dogmatism held constant.¹¹ On the other hand, the correlation between religious dogmatism and religious hostility toward modern Jews is reduced from .30 to .25 with secular anti-Semitism held constant; whereas Levinson expected that the partial correlation in this instance would be near zero. Similarly, the correlation between religious dogmatism and secular anti-Semitism is reduced from .27 to .22 with religious hostility toward modern Jews held constant; whereas Levinson expected here as well that the partial correlation would be near zero. It is true that the data from the Bay Area sample appear to conform more closely to Levinson's predictions, though even there Levinson is probably wrong in believing that religious hostility to modern Jews is more highly correlated with secular anti-Semitism than with religious dogmatism. The national data, however, do not conform fully to the predictions of either Levinson or Glock and Stark. Clearly, a more sophisticated test of the causal theory is necessary—one which examines the interrelationships of all the variables in the theory and which does not depend on composite measures.

The Glock and Stark theory appears to be a recursive model, and we may use path analysis to test its adequacy. The only difficulty that arises in translating the theory into path analytic terms concerns the role of religious libertarianism in the causal model.

¹¹ $p < .05$ for all coefficients mentioned in this paragraph.

Glock and Stark state that religious libertarianism "intervenes" at three steps in the causal sequence, partly "disconnecting" the causal relationship between orthodoxy and particularism, between particularism and religious hostility toward the historic Jew, and between religious hostility toward the historic Jew and religious hostility toward the modern Jew. From their language, however, it is not clear whether they believe that religious libertarianism has an interactive or additive effect.

I sought first to find whether religious libertarianism did in fact have an interactive effect. I used two approaches. First, I divided the sample into two groups—those above the median in religious libertarianism and those below. I then carried out separate path analyses for each group, including all variables in Figure 1 except religious libertarianism. If the interaction model is correct and libertarianism partly "disconnects" the three causal relationships in question, the path coefficients for the hypothesized causal paths should be smaller for those who are more libertarian. In each case, however, they were slightly greater for those high in libertarianism than for those low.

Second, I tested a large number of interaction models through multiple regression analysis, examining whether the addition of interaction terms increased the size of multiple R . I tested both complete interaction models and partial models with interaction effects only for high scores in libertarianism. These models were tested with the variables in their original form and also with them transformed into dummy variables. The interaction effects for religious libertarianism, however, proved almost nonexistent. In no instance did the addition of interaction terms lead to an increase in R of more than .01, even for a cumulative interaction model that included the interaction effects of religious libertarianism with all three of the other variables hypothesized to cause religious hostility toward the modern Jew.

Since the interaction model for religious libertarianism is not supported empirically and since some of the other language of Glock and Stark (1966:96) suggests an additive model, I chose to construct a path model which gives religious libertarianism merely an additive role as the best represen-

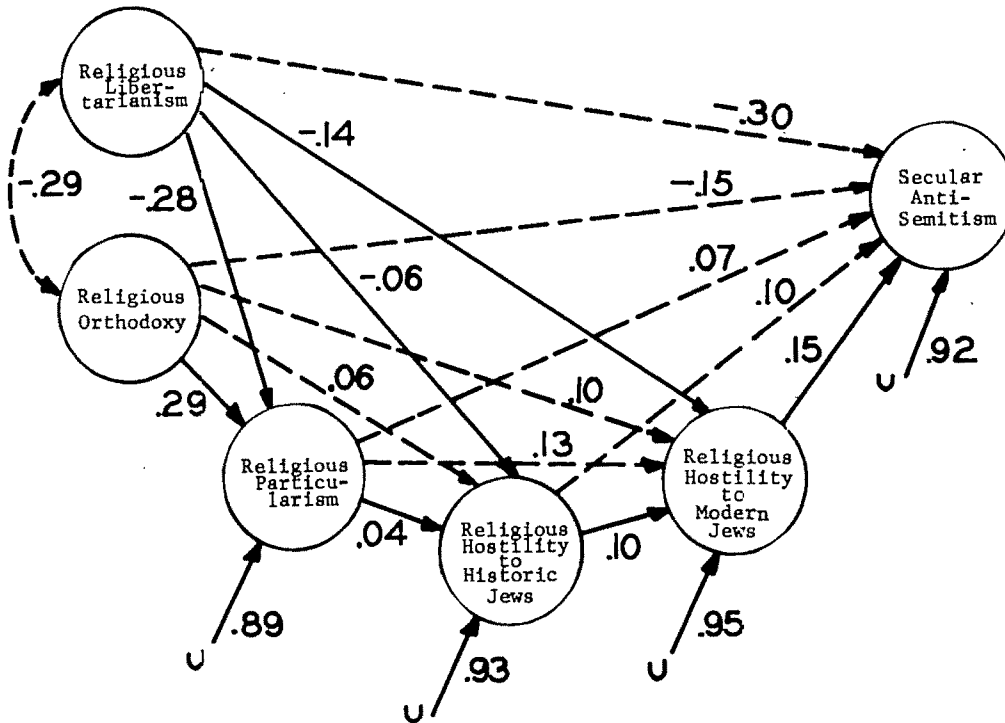
tation of the Glock and Stark theory. This path model is presented in Figure 1, with the causal paths hypothesized by Glock and Stark represented by solid lines.

The first thing we may note in the path diagram is that the path coefficients (standardized regression coefficients) tend to be small for the links in Glock and Stark's hypothesized chain. Religious orthodoxy and religious libertarianism are related in the expected way to religious particularism, but the causal sequence is almost completely disconnected at the next step, for the path coefficient between particularism and religious hostility toward the historic Jew is only .04. The path coefficients for the next two steps leading to religious hostility to modern Jews and finally to secular anti-Semitism are slightly higher but still not great.

Glock and Stark argue that the first four religious belief variables do not affect secular anti-Semitism directly but only indirectly through religious hostility to modern Jews. If they are correct, the path coefficients for the broken lines leading to secular anti-Semitism should all be zero. This does not prove to be the case. None of the coefficients for a direct path is zero. In fact, one is double the magnitude of the path coefficient between religious hostility to modern Jews and secular anti-Semitism. Another is equal in size. An additive model including religious libertarianism, orthodoxy, particularism, and religious hostility to historic Jews accounts for 9.3 percent of the variance in religious hostility to modern Jews. Religious hostility to modern Jews in turn accounts for only 4.6 percent of the variance in secular anti-Semitism. An additive model which includes all five antecedent religious variables, however, accounts for 15.1 percent of the variance in secular anti-Semitism. Clearly, then, Glock and Stark's hypothesis of a particular causal sequence is not supported by the data from the national sample.

The role of religious orthodoxy in the causal process is particularly interesting, since it diverges most widely from Glock and Stark's predictions. The path coefficients for the paths leading from orthodoxy to particularism, religious hostility to historic Jews, and religious hostility to modern Jews are positive, as Glock and Stark expected. The coefficient for the direct path from

FIGURE 1. PATH MODEL FOR ANTI-SEMITISM AND ANTECEDENT RELIGIOUS BELIEFS *



CAUSAL PATHS HYPOTHESIZED BY GLOCK AND STARK: ———

OTHER PATHS: - - - - -

* $P < .05$ FOR ALL COEFFICIENTS GREATER THAN $.04$

religious orthodoxy to secular anti-Semitism, however, is negative. This means that if one controls on each of the other religious variables, a person who tends to be high in orthodoxy is *less* likely to be highly anti-Semitic. The direct and indirect effects of orthodoxy cancel each other out, and, as we saw earlier, there is no correlation between orthodoxy and secular anti-Semitism at the zero-order. One might be tempted to speculate that the direct path in this case reflects an "intrinsic religiosity" component of orthodoxy, whereas the indirect paths reflect an "extrinsic religiosity" component. As we shall see later, however, much of the direct negative relationship between orthodoxy and secular anti-Semitism disappears when other factors are controlled as well.

One might argue that the path analysis shown in Figure 1 is distorted by the unreliability of the measures of the variables. Random error attenuates the relationship between variables, and hence the size of the true correlations may be masked when scales have low coefficients of reliability (cf. Bohrnstedt, 1970:84-5). I therefore tried to determine whether a path analysis would more likely support Glock and Stark's theory after the correlation coefficients were corrected for attenuation. A problem arises, however, in that the reliabilities of two of the measures—religious hostility to historic Jews and religious hostility to modern Jews—are unknown. In carrying out the corrected path analysis I first assumed that the reliabilities for these two variables were 1.00. The results,

I found, were even more strongly against Glock and Stark's theory. The coefficients for the direct paths from religious libertarianism and religious hostility to historic Jews to secular anti-Semitism were almost unchanged; the coefficient for the direct path from religious orthodoxy to secular anti-Semitism was substantially increased in a negative direction to $-.41$; and the coefficient for the direct path from religious particularism to secular anti-Semitism was also substantially increased to $.34$. On the other hand, for the one hypothesized direct causal path from religious hostility to modern Jews to secular anti-Semitism the coefficient showed a slight decrease to $.12$. For the other six hypothesized causal paths, the coefficients increased in three cases and decreased in three.

I then carried out additional path analyses varying the size of the assumed reliability coefficients for religious hostility to historic Jews and religious hostility to modern Jews. With a reliability as low as $.30$ for the two variables the results were still contrary to Glock and Stark's theory. In this case the coefficient for the hypothesized direct path between religious hostility to modern Jews and secular anti-Semitism increased slightly to $.18$, but this figure was the *lowest* of all the coefficients for the direct paths to anti-Semitism. With reliabilities at levels intermediate between $.30$ and 1.00 the results of the corrected path analyses were no more supportive of the Glock and Stark theory.

Though the specific Glock and Stark theory is not supported by the data from the national survey, the fact remains that four of the five religious belief variables

they identified are related to secular anti-Semitism and together account for over 15 percent of the variance in anti-Semitism. So far, however, we have worked only with the religious belief measures as independent variables. We must still consider whether the relationships are spurious. Glock and Stark tried to deal with this question by examining the relationship of the composite measure of religious bigotry to secular anti-Semitism while controlling on several other variables, for the most part one or two at a time. They concluded that the controls did not alter the original relationships. For the national data, however, I found that when I controlled simultaneously on the same variables Glock and Stark used as controls for the Bay Area sample, the correlation between religious bigotry and anti-Semitism was reduced from $.303$ to $.229$. Thus, the percent of the variance explained by religious bigotry was reduced from 9.2 to 5.2 . Glock and Stark did not control for any social psychological variables, but even without them the percent of the variance in anti-Semitism explained by religious belief is considerably reduced.

Table 2 presents the standardized regression coefficients in the regression of anti-Semitism on each religious belief variable in turn, with controls on a broader range of other variables. I divided the control variables into three blocks to examine the effects of controlling for the blocks separately as well as together. The zero-order r and r^2 are shown in the first two columns, and β and β^2 after controlling for the block of socioeconomic status variables (education, family income, and occupation of head of

Table 2. Standardized Regression Coefficients in the Regression of Anti-Semitism on Measures of Religious Beliefs, with Selected Controls.*

Religious Beliefs	Zero-Order Correlations		Control Variables							
	r	r^2	Socioeconomic Status (V8-10)**		Other Social Attributes (V11-18)**		Social Psychological Traits (V19-28)**		All Control Variables (V8-28)**	
			Beta	Beta ²	Beta	Beta ²	Beta	Beta ²	Beta	Beta ²
Religious orthodoxy	.004	.000	-.024	.001	.013	.000	-.024	.001	.013	.000
Religious particularism	.167	.028	.127	.016	.160	.026	.056	.003	.081	.007
Religious libertarianism	-.323	.104	-.257	.066	-.297	.088	-.091	.008	-.107	.011
Religious hostility to historic Jews	.144	.021	.127	.016	.118	.014	.086	.007	.076	.006
Religious hostility to modern Jews	.214	.046	.171	.029	.176	.031	.072	.005	.067	.004
Religious dogmatism	.272	.074	.217	.047	.250	.062	.101	.010	.124	.015
Religious bigotry	.303	.092	.249	.062	.277	.077	.116	.013	.133	.018

* $P < .05$ for all coefficients except those for religious orthodoxy

**Refers to variable numbers given in Table 1

household) are shown in the third and fourth columns. Observe that for most of the religious belief measures the coefficients are substantially reduced. After controlling for socioeconomic status none of the religious variables accounts directly for more than 6.6 percent of the variance in secular anti-Semitism.

The results of controlling for other social attributes (age, size of community in which reared, region in which reared, years lived in community, sex, race, organization memberships, marital status) are presented in the fifth and sixth columns. Again the size of the coefficients is reduced, though generally not as much as when socioeconomic status is controlled.

The first two blocks of control variables include many that Glock and Stark themselves used. They did not use any of the third block of social psychological variables, however. In fact, a number of social scientists (e.g., Greeley, 1967:1009; Levinson, 1967:1012-13; Dittes, 1967:187) criticized them for failing to consider the possibility that religious beliefs and secular anti-Semitism may both be caused by a particular cognitive style or by certain attitudes or personality characteristics. Thus, in testing the relationship of religious beliefs to anti-Semitism for spuriousness, Glock and Stark introduced no social psychological control variables, though data on anomia and political conservatism were available in their Bay Area survey and data on authoritarianism, anomia, political conservatism, and numerous other social psychological traits were available in the national survey.

Stark (1971) has sought to defend the earlier study by analyzing further the data for northern whites from the 1964 national survey. He shows that authoritarianism, "neurotic distrust" (a measure similar to my "faith in people" variable), and psychic inadequacy are either unrelated or negatively related to religious orthodoxy and church attendance. He concludes that Levinson and other critics were wrong in suggesting that both religious beliefs and anti-Semitism may be caused by some other social psychological factor. Actually Stark's arguments are irrelevant. It is true that authoritarianism, faith in people, and psychic inadequacy are unrelated or weakly related to orthodoxy

and church attendance, but it is also true that orthodoxy and church attendance are virtually unrelated to anti-Semitism. Authoritarianism, faith in people, and psychic inadequacy are much more strongly related to the other religious belief variables. These, of course, are precisely the ones most strongly related to secular anti-Semitism.

The results of controlling for a number of social psychological traits (anomia, authoritarianism, anti-black prejudice, ethnocentrism, faith in people, anti-Catholic prejudice, anti-poor prejudice, political conservatism, psychic inadequacy, cultural sophistication) are shown in the seventh and eighth columns of Table 2. None of the religious variables accounts directly for more than 1.3 percent of the variance in anti-Semitism when this block of control variables is introduced.

If one can argue that the specific religious beliefs in question have a causal effect on the social psychological traits, of course, it becomes more questionable whether one can legitimately control for the latter when examining the relationship between the religious beliefs and anti-Semitism. Elsewhere Stark and Glock (1969:79-84) pursue this type of argument for anti-black prejudice. They maintain that religious commitment among Christians tends to engender a free-will conception of man, which in turn leads Christians to view blacks as largely responsible for their own situation and makes Christians unsympathetic to the civil rights movement. They argue, however, that Christian particularism is a cause of anti-Semitism but not of anti-black prejudice. They demonstrate that prejudice toward blacks is not highly related to the religious belief variables that they focus on in *Christian Beliefs and Anti-Semitism*. Since they maintain that these beliefs affect anti-Semitism but not anti-black prejudice, we are certainly justified in controlling for the latter, which is one of the traits most highly correlated with anti-Semitism.

It is impossible to determine in a cross-sectional study whether religious beliefs cause the other social psychological traits or vice versa. It seems more plausible to me that such traits as anomia and authoritarianism influence which theological beliefs an individual accepts than that religious beliefs

of the sort we are considering here cause an individual to be anomic or authoritarian. It is significant that in the present study the non-ideological measures of religious commitment—church attendance and the importance an individual attaches to religion—have very low correlations with such traits as authoritarianism, anomia, ethnocentrism, political conservatism, and anti-poor prejudice, as well as with anti-Semitism and anti-black prejudice. If exposure to religion or religious commitment played a powerful causal role in connection with these social psychological traits, we would expect these correlations to be higher.

The results of controlling for all three blocks of control variables at the same time are shown in the last two columns of Table 2. Once again there are major reductions from the zero-order coefficients. Among the individual measures of religious beliefs, religious libertarianism accounts for the greatest percent of the variance in anti-Semitism, but only slightly over 1 percent. Neither composite measure accounts for more than 1.8 percent of the variance.

In Table 3 the standardized regression and multiple correlation coefficients derived from three multiple regression equations are shown. The first equation includes only the individual religious belief measures as independent variables and anti-Semitism as the dependent variable. Multiple R for this equation is .392, and the corrected coefficient of determination¹² is .151. The second equation includes the twenty-one control variables used in Table 2 but not the religious belief measures as independent variables. In this case R is .585 and \bar{R}^2 is .329. The third equation, shown in the last column of Table 3, includes both the religious belief measures and the other variables as independent variables. Comparing the results of this equation with the results of the second equation, we

see that the effect of adding the religious variables is to increase R by a mere .015 and \bar{R}^2 by .017.¹³ The multiple-partial coefficient of determination¹⁴ between the five religious belief variables and anti-Semitism with all other variables held constant is only .025. Even adding such other religious measures as church attendance, importance attached to religion, and Protestant vs. Catholic affiliation to the equation increases the multiple correlation by only .002.

The data in Table 3 thus suggest that the religious variables do not play a very important independent role in causing secular anti-Semitism. Except in the case of religious orthodoxy the *betas* for the religious variables are statistically significant, but they are not large.

Additional regression analyses not shown in Table 3 reveal that the social psychological variables account for the most variance in secular anti-Semitism. Socioeconomic status alone accounts for 7.7 percent; other social attributes alone account for 7.9 percent; socioeconomic status and other social attributes together account for 11.2 percent; and the social psychological traits alone account for 29.5 percent. Adding the five religious belief variables to these sets of independent variables increases the percent of the variance accounted for by 9.9 for socioeconomic status, 10.6 for other social attributes, 8.7 for socioeconomic status and other social attributes together, and only 2.0 for the social psychological traits.¹⁵

Finally, in Figure 2, I present some first steps toward what I believe is a more realistic path model for analyzing secular and religious anti-Semitism. In this model I have added four new variables to the original six: education, family income, authoritarianism, and anomia. Apart from these additions, the major change from the Glock and Stark model is that religious hostility to historic and modern Jews is seen as a consequence rather than as a cause of secular anti-Semitism. There is, of course, no way with the present data to determine which of these alternative conceptions is correct, but I

¹² Since the inclusion of even an irrelevant variable generally increases R^2 because of chance fluctuation, I have used the following correction formula in which k is the number of regressors (not counting the constant regressor):

$$\bar{R}^2 = R^2 - \frac{k}{n - k - 1} (1 - R^2)$$

See Wonnacott and Wonnacott (1970:311) and Goldberger (1964:217).

¹³ $P < .05$ for the increment to \bar{R}^2 .

¹⁴ For a discussion of the multiple-partial coefficient see Blalock (1960:350-1).

¹⁵ $P < .05$ for each of the increments to \bar{R}^2 .

Table 3. Standardized Regression Coefficients in the Regression of Anti-Semitism on Measures of Religious Beliefs, Socioeconomic Status, Other Social Attributes, and Social Psychological Traits.

Variables	Equation 1	Equation 2	Equation 3
1. Religious orthodoxy	-.149*		-.042
2. Religious particularism	.073*		.059*
3. Religious libertarianism	-.296*		-.093*
4. Religious hostility to historic Jews	.105*		.067*
5. Religious hostility to modern Jews	.147*		.050*
8. Education		-.031	-.027
9. Family income		.033	.041
10. Occupation of head of household			
Professionals		.002	.010
Semiprof., technical, kindred		.032	.035
Managers, officials, proprietors		.013	.018
Clerical and kindred		.005	.010
Sales workers		.006	.007
Craftsmen, foremen, kindred		.057	.060
Operatives and kindred		.019	.030
Service workers		.015	.024
Farm managers and proprietors		.018	.023
Farm laborers and foremen		.004	.007
Nonfarm laborers and other		#	#
11. Age		.053*	.039
12. Size of community in which reared		-.025	-.005
13. Region in which reared			
Northeast		-.034	-.034
South		-.090*	-.096*
West		.037	.036
Midwest		#	#
Foreign-born		.070*	.070*
14. Years lived in community		-.008	-.007
15. Female = 1, male = 0		-.106*	-.116*
16. Black = 1, nonblack = 0		.150*	.125*
17. Organization member		-.007	-.009
18. Marital status			
Single		.096*	.097*
Married		.104	.109*
Separated		#	#
Divorced		.102*	.103*
Widowed		.072	.081*
19. Anomia		.159*	.141*
20. Authoritarianism		.135*	.124*
21. Anti-black prejudice		.217*	.196*
22. Ethnocentrism		.126*	.108*
23. Faith in people		-.070*	-.064*
24. Anti-Catholic prejudice		.120*	.109*
25. Anti-poor prejudice		.047*	.044*
26. Political conservatism		.044*	.051*
27. Psychic inadequacy		.020	.019
28. Cultural sophistication		.060*	.068*
Multiple R	.392*	.585*	.600*
Multiple R ² (corrected)	.151*	.329*	.346*

*P<.05

#Constrained category

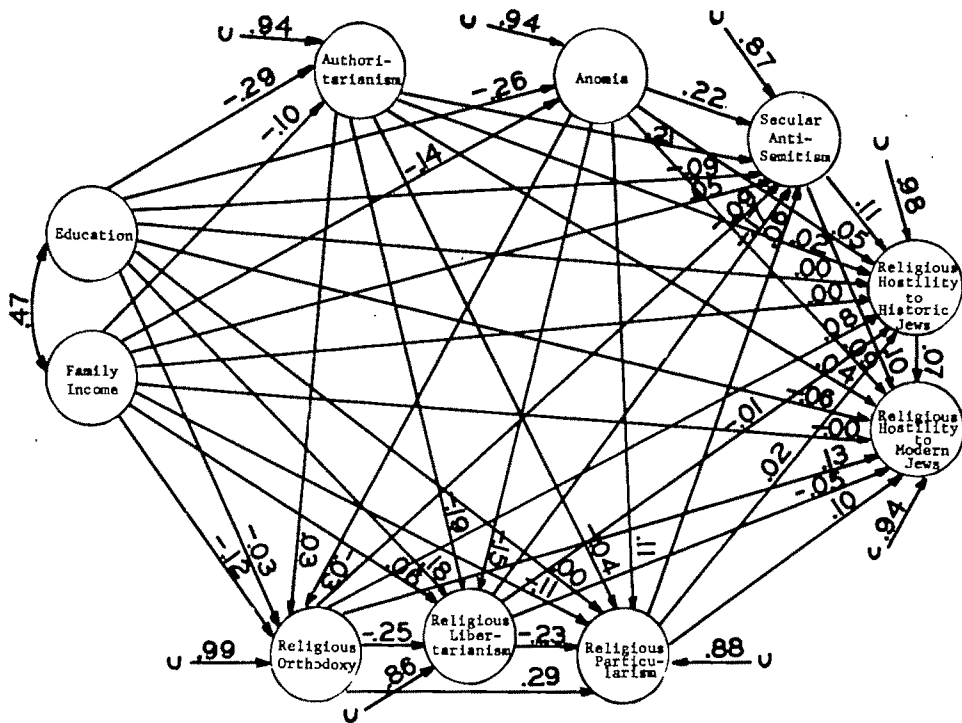
believe my model is at least as plausible as Glock and Stark's.

The path coefficients for Figure 2 show that education and family income are negatively related to authoritarianism and anomia. Religious orthodoxy is affected by family income only—not by education, authoritarianism, or anomia. Religious libertarianism is affected by religious orthodoxy and each of the preceding variables, though the path coefficient from family income is low. Religious particularism is affected by

orthodoxy, libertarianism, family income, and anomia—but not directly by education or authoritarianism.

When we turn to secular anti-Semitism we find that family income is the only variable which does not have a significant coefficient for the direct path to anti-Semitism, though particularism, education, and orthodoxy are only slightly higher. The largest coefficients are those for the direct paths from anomia, authoritarianism, and religious libertarianism. The picture is little changed even if

FIGURE 2. PATH MODEL FOR SECULAR AND RELIGIOUS ANTI-SEMITISM*



* $P < .05$ FOR ALL COEFFICIENTS GREATER THAN .05

we reverse the direction of the causal arrows running from secular anti-Semitism to the two measures of religious anti-Semitism. When the arrows are reversed the resulting coefficients for the paths leading to secular anti-Semitism are, from top to bottom, .86, .21, .20, -.09, .04, -.10, -.16, .05, .09, and .08. In all but one case the coefficients remain unchanged or change by no more than .01.

Thus, the most important determinants of secular anti-Semitism in this model are authoritarianism and anomia. The only religious variable which plays a major role is religious libertarianism—actually the willingness to grant civil liberties to atheists. This variable appears to stand apart from the other religious variables, perhaps reflecting a particular attitude syndrome or liberality of spirit at least as much as a certain orientation toward orthodox religion. It is the only “religious” variable in the model affected to any great degree by education, authoritarianism, and anomia.

As in the earlier path model, religious orthodoxy plays an interesting role, for even

though it shows a slight positive relationship to the two variables measuring religious anti-Semitism, it has a slight negative relationship to secular anti-Semitism. Thus, if one holds constant the other variables in the model, a highly orthodox religious person is slightly less likely to accept the secular anti-Semitic stereotype. As I pointed out before, however, the direct and indirect effects of religious orthodoxy cancel each other out, so that in sum orthodoxy has no effect on secular anti-Semitism.

The model is relatively poor in predicting religious anti-Semitism. Only secular anti-Semitism and religious orthodoxy have significant coefficients for the direct paths to religious hostility to historic Jews. The chief variables related in a direct way to religious hostility to modern Jews are religious orthodoxy, secular anti-Semitism, religious particularism, and anomia.

CONCLUSIONS

The data for Protestants and Catholics from the national survey of 1964 do not support the specific causal model formulated

by Glock and Stark to explain the relationship of certain religious beliefs and secular anti-Semitism. A path analysis reveals that the hypothesized causal sequence tends to break down at certain points. Also contrary to the theory, the impact of the religious variables tends to be expressed more through the direct paths leading to anti-Semitism than through the indirect paths mediated through religious hostility to modern Jews. Religious orthodoxy, the supposed starting point for the causal chain, is uncorrelated with secular anti-Semitism at the zero order, and in the path analyses the coefficient for the direct path between orthodoxy and anti-Semitism is actually negative.

Though their specific theory is not supported, there is some evidence supporting their general position that certain religious beliefs are related to secular anti-Semitism. Four of the five religious measures are significantly correlated with anti-Semitism, and together they account for approximately 15 percent of the variance in anti-Semitism. When we control on socioeconomic status, a number of other social attributes, and a number of social psychological traits, however, the religious variables account for no more than about 2 percent of the variance in anti-Semitism. This conclusion contrasts sharply with Glock and Stark's claims—based on the same national survey—that no less than a fourth of American anti-Semitism is attached to religious sources.

Do Christian beliefs cause anti-Semitism in the U.S. today? To this question I must give the response, "not proved." For the national survey the religious variables together account uniquely for about 2 percent of the variance in secular anti-Semitism, but even here we cannot be sure that the relationship is a causal one. Some of the religious measures may simply reflect a more general anti-Semitic ideology, or the relationship between the religious variables and secular anti-Semitism might disappear entirely if still other variables were controlled or if the present control variables were better measured.

On the basis of their analysis Glock and Stark admonished Christian religious leaders to undertake a systematic reappraisal of Christian education and to mount a massive assault on the beliefs currently held by many

Christians that the Jews were responsible for the death of Christ and are continuing to suffer divine punishment for their actions. The analysis I have carried out in this paper, however, leads me to be pessimistic about the efficacy of such an approach in reducing anti-Semitism. Merely altering the religious teachings regarding Jewish culpability and Jewish damnation is not likely to have a major effect on anti-Semitism if the well-springs of anti-Semitism today are largely secular. Nevertheless, Glock and Stark have performed an important service to Christian religious leaders in revealing that a great many Christians do adhere to anti-Semitic beliefs. The inconsistency of those beliefs with the Christian ethic of brotherhood represents a major challenge to these leaders.

REFERENCES

- Abelson, Robert P. and John W. Tukey
1959 "Efficient conversion of non-metric information into metric information." Pp. 226-30 in *Proceedings of the Social Statistics Section of the American Statistical Association*.
- Adorno, T. W. et al.
1950 *The Authoritarian Personality*. New York: Harper and Brothers.
- Allport, Gordon W.
1962 "Prejudice: is it societal or personal?" *Journal of Social Issues* 18 (April):120-34.
1966 "The religious context of prejudice." *Journal for the Scientific Study of Religion* 5 (Fall):447-57.
- Argyle, Michael
1958 *Religious Behaviour*. London: Routledge and Kegan Paul.
- Bettelheim, Bruno and Morris Janowitz
1950 *Dynamics of Prejudice: A Psychological and Sociological Study of Veterans*. New York: Harper and Brothers.
- Blalock, Hubert M., Jr.
1960 *Social Statistics*. New York: McGraw-Hill Book Co.
- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: John Wiley and Sons.
- Blum, Barbara S. and John H. Mann
1960 "The effect of religious membership on religious prejudice." *Journal of Social Psychology* 52 (August):97-101.
- Bohrnstedt, George W.
1970 "Reliability and validity assessment in attitude measurement." Pp. 80-99 in Gene F. Summers (ed.), *Attitude Measurement*. Chicago: Rand McNally.
- Borgatta, Edgar F.
1962 "Some comments on methodological developments in the last quarter-century." *Sociological Quarterly* 3 (October):296-315.

- 1968 "My student, the purist: a lament." *Sociological Quarterly* 9 (Winter):29-34.
- Boyle, Richard P.
1970 "Path analysis and ordinal data." *American Journal of Sociology* 75 (January):461-80.
- Brown, James
1957 "Christian teaching and anti-Semitism: scrutinizing religious texts." *Commentary* 24 (December):494-501.
- Camilleri, S. F.
1959 "A factor analysis of the F-scale." *Social Forces* 37 (May):316-23.
- Christie, R. and J. Garcia
1951 "Subcultural variation in authoritarian personality." *Journal of Abnormal and Social Psychology* 46 (October):457-69.
- Cohen, Jacob
1968 "Multiple regression as a general data-analytic system." *Psychological Bulletin* 70 (No. 6):426-43.
- Démann, Paul
1952 *La Catéchèse Chrétienne et le Peuple de la Bible*. Paris: Cahiers Sloniens.
- Dittes, James E.
1967 Review article on Charles Y. Glock and Rodney Stark, *Christian Beliefs and Anti-Semitism*. *Review of Religious Research* 8 (Spring):183-7.
1969 "Psychology of religion." Vol. 5, pp. 602-59 in Gardner Lindzey and E. Aronson (eds.), *The Handbook of Social Psychology*. Reading, Mass.: Addison Wesley.
- Eakin, Frank
1935 "What Christians teach about Jews: church school lesson materials." *The Christian Century* 52 (September 18):1173-6.
- Evans, Richard I.
1952 "Personal values as factors in anti-Semitism." *Journal of Abnormal and Social Psychology* 47 (October):749-56.
- Frenkel-Brunswick, Else and R. Nevitt Sanford
1945 "Some personality factors in anti-Semitism." *Journal of Psychology* 20:271-91.
- Furfey, Paul Hanly
1966 "Sociology and anti-Semitism." *Commonweal* 84 (September 2):558-9.
- Glock, Charles Y. and Rodney Stark
1966 *Christian Beliefs and Anti-Semitism*. New York: Harper and Row.
- Goldberger, Arthur S.
1964 *Econometric Theory*. New York: John Wiley and Sons.
- Greeley, Andrew M.
1967 Review of Charles Y. Glock and Rodney Stark, *Christian Beliefs and Anti-Semitism*. *American Sociological Review* 32 (December):1007-9.
- Hadden, Jeffrey K., Bruce Vawter and Israel Mowshowitz
1966 "Churchly particularism and the Jews: a trifaith symposium on the Glock-Stark anti-Semitism survey." *The Christian Century* 83 (August 10): 987-92.
- Hawkes, Roland K.
1971 "The multivariate analysis of ordinal measures." *American Journal of Sociology* 76 (March):908-26.
- Isaac, Jules
1956 *Genèse de l'Antisémitisme; Essai Historique*. Paris: Calmann-Lévy.
- 1964 *The Teaching of Contempt: Christian Roots of Anti-Semitism*. Tr. by Helen Weaver. New York: Holt, Rinehart and Winston.
- Jones, Marshall B.
1958 "Religious values and authoritarian tendency." *Journal of Social Psychology* 48 (August):83-9.
- Kelly, James G., Jean E. Ferson and Wayne H. Holtzman
1958 "The measurement of attitudes toward the Negro in the South." *Journal of Social Psychology* 48 (August):305-17.
- Labovitz, Sanford
1967 "Some observations on measurement and statistics." *Social Forces* 46 (December):151-60.
1970 "The assignment of numbers to rank order categories." *American Sociological Review* 35 (June):515-24.
1971 "In defense of assigning numbers to ranks." *American Sociological Review* 36 (June):521-2.
- Levinson, Daniel J.
1967 Review of Charles Y. Glock and Rodney Stark, *Christian Beliefs and Anti-Semitism*. *American Sociological Review* 32 (December):1009-13.
- Levinson, Daniel J. and R. Nevitt Sanford
1944 "A scale for the measurement of anti-Semitism." *Journal of Psychology* 17:339-70.
- Luttermann, Kenneth G. and Russell Middleton
1970 "Authoritarianism, anomia, and prejudice." *Social Forces* 48 (June):485-92.
- Mayer, Lawrence S.
1970 "Comment on 'the assignment of numbers to rank order categories.'" *American Sociological Review* 35 (October):916-17.
1971 "A note on treating ordinal data as interval data." *American Sociological Review* 36 (June):519-20.
- Monas, Sidney
1966 "Reasonable' bigotry." *Commentary* 42 (December):96-9.
- Neuwien, Reginald A.
1966 *Catholic Schools in Action*. Notre Dame, Ind.: University of Notre Dame Press.
- Olson, Bernhard E.
1963 *Faith and Prejudice: Intergroup Problems in Protestant Curricula*. New Haven: Yale University Press.
- O'Reilly, Charles T. and Edward J. O'Reilly
1954 "Religious beliefs of Catholic college students and their attitudes toward minorities." *Journal of Abnormal and Social Psychology* 49 (July):378-80.
- Parkes, James William
1934 *The Conflict of the Church and the Synagogue: A Study in the Origins of Anti-semitism*. London: Soncino Press.

- 1962 *A History of the Jewish People*. London: Weidenfeld and Nicholson.
- 1963 *Antisemitism*. Chicago: Quadrangle Books.
- Parry, Hugh J.
1949 "Protestants, Catholics, and prejudice." *International Journal of Opinion and Attitude Research* 3:205-13.
- Pettigrew, Thomas F.
1959 "Regional differences in anti-Negro prejudice." *Journal of Abnormal and Social Psychology* 59 (July):28-36.
- Prothro, E. Terry and John A. Jensen
1950 "Interrelations of religious and ethnic attitudes in selected southern populations." *Journal of Social Psychology* 32 (August):45-9.
- Rose, Arnold M.
1963 "America is changing the mutual images of Jews and Catholics." *Social Order* 13 (February):19-33.
- Rosenblith, Judy Francis
1949 "A replication of 'some roots of prejudice.'" *Journal of Abnormal and Social Psychology* 44 (October):470-89.
- Sanford, R. Nevitt
1950 "Ethnocentrism in relation to some religious attitudes and practices." Pp. 208-21 in T. W. Adorno et al. (eds.), *The Authoritarian Personality*. New York: Harper and Brothers.
- Schweitzer, Sybil and Donald G. Schweitzer
1971 "Comment on the Pearson R in random number and precise functional scale transformations." *American Sociological Review* 36 (June):518-9.
- Selznick, Gertrude J. and Stephen Steinberg
1969 *The Tenacity of Prejudice: Anti-Semitism in Contemporary America*. New York: Harper and Row.
- Simon, Marcel
1948 *Versus Israel*. Paris: E. de Boccard.
- Simpson, Richard L.
1959 "Negro-Jewish prejudice: authoritarianism and some social variables as correlates." *Social Problems* 7 (Fall):138-46.
- Srole, Leo
1956 "Social integration and certain corollaries: an exploratory study." *American Sociological Review* 21 (December):709-16.
- Stark, Rodney
1970 "Rokeach, religion, and reviewers: keeping an open mind." *Review of Religious Research* 11 (Winter):151-4.
1971 "Psychopathology and religious commitment." *Review of Religious Research* 12 (Spring):165-76.
- Stark, Rodney and Charles Y. Glock
1969 "Prejudice and the churches." Pp. 70-95 in Charles Y. Glock and Ellen Siegelman (eds.), *Prejudice U.S.A.* New York: Fredrick A. Praeger.
- Stark, Rodney, Bruce D. Foster, Charles Y. Glock and Harold Quinley
1971 *Wayward Shepherds: Prejudice and the Protestant Clergy*. New York: Harper and Row.
- Strommen, Merton P.
1967 "Religious education and the problem of prejudice." *Religious Education* 1 (January-February):52-9.
- Sudman, Seymour
1966 "Probability sampling with quotas." *Journal of the American Statistical Association* 61 (September):749-71.
- Suits, Daniel B.
1957 "Use of dummy variables in regression equations." *Journal of the American Statistical Association* 52 (December):548-51.
- Trachtenberg, Joshua
1943 *The Devil and the Jews: The Medieval Conception of the Jew and Its Relation to Modern Antisemitism*. New Haven: Yale University Press.
- Vanecko, James J.
1966 "Religious behavior and prejudice: some dimensions and specifications of the relationship." *Review of Religious Research* 8 (Fall):27-37.
- Vargo, Louis G.
1971 "Comment on 'the assignment of numbers to rank order categories.'" *American Sociological Review* 36 (June):517-18.
- Williams, Robin M., Jr.
1964 *Strangers Next Door: Ethnic Relations in American Communities*. Englewood Cliffs, N.J.: Prentice-Hall.
1967 *Review of Charles Y. Glock and Rodney Stark, Christian Beliefs and Anti-Semitism*. *American Sociological Review* 32 (December):1004-7.
- Wilson, Thomas P.
1971 "Critique of ordinal variables." *Social Forces* 49 (March):432-44.
- Wilson, W. Cody
1960 "Extrinsic religious values and prejudice." *Journal of Abnormal and Social Psychology* 60 (March):286-8.
- Wisconsin, University of
1969 *Users Manual, STATJOB Statistical Programs: FACTOR1*. Madison, Wis.: Academic Computing Center, University of Wisconsin.
- Wonnacott, Ronald J. and Thomas H. Wonnacott
1970 *Econometrics*. New York: John Wiley and Sons.
- Woodmansee, John J. and Stuart W. Cook
1967 "Dimensions of verbal racial attitudes: their identification and measurement." *Journal of Personality and Social Psychology* 7 (October):240-50.

American Sociological Review 1973, Vol. 38 (February):53-59

DO CHRISTIAN BELIEFS CAUSE ANTI-SEMITISM?—A COMMENT

Ten years ago, we set about designing a study to find out whether Christianity might still be a source of anti-Semitism in modern America. The idea that Christianity and anti-Semitism might be linked was scarcely new; of course, but the plan to test it proved to be more novel than we had surmised. For all the speculation, no one had set out with serious intent and reasonable resources to discover what Christian belief might mean for how Jews are responded to in today's world.

Once embarked on our inquiry, it became necessary to be more precise about the proposition we wanted to test; we were not anticipating, obviously, any one-to-one relationship between being a Christian and being anti-Semitic. We decided, partly out of historical study, partly out of insights developed from depth interviews with practicing Christians and partly because it seemed to make common sense, to focus our attention on the significance of Christian particularism for anti-Semitism. We reasoned that if a Christian is convinced that his religion has an exclusive claim on religious truth and conceives as central to that truth the *only* path to salvation to be through Jesus Christ, he is likely to experience difficulty in accommodating himself to religious outsiders generally and to Jews especially, who in America are the most visible "apostates." The alleged Jewish role in Christ's crucifixion would be a source of strain, we thought, as well as the Jews' continued rejection of Christ. We did not conceive a direct link between Christian particularism and secular anti-Semitism: nothing in particularism provides a basis for thinking, for example, that Jews are prone to cheat in business or to be less loyal to the United States. The religious hostility toward Jews which Christian particularism might generate, however, could spill over, we proposed, into secular anti-Semitism—it would be easy to move from thinking ill of Jews religiously to thinking badly of them on other grounds as well.

We formalized these ideas into a more elaborate, but basically simple model, adding that we expected that for a Christian to be particularistic, he would also have to be orthodox, that is, accepting of such fundamental tenets of Christian faith as belief in the divinity of Christ, in Biblical miracles, etc. We also introduced the idea that the postulated tendencies would be muted where traditional American values of religious libertarianism are absorbed. We first tested the model in an intensive study of a sam-

ple of Protestant and Catholic churchgoers in Northern California, and then more extensively on a national sample of the adult U.S. population. Our results were published in 1966.

Although our published writings do not differ markedly from *Christian Beliefs and Anti-Semitism* in substance or in methodological style, nothing that either of us has written alone or together has generated a similar critical storm. The stream of comment has continued almost unabated for some seven years; and by now, what has been written about the book pro and con, would fill several volumes, each as long or longer than the book itself.

We anticipated that the book would be controversial in religious circles, and it has. We did not anticipate a visceral reaction in the social science community. The book, however, landed on some tender nerves. The proposition that ideas can be independent causal agents has been one source of controversy, and the resistance to accepting it has informed much of the comment on the book. Mostly, the counter argument has been that some psychological or social structural variable has been overlooked which, if taken into account, would establish that ideas themselves have no punch. Our methodology has also borne the burden of much of the comment. Methodologists schooled in other perspectives than ours have not been entirely persuaded by our use of cross tabular procedures.

As might be expected, the commentary about the book has been mostly exegetical, although Russell Middleton is not the first to lay claim to having tested our work. In addition to the paper by Vanecko (1966) and our own replication using a sample of California clergy (Stark, et al., 1971), both of which Middleton cites in his bibliography, other studies having claimed to test our thesis include Mauss (1968), Kersten (1970), and Strommen, et al. (1972). There is also an additional study by Mauss (forthcoming). All these studies have used independent data. Middleton has scored a first in basing his test of our thesis on our own data, or more accurately, part of them.

We do not reject Middleton's paper out of hand. We've learned from it and have had some of our ideas modified by it. We remain unsatisfied, however, that his analysis denies our thesis or, as he puts it, doesn't support it. We are also not persuaded that he has come up with a more illuminating model of the role Christian belief may play in generating anti-Semitism or for that matter, a model which gives us a better general understanding of the sources of anti-Semitism.

Our uneasiness begins with Middleton's choice of data. The assumptions underlying the statistics he uses are best met not with our national data but with the data collected from Northern California Protestant and Catholic churchgoers. We had complete control over the California research instrument; it was devoted entirely to our purposes, and it was the data which we used to test our model most completely. We did a "piggy back" on someone else's questionnaire for the national study, and we were limited in the number of questions we were able to ask. To be sure, we picked questions which had worked effectively in California, but we had in mind a less extensive analysis using cross tabular procedures rather than regression. To insure that our findings were not an artifact of one set of data, we used three sets in the end: the Northern California parishioner data, the national sample, and the subsequent sample of California clergy. Middleton might have done the same. The data were accessible to him. Why he didn't elect to use them and why he opted for other than the strongest set of data by the standards of his methodology, we find enigmatic. That he did not do so weakens the power of his conclusions even if they were impeccable for the data he employed.

Middleton is close to being a true believer in his methodology, although he recognizes that there are ambiguities still to be resolved—for example, the justification for applying procedures to ordinal data originally developed for use with interval data. By and large, however, he has a large tolerance for the ambiguities. His preference for multiple regression and path analysis over cross tabulations is sufficiently strong that on occasions where the alternative analytic modes produce contradictory results, he does not pause to consider why. He takes for granted that his results using regression or path analysis are correct.

Our own methodological convictions are less secure though they strongly favor cross tabulation. We have serious misgivings about multiple regression as a tool in survey analysis. And, while we are more sympathetic to path analysis, we see it as a complement rather than a substitute for cross tabulation. Our taste is rooted in our academic socialization, but it is not out of loyalty to our mentors that it has persisted. For all their weaknesses, we continue to be convinced that cross tabular procedures, more than other methods, allow for following out the logic of survey analysis as it has been codified to date. We are also persuaded that these procedures are more suitable than others to elaborate the logic, a task still far from being completed even for three variable relations.

Our resistance to multiple regression stems in

part from its fundamental assumptions being unmet by the average set of cross-sectional survey data. Unless it is used with extraordinary care, multiple regression rides roughshod over most of the elementary logic of survey analysis. It used to be that the problem of interaction was not taken into account at all. Now it is not being taken care of satisfactorily. How to modify multiple regression when the independent variables stand in clearly delineated time order relative to one another has also to be satisfactorily worked out. Moreover, such more fundamental problems as how to deal with certain forms of specification or with suppression or distortion effects have so far not been tackled at all.¹ In our view, rarely can multiple regression safely be used as an exclusive analytic tool; for the reasons stated, we do not consider it a useful back-up tool either.

Compared to straight multiple regression, path analysis readily allows the time order of variables to be taken into account. Moreover, where the investigator knows the elementary logic of survey analysis, interpretations as well as suppression and distortion (which don't involve a specification) can be detected through path analysis. The method is not conducive to detecting specifications without extra care; nor has it been worked out how, if at all, the method may be used where specifications are present. Path analysis also, of course, applies only when certain theoretical conditions are met, most especially that the model be recursive. Still, we would not deny that it can be a useful adjunctive analytic tool.

We did not use path analysis in *Christian Beliefs and Anti-Semitism*. In 1964 and 1965 when we were doing the analysis for the book, path analytic procedures had not been fully codified for social science applications. As Middleton notes, we did make limited use of path analysis in the subsequent replicative study of clergy (Stark et al., 1970), although the primary tool of analysis in that study as in the original one was cross tabulation. The path analysis was added because it was in vogue at the time, because our analytic model seemed of a kind to allow its use, and because we were curious to discover whether the results of the two methodologies would coincide and what path analysis might tell us that cross tabulation would not. At the time, we concluded that the cross tabular and the path analyses essentially agreed, although because of ambiguities in techniques based on partial correlations, we cautioned against the use of path analysis except in combination with cross tabulations.

¹ See Rosenberg (1968) for a discussion of such effects.

Middleton faults us for discrepancies which he discovers between our theoretical model and our path analyses. He notes in commenting on the path analysis for the Northern California data that:

The direct path from orthodoxy to religious hostility toward modern Jews, for example, is greater than the theorized path through particularism. Furthermore, the causal sequence is almost entirely disconnected at the next step when we reach religious hostility toward the historic Jew. For the laity it is clear that this variable is not caused by particularism, nor does it in turn cause religious hostility toward modern Jews.

The discrepancies are real given one interpretation of our theoretical model and overlooking ambiguities in the substantive meaning of the path coefficients. They virtually disappear given our interpretation of our theoretical model and taking account of the interrelationships between these variables in their unsummarized form as revealed in cross tabulation.

How our theoretical model is understood depends, Middleton has helped us to discover, on whether attention is paid to what we say *or* to what we say and do. Middleton pays attention primarily to what we say and ends up interpreting our model in almost absolutist terms. Our tendency in narrative discussion was to describe it that way, and it is appropriate to fault us for doing so. It does not require a path analysis, however, to make evident that we mean the theory to be a probabilistic *and* an additive one. The thrust of the theory is that the more a subject conforms to the hypothesized path from orthodoxy to particularism, the more likely he is to exhibit religious hostility toward the modern Jew and in turn, secular anti-Semitism. By contrast, the less conformity to the model, the less the tendency to religious hostility and anti-Semitism. We meant the model to be additive as is evident, we think, from our development of composite measures as we went along. *The additivity is progressive, however.* Orthodoxy is postulated as a stimulant to particularism. Orthodoxy and particularism in combination are a source of religious hostility, and so on. *There are no propositions in the model about the effects of any individual religious variable considered alone on secular anti-Semitism.*

Path analysis is not suited especially to handling a progressively additive model. In consequence, certain relationships revealed with great clarity in the cross tabulations are obscured in the path analysis. Without reproducing all the relevant cross tabulations, it is not possible to deal thoroughly with all discrepancies included in Middleton's quotation. Perhaps, however, we can deal with the central points by reexamining one table from *Christian Beliefs*

and Anti-Semitism (1966:70). The table, reproduced as Table 1 below, reports for Northern California Protestants the joint effects of orthodoxy, particularism, and assigning to Jews the responsibility for Christ's crucifixion on religious hostility toward modern Jews, the latter being measured in this instance by acceptance of the proposition that "The Jews can never be forgiven for what they did to Jesus until they accept him as True Savior."²

This table tells us that those subjects who conform most to the theoretical model—that is, those who are highly orthodox, highly particularistic, and assign responsibility to the Jews for the crucifixion—are most likely to exhibit religious hostility toward the modern Jew; 86 percent do so. Among those who least conform to the model—that is, those low on orthodoxy, low on particularism, and who do not assign responsibility for the crucifixion to the Jews—one percent are religiously hostile. Those who deviate from the model in one or more but not all respects fall, with one small exception, between the extremes.³ The degree of the conformity, however, highly determines the amount of religious hostility.

It is true, as both the cross tabulation and path analysis show, that when particularism and responsibility for the crucifixion are controlled, orthodoxy is independently related to religious hostility in the partials. (This is what Middleton refers to when he talks about "the direct path from orthodoxy to religious hostility towards modern Jews.") This finding has no meaning with respect to the theoretical model, however. The model offers no proposition about the relation between orthodoxy considered alone and religious hostility.

With regard to the significance of who is assigned responsibility for Christ's crucifixion for religious hostility toward modern Jews, the path coefficient here is a modest .04 rather than the zero implied by Middleton's remarks. It is not clear what the coefficient means substantively. Looking at the data in the less summarized form afforded by cross tabulation in Table 1, we see that regarding the Jews as responsible for the crucifixion does produce greater religious

² The table reproduced as Table 1 is the strongest of several tables of similar structure presented in *Christian Beliefs and Anti-Semitism* (1966) and allows us to put our best foot forward. Examination of the other tables, however, (see Tables 28 for Catholics and 29 for Protestants and Catholics, pp. 70-2), will reveal that the thrust of the findings are reproduced, though less powerfully, in all the tables.

³ Note that in one cell of the table, zero percent show religious hostility, one percentage point less than the 1 percent for the least conforming cell.

Table 1. Influence of Orthodoxy and Particularism on Linking of Modern Jews with the Crucifixion (Protestants only)

Rank on Particularism Index	Orthodoxy Index					
	High		Medium		Low	
	Group Most Responsible for Crucifixion		Group Most Responsible for Crucifixion		Group Most Responsible for Crucifixion	
	Jews	Not Jews ^a	Jews	Not Jews ^a	Jews	Not Jews ^a
High	86% (242)	70% (78)	52% (103)	28% (54)	40% (21)	0% (22)
Medium	69 (152)	59 (49)	25 (221)	13 (123)	10 (114)	7 (85)
Low	19 (16)	.. ^b (5)	9 (67)	2 (46)	1 (135)	1 (101)

NOTE: Figures in parentheses show total number of respondents. (Percentage who agreed, "The Jews can never be forgiven for what they did to Jesus until they accept Him as the True Savior.")

^aIncludes persons who answered "the Romans," "The Christians," "None of these."

^bToo few cases to compute meaningful percentage.

hostility. In seven out of eight possible subgroup comparisons, those who conceive of the Jews as Christ killers are more prone to religious hostility than those who do not.⁴

Middleton is correct *statistically* in his observations that hostility toward the historic Jew is not caused by particularism. This is shown in cross tabular form in Table 2 which reports the joint effects of orthodoxy and particularism on conceiving of the Jews as responsible for the crucifixion.⁵ There is a positive zero order relation between particularism and hostility to-

ward the historic Jew, as can be seen in the total column. This relation is wiped out in the partials, however, when orthodoxy is introduced as a test factor.⁶

Table 2 would also appear to deny our earlier claim that our model is progressively additive. Orthodoxy alone does as well in predicting a conception of the Jews as Christ killers as do orthodoxy and particularism jointly.

However, the statistics in this instance mask the substantive (and statistical) point revealed in Table 1 that particularism, while not an influence on whether Jews are held responsible for the crucifixion, does influence how that be-

⁴The exception occurs among those who are low both on orthodoxy and particularism where religious hostility is expressed by one percent irrespective of views on the crucifixion.

⁵Table 2 has been computed from data reported in Table 1.

⁶This is an example, using the elaboration formula, of an *explanation* (see Hyman 1955:283-95).

Table 2. Influence of Orthodoxy and Particularism on Conceiving of the Historic Jews as Responsible for Christ's Crucifixion--Protestants Only (percentage who identify the Jews as the Group most responsible for crucifying Christ)

Rank on Particularism Index	Orthodoxy Index			
	High	Medium	Low	Total
High	76% (329)	66% (157)	49% (43)	70% (529)
Medium	76% (207)	64% (344)	57% (199)	65% (744)
Low	76% (21)	59% (113)	57% (236)	59% (390)
Total	76% (542)	64% (616)	55% (478)	

lief is interpreted. With greater particularism, as with greater orthodoxy, the tendency to link the perceived behavior of the historic Jew is increased. Thus, contrary to the impressions conveyed in Table 2, the model is indeed additive, and particularism has a substantive bearing on what the perception of the Jews as responsible for the crucifixion is interpreted to mean.

Cross tabulation procedures afford no guarantee that the kind of substantive point masked in Table 2 will be revealed. For example, given the results of Table 2, we may have lost trust in our model and never tabulated Table 1. Relatively, however, we think such subtleties are much less likely to be revealed by multiple regression or path analysis than by cross tabulations.

In retrospect these points ought appropriately to have been made in our discussion of path analysis in *Wayward Shepherds*. We stand grateful for the opportunity Professor Middleton has given us to make them here.

We turn now to Middleton's new analysis of our data. By and large, we were reassured rather than threatened by the correlations reported in Middleton's Table 1. The absence of a small relation between orthodoxy and anti-Semitism surprised us slightly since we had found a small positive relation in our own analysis.⁷ However, we had made no predictions about the direct relation between orthodoxy and anti-Semitism; and Middleton confirms that "orthodoxy is correlated with the other religious variables as [we] predicted."⁸

The crucial correlation between religious bigotry and anti-Semitism is also in the direction we predicted, and in the middle range (.30) as correlations go. Our theoretical model simply postulated that Christian belief would be found to be a contributing cause to anti-Semitism, it did not predict how high a correlation there might be. We make no claim to have discovered a higher correlation than this one. That a correlation of .30 explains 9 percent of the variance by the usual conceptual procedures also does not contradict our findings. The explained variance seems low when the maximum is conceived of as 100 percent; but as social science findings

go (explaining much more than 20 percent of the variance is rare), 9 percent doesn't seem quite as small.

A problem with measures of variance, as with other summary statistics, is that the same score may be reached by several routes. Consequently, without examining the unsynthesized data, it is difficult to know what substantive meaning to attach to a summary measure. For example, we consider it highly significant statistically and substantively that 85 percentage points separate those in our Table 1 who most and least conform to the model. This difference would be completely obscured in any summary measure of the results reported in this table.

Our skepticism about the rote use of multiple regression and path analysis extends to factor analysis. We are particularly distressed at the widespread tendency to submit variables indiscriminately into a factor analysis without a theoretical reason and without paying attention to the relative time order of the variables submitted. Two variables may load high on a factor because each is a measure of the same more general phenomenon or because one causes another. How the high loadings are interpreted obviously depends on what assumption is made about the time order of the two variables relative to each other.

In reporting the factor analysis which he conducted on our data, Middleton does not acknowledge the need to be sensitive to the problem of time order in factor analysis. Variables postulated by us to be causally connected are submitted to factor analysis indiscriminately along with variables we judged only conceptually related. The result is a hodge podge which, because he is not sensitive to the theoretical and logical issues, Middleton is unable to sort out. We came away from his discussion without experiencing his surprise, "that the various types of religious beliefs are not clearly differentiated." They shouldn't be. The question is why not. Middleton and the main source he paraphrases, James Dittes, assume it is because the types of belief are all measures of the same thing. Our position, which we reached on theoretical and logical grounds—not statistical—is that they comprise a causal sequence. Factor analysis cannot resolve the controversy.

Levinson's disagreement with our finding is also one of time order. Essentially, Levinson (and other critics of our work) argues that religious hostility toward the Jews and secular anti-Semitism are, as Middleton puts it, interchangeable measures of the same phenomenon. We argue on logical grounds that the two are distinct and that insofar as Christian beliefs are a cause of anti-Semitism, religious hostility pre-

⁷ This is one of the ambiguities just referred to.

⁸ The absence of a zero order relation between orthodoxy and anti-Semitism given these circumstances does not mean that orthodoxy is not an element in generating anti-Semitism. Rather it suggests that in addition to having such effects, there are circumstances which neither our analysis nor Middleton's reveals, where orthodoxy has an opposite effect. In Rosenberg's (1968) terms, the absence of a zero relation is the result of a suppressor effect.

cedes secular anti-Semitism.⁹ There is no statistical way to resolve the issue given cross sectional data. Middleton apparently disagrees but acknowledges, nevertheless, that his limited statistical treatment does not resolve the debate.

In his discussion of the path analysis which he constructed from our national data, Middleton acknowledges that while the path coefficients are small, religious orthodoxy is linked to anti-Semitism through the causal path we hypothesized. The path analysis also reveals direct links between the four religious belief variables in our theory and anti-Semitism, a result which Middleton judges to be in disconformity with our theory.

We can only repeat what we said earlier—the discrepancies are real given his interpretation of our theoretical model and overlooking certain ambiguities in the substantive meaning of the path coefficients. Given a correct understanding of our theoretical model and recognizing that path analysis is unsuited to testing that model except in a limited way, we expect that the discrepancies are not real, although we do not have in hand the detailed cross tabulations to prove it except for the Northern California data. There is no reason to expect, however, any lack of congruence between the Northern California and national data on this score.

In light of the effort made to discredit it, we were mildly astonished that our model survived to the extent that Middleton judges it worthy enough to subject to tests of causality. However, this reaction preceded our recognition that an intended *coup de grace* was still on his agenda.

Our own tests for spuriousness were not exhaustive. They went as far as our imagination and data allowed. While we ended up convinced that our case was strong that Christian beliefs remain a source of anti-Semitism in the modern world, we recognized that we could not prove this absolutely and that we or someone else might later discover some antecedent factor which would disprove it.

Our reservations about the tests for spuriousness which Middleton conducted are rooted again in methodology, though we were disturbed also at his lack of sensitivity to theoretical and substantive issues.

Middleton first tests the relation between religious bigotry and anti-Semitism by simultaneously controlling for the same antecedent factors which, in our test, we controlled for one or two at a time. The result of his test is *not* to prove

the original relation spurious, but to demonstrate, allegedly, that the relation is smaller when the controls are introduced.

Evaluating Middleton's test is difficult because he fails to report how one antecedent variable to be tested for, region of the country, was treated in computing the partial correlation. Region of country, as we had used it, was a nominal variable of five categories. All other test variables were ordinal and could be treated ordinally. In our experience, quite different results can be obtained depending on how nominal variables are handled in such computations.

Middleton fails also to state whether tests were made for interactions or specifications. Partial correlations, about which we have more general reservations, assume no interactions or specifications, of course.

Methodological ambiguities aside, the test for spuriousness in this instance is made rank empirically without reference to theory or substance. Just what may be reducing the size of the relation is left a mystery. We consider it important in evaluating a statistic to know, substantively, how it came to be.

A neglect of theory and substance also characterize Middleton's report on all of his other tests for spuriousness. The net impression conveyed is that the original relation between Christian beliefs and anti-Semitism is spurious, or almost so. We are never told, however, just what makes it spurious; the antecedent variables which explain away the original relation are never clearly stated. Moreover, insofar as such social psychological traits as anomia and authoritarianism are alluded to as the "true" causal agents, no theoretical reasons are given as to how they may lead simultaneously to certain religious convictions and anti-Semitism.

Otherwise, we found most of Middleton's tests for spuriousness irrelevant to our theoretical model. The model offers no propositions about the relation between individual religious variables taken alone and secular anti-Semitism. Middleton's tests for such relations are superfluous. Of the relations between composite measures and anti-Semitism tested for, the only relevant one for our model is the relation between religious bigotry and anti-Semitism. This relationship survives all his tests by never being wholly washed away, even when *all* control variables are submitted simultaneously to the regression. Still, if the regressions are taken seriously, what remains has hardly any substantive or statistical significance.

Our resistance to accepting Middleton's results stems partly from their failure to make clear what is going on substantively. From his report, it also does not appear that he took pre-

⁹ Simply put, the logic is that there are grounds in Christian particularism for religious hostility toward Jews but not for holding them in contempt for perceived non-religious behavior.

cautions to insure that no interactions, specifications, hidden distortion or suppression effects exist which might make the regressions invalid. More fundamentally, we find the premises of multiple regression inconsistent with any theoretical view of the structure of reality that we can work with. Among other things, it just does not make substantive sense to us to assume, as Middleton's multiple regression does, that all test variables are of the same time order.¹⁰ As for survey analysis, we find the logic of multiple regression incompatible with the logic of cross tabular analysis as we have used it and as it has been codified by Lazarsfeld and Kendall (1950), Hyman (1955), Rosenberg (1968), among others.

Middleton's "more realistic path model for analyzing secular and religious anti-Semitism" is far more pretentious than our model. To our view, not enough is said about the theory and logic underlying his model to give it substance. And, we have reservations about path analysis when its theory and propositions do not take a certain form and are not clear. The reservations multiply when the grounds for a path analysis are largely rank empirical.

As to the bearing of the model and path analysis on the issues which concerned us, we must confess to being confused. If there is a rationale for conceiving of secular anti-Semitism as a source rather than a consequence of religious hostility to Jews, we are at a loss to imagine it, and Middleton's discussion gives no clue. Otherwise, we found little of relevance in the path analysis to the propositions we sought to test.

The gulf which separates Middleton's and our approach to data is not idiosyncratic, but reflects a much wider disagreement among quanti-

tative sociologists about theory and method. By and large, we have lived with the disagreements until now without seriously and systematically confronting them. Perhaps the time has come to do so. We hope that these remarks on Middleton's paper will stimulate dialogue.

CHARLES Y. GLOCK

University of California, Berkeley

RODNEY STARK

University of Washington

REFERENCES

- Glock, Charles Y. and Rodney Stark
1966 *Christian Beliefs and Anti-Semitism*. New York: Harper and Row.
- Hyman, Herbert
1955 *Survey Design and Analysis*. Glencoe, Illinois: The Free Press.
- Kersten, Lawrence L.
1970 *The Lutheran Ethic*. Detroit: Wayne State University Press.
- Kendall, Patricia L. and Paul F. Lazarsfeld
1950 "Survey analysis." Pp. 133-96 in Robert K. Merton and Paul F. Lazarsfeld (eds.), *Continuities in Social Research: Studies in the Scope and Method of the American Soldier*. Glencoe, Illinois: The Free Press.
- Mauss, Armand L.
1968 "Mormon Semitism and anti-Semitism" in *Sociological Quarterly* 29 (Spring):11-27.
1973 *Mormonism and Minorities*. Berkeley and Los Angeles: University of California Press.
- Rosenberg, Morris L.
1968 *The Logic of Survey Analysis*. New York: The Free Press.
- Stark, Rodney, Bruce D. Foster, Charles Y. Glock and Harold Quinley
1971 *Wayward Shepherds: Prejudice and the Protestant Clergy*. New York: Harper and Row.
- Strommen, Merton R., Milo L. Brekke, Ralph C. Underwager and Arthur L. Johnson
1972 *A Study of Generations*. Minneapolis: Augsburg Publishing House.
- Vanecko, James J.
1966 "Religious behavior and prejudice: some dimensions and specifications of the relationship." *Review of Religious Research* 8 (Fall):27-37.

¹⁰ Indeed, in the present instance, we did not find it appropriate to assume that all the independent variables precede the dependent variable in time. On logical grounds, for example, a case can be made for assuming anomie to be of the same time order as anti-Semitism rather than prior to it. Questions of time order are difficult, we recognize. They are not resolved by ignoring them.

RESPONSE

I chose to analyze the data from NORC's national sample because I thought it more appropriate to test general theoretical propositions with a representative national sample than with the more specialized samples—members of pre-

dominantly white churches in the San Francisco Bay area or parish clergymen of the nine largest Protestant denominations in California. Not only do the California samples tend to exclude those who are less religiously oriented,

but they also exclude the one-third or more persons drawn in the samples who failed to return their questionnaires. The national study also includes measures of authoritarianism and a number of other social psychological variables useful in testing whether other theoretical interpretations are feasible. It is precisely these social psychological variables which have the greatest effect when introduced as controls. Thus, the type of analysis I have made with the national data cannot be carried out fully with the other two samples.

I agree with Glock and Stark that our differences are in large part ones of methodological orientation. I do not regard multiple regression analysis as a panacea or method that has no limitations. I also believe that cross tabulations are very useful in conjunction with a regression analysis. On the other hand, I believe that Glock and Stark seriously underestimate the power, flexibility, and fruitfulness of multiple regression analysis in testing sociological theory.

Glock and Stark complain that the fundamental assumptions of regression analysis are not met by the average set of survey data and that regression analysis tends to ride roughshod over the elementary logic of survey analysis. They also say that regression analysis fails to deal in a satisfactory way with interactions, specifications, suppressor and distorter variables, and the time sequence of variables. These charges are extraordinarily vague and cannot be taken seriously in the absence of particulars. Just which assumptions were violated in the data that I used, and how did these violations affect the results? In what way did my use of regression analysis violate the logic of survey analysis? How did I fail to handle the problem of interactions satisfactorily? In what sense is regression analysis inadequate for handling specifications, suppressor and distorter variables, or time sequences of variables? Actually, regression analysis is a robust statistical technique which is not very sensitive to violations of assumptions, and it can handle the kinds of analytic problems which Glock and Stark mention effectively. Contrary to what they assert, regression analysis can even be used under certain circumstances to test nonrecursive models.

Regression analysis is particularly valuable in testing theories in which the variables are linked in a causal time sequence. Glock and Stark distinguish between "straight multiple regression" and path analysis, toward which they feel more sympathetic; but path analysis is simply regression analysis in the service of a strictly defined theoretical model. Of course, any method of analysis—including cross tabula-

tions—may be used in an atheoretical, unintelligent manner; but the fault is in the user, not the method.

The crux of the matter in the present instance is that with a limited sample size, cross tabular analysis is simply inadequate to test a complex theoretical model linking six variables in a precise causal sequence. If one includes all six variables and at the same time controls on several "test factors," the number of cells can quickly mount into the thousands. The number of cases per cell would, of course, be too small; but even if there were sufficient cases the sheer number of cells would make it virtually impossible to discern any pattern. In *Christian Beliefs and Anti-Semitism* Glock and Stark presented a complex theory of this nature. In its main outlines it was precisely stated in both words and diagrams. They argued that there is a particular causal chain linking certain religious beliefs and leading to secular anti-Semitism. Furthermore, they maintained that orthodoxy, religious libertarianism, particularism, and religious hostility toward the historic Jew have *no* direct effect on anti-Semitism but only an indirect effect through religious hostility toward modern Jews. I took their statement of their theory seriously and attempted to test it through regression analysis. The evidence was clearly against it, and Glock and Stark now appear to be ready to abandon that specific formulation.

I also attempted to test the much looser theoretical formulation that the various religious beliefs have simply an additive effect on anti-Semitism. If this is all Glock and Stark really intended, then they are correct that the various religious beliefs do account for a substantial part of the variance in anti-Semitism—at least if one does not use any controls. In their comments Glock and Stark say that their model should be interpreted as "progressively additive" and that path analysis is not really suited to handling such a model. If I understand what they mean by "progressive additivity," they are wrong. The path analysis which appears in my Figure 1 shows precisely what Glock and Stark say that only their tables reveal. When a person is "high" on orthodoxy, particularism, and religious hostility to historic Jews, he is most likely to be religiously hostile to modern Jews. When he is "low" on one or more of the preceding variables, he is less likely to be religiously hostile to modern Jews. This is all clear from the path coefficients. Their discussion of "progressive additivity" is ambiguous however. They imply that there is some special effect from taking the different religious beliefs in combination with each other. If so, theirs is really an

interactive rather than a "progressively additive" model. In my regression analysis I did test for interactions and found none.

We diverge once more over the question of introducing controls or "test factors," and our disagreement here is more theoretical than methodological. I employed the same controls that Glock and Stark did, but I introduced them simultaneously, and I also introduced some additional controls primarily of a social psychological nature that I regarded as theoretically important. I thought I had made clear the rationale for my controlling on the social psychological variables. The point is really commonplace in the literature on prejudice, and I did not feel it required extended discussion in an already long paper. Briefly, the argument is that certain underlying personality characteristics lead some individuals to accept prejudiced beliefs as well as certain types of dogmatic religious beliefs. By controlling on a number of social psychological indicators of underlying personality traits or systems of beliefs theoretically presumed to be related to anti-Semitism, I attempted to filter out the effects of some of these personality factors or cognitive styles and to determine whether any specifically religious components of beliefs had an independent effect. They do, but not much—only two percent of the variance. Even this modest effect might well

disappear if still other social psychological controls were added or if the ones I used were better measured.

Glock and Stark state that I failed to report how region was treated in the regression analysis. The categories used for this dummy variable as well as for occupation of head of household and marital status are clearly indicated in my Table 3.

I strongly agree with Glock and Stark that beliefs and ideas can be independent causal agents. It is the researcher's obligation, however, to test whether this is the case in a particular instance—not simply to assume it. In the literature on prejudice a major theoretical tradition emphasizes personality factors and belief systems which would lead to a different interpretation of Glock and Stark's findings. Glock and Stark did not test this alternative interpretation. I attempted to do so. I have not proved that Glock and Stark's more general theory is wrong. Such proof is probably not possible in a cross sectional survey, since one cannot demonstrate the time sequences of the variables. I believe I have demonstrated, however, that other interpretations of their findings are at least as plausible—and to my mind more plausible.

RUSSELL MIDDLETON

University of Wisconsin, Madison

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Fellows and Active and Student members of the Association may submit manuscripts of 100 to 300 typed pages for publication in the *ASA Arnold and Caroline Rose Monograph Series in Sociology* to the Series Editor, Sheldon Stryker, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

THE DEVELOPMENT OF POLITICAL ORIENTATIONS AMONG BLACK AND WHITE CHILDREN *

ANTHONY M. ORUM

University of Texas at Austin

ROBERTA S. COHEN

University of Illinois at Urbana-Champaign

American Sociological Review 1973, Vol. 38 (February):62-74

Recent research suggests that, with socioeconomic differences held constant, black adults are more disaffected and active politically than their white counterparts; yet, this same research neglects to specify the timing and location of these differences. The present study undertakes this task, attempting to discover whether the differences in political orientation between blacks and whites is found among children as well. The results convincingly demonstrate that such differences are found among very young children as well as teenagers: for example, among children who are 9, 10 and 11 years old, black youngsters are consistently the more cynical and informed politically. An effort to isolate the general origins of these differences, moreover, suggests the viability of a subcultural as opposed to a psychodynamic hypothesis.

THE racial conflicts which so dominated American domestic politics in the 1960's left an indelible imprint on many institutions, significantly altering relations between blacks and whites, and creating a variety of innovative political activities. The turmoil of these times sprang from major differences in the political beliefs and values of blacks and whites, but intensified such differences as well. Black Americans became increasingly aware of their collective identity, an awareness that manifested itself in elevated levels of political activism and disaffection with the wider society.

* These data were collected as part of a study of the political socialization of black and white youth conducted under the auspices of the Survey Research Practicum, Department of Sociology, University of Illinois at Urbana, Illinois, 1970-71. The study was funded by the University of Illinois. The Practicum was directed by Anthony M. Orum assisted by Gregory Arling and Gordon Lurie. Students who participated in the Practicum were: Roberta Cohen, Kay Darnell, McKinley Jones, Marilyn Klohr, Janice Perrier, Dennis Roncek, and Leonard Thornton. The authors are quite grateful to each of the above-named persons for their aid as well as to Seymour Sudman and Matt Hauck of the Survey Research Laboratory at the University of Illinois who provided the Practicum with able guidance during the design of the study. Judith V. Torney also provided helpful advice on the development and construction of the questionnaire.

We are grateful to Seymour Sudman and Norbert Wiley for their comments on an earlier draft of this paper. In the course of a Summer Postdoctoral Fellowship at the American College Testing Program, the senior author also received helpful comments on the contents of the paper from Melvin Novick and Philip Rever.

What social psychological conditions led to the growth of such high levels of political activism and disaffection among blacks? This issue can be put in the form of two queries. First, at what age do the differences in political orientations first appear for blacks and whites? That is, are they found principally among adults rather than young people? And second, what specific dynamics have produced such differences?

Systematic evidence on the first of these matters tends to be scarce. While recent studies (Olsen, 1970; Orum, 1966; and Sears, 1969) consistently show political differences between black and white adults, blacks being the more active in politics, yet the more disaffected about government, only a few studies have investigated the political attitudes and behavior of young black and white Americans (Green, 1972; Greenberg, 1969, 1970a, 1970b, and 1970c; Jaros, 1967; and Lyons, 1970).¹ With the exception of Jaros' study, which found no differences in the feelings of black and white children toward the office of the President, the results of these latter investigations parallel those for adults. Nevertheless, each of these

¹ Three other studies contrast the political characteristics of black and white school children, one by Bachman and his associates (1970), one by Ehman (1969), and one by Langton and Jennings (1968). We have not included them in this discussion because they only cover sophomores and/or seniors in high school and, therefore, do not actually treat at what age political differences might first appear. Certain results from these studies, however, are relevant to the concerns of this paper and will be discussed later.

studies suffers from deficiencies which render its conclusions tentative at best. Green's examination treated only a small sample of black children attending a private Catholic school; while the investigations by Greenberg, Jaros and Lyons compare black and white children on a fairly limited range of political activities and beliefs.

The etiology of differences in the political orientations of blacks and whites has not been systematically investigated, although intriguing speculative arguments have been made (Olsen, 1970; Orum, 1966). One argument claims, for instance, that blacks have become more active in politics to compensate for white discrimination; while another suggests that the increased unity of the black community somehow prompts higher rates of activism. In articulating the notion of compensation, Orum (1966:45) remarks:

Since Negroes are deprived of the usual social and psychological satisfactions of everyday life, they are compelled to seek such satisfactions collectively through other means. Opportunities for association are restricted by explicit or tacit observance of segregation in public places of entertainment. The oppressive atmosphere of slum dwellings also does not offer a congenial environment for social activity. Quite naturally, then, clubs and associations become focuses for Negroes' social life.

The principal difficulty with such arguments is that they rely chiefly on the forces and circumstances in adult lives that forge political styles, or what Greenstein (1965) calls "situational restraints," and ignore experiences in childhood and adolescence that might help lay the groundwork for political beliefs. Much recent research suggests that the development of basic behavior and beliefs about politics occurs long before adulthood, and that the social sources of these phenomena must be traced to events at a similar point in time (Dawson and Prewitt, 1969; Easton and Hess, 1962; Easton and Dennis, 1969; Greenstein, 1965; Hess and Torney, 1967; Hyman, 1959).

In this paper, we shall address several of these issues. To begin with, we shall compare black and white children along a wide spectrum of political dimensions to see if they differ politically. Our investigation has several important advantages over previous

research. We have interviewed black and white children at the same point in time, thereby avoiding the difficulty of comparing responses of children interviewed several years apart (cf. Green, 1972). And our questions cover a wide range of political topics (cf. Greenberg, 1969, 1970a, 1970b, and 1970c; Jaros, 1967; and Lyons, 1970).

Our other major task will be to assess the origin of whatever differences might exist between black and white school children. We shall compare two theories about these sources, to identify the kind of factors that might produce differences in political orientations between blacks and whites.

Throughout our analysis, two variables will be held constant, the child's grade in school and his parents' socioeconomic status, as measured by the occupation of the chief wage earner. The former variable is controlled to determine whether differences between black and white children are similar in magnitude across different age groups or whether, as Greenberg (1969, 1970a) suggests, such differences increase among older children.² Socioeconomic status is controlled to eliminate its possible attenuating influence on the relationship between race and the measures of political behavior and belief. (The reader will note, however, the effects of socioeconomic status throughout our analyses were fairly minimal, in contrast to our expectations.)³

² We have consistently used grade in school as our measure of age, assuming a high correspondence between age and grade for both white and black children. We ought to mention further that variations in grade, or age, simply are a substitute for performing a longitudinal analysis on the same set of children; in effect, we assume that, apart from maturation and development, no other variables correlated with age will explain patterns of differences in political belief and behavior among children of different ages. While this assumption is reasonable, studies need to determine the extent to which historical experiences, particularly of lasting political consequence, interfere with the pure effects of maturation. Unfortunately, no such investigations have been conducted.

³ Much time and thought were spent on the issue of why socioeconomic status so little influenced the political beliefs and behavior of these children. For example, in addition to using occupation of the chief wage earner to measure class, we repeated the analyses using parents' education for mothers and fathers separately; it, too, produced only minor differences. We further considered whether the children might have consistently misreported their

Sample

The data for this study were collected by means of self-administered questionnaires completed by 2,365 Illinois students in their classrooms in the spring and fall of 1971. Children in the fourth through twelfth grades were sampled in four urban areas of the state. Sample areas were chosen by the extent to which they represented the racial and socioeconomic characteristics desired by the investigators. Each area represented a peculiar combination of characteristics. Fifty percent of the total sample was drawn from an inner-city, lower-class, predominantly black public school system. The second area, from which 10 percent of the total sample was drawn, consisted of an inner-city, middle- to upper-middle-class, integrated private school. The last two areas each contributed twenty percent of the total sample, with one area constituting a lower-middle- to working-class public school system and the other a middle-class public school system, both areas being principally composed of white students.

The total sample consisted of approximately half white and half black students, with equal numbers of each sex. Children in the fourth, fifth, and sixth grades filled out a slightly abbreviated version of the questionnaire.

Dimensions of Political Behavior and Belief

The four major dimensions of political orientations are the affective, behavioral,

parents' occupational status and/or level of educational attainment, but an investigation of a small sub-sample, consisting of four hundred children and their parents, revealed no major amount of discrepancy in the reports of children and adults. Indeed, using a gross classification of chief wage earners into white- and blue-collar groups, as we report later in this paper, the amount of error is almost imperceptible. In the end, we simply concluded that the method of selecting schools for our sample had the ultimate effect of underrepresenting important segments of middle-class black and white youth, the white children no doubt much more substantially than the black ones. Such underrepresentation, however, does not vitiate the racial comparisons here, but suggests caution in drawing inferences from our sample to the larger population of black and white school children. Partly to cope with this problem, we later discuss our findings compared to those uncovered by somewhat similar studies in different locales.

cognitive, and evaluative (Hess and Torney, 1967). In the following sections, we shall examine how black and white children rank on each dimension.

Political Affect: Benevolence, Cynicism and Image of the President. Without exception, investigations of early political learning emphasize that the affective side of political orientations develops before the others, and colors one's later reactions to political objects and symbols (Dawson and Prewitt, 1969; Hess and Torney, 1967). As Dawson and Prewitt (1969:48) observe:

Children learn, early and fervently, that there are significant political groupings in society and that some groups are their friends and others their enemies. The intensity of regional, racial, class, ethnic, and tribal political conflicts are attributable, in part, to these aspects of early socialization. Political friends and enemies are formed long before the child fully understands what interest or policy differences actually divide them, and may persist long after such interests or policies actually make any differences.

Assuming for the moment that children do differ politically and in ways comparable to adults, we expect black children to be less positive about government and politics, in general, than white children.

To assess children's sentiments toward the polity, we developed three measures of affect based on a factor analysis of a wide range of attitudinal and behavioral questions.⁴ The first, a measure of political cynicism, consists of such standard questions as, for example: "Does the government make a lot of mistakes?"⁵ Table 1 presents data on

⁴ Principal component factor analysis was performed on two thirty-six variable sets to ascertain the existence of "unobserved" or "latent" variables represented by the resulting vectors. Items explaining the greatest amount of vector variance were then extracted to form composite variables, with the factor loadings being used to weight the relative item importance within each scale.

⁵ The political cynicism scale for fourth to sixth graders, which had scores ranging from .64 to 2.56, consisted of the following items:

Does the government make a lot of mistakes? (weight = .71)

Can the government be trusted? (weight = .68)

Are there some big powerful men running the government who do not care about us ordinary people? (weight = .61)

The political cynicism scale for seventh to twelfth graders is made up of the same items as those for the younger children plus the following items:

the child's racial background, grade in school, socioeconomic status and political cynicism.

The figures in the main cells of the table represent the children's mean political cynicism scores. Two political cynicism scales were developed, one for children in the fourth through sixth grades, and the other for children in the seventh through twelfth grades. Thus, comparisons between the mean scores of children in the cluster of lower grades and the cluster of higher grades are invalid, though comparisons can legitimately be made for children within each cluster. The last column of this table provides summary measures of the relationship between the child's race and political cynicism, within particular grades with the occupation of the chief wage earner held constant; in computing these partial correlations, the complete range of occupational categories was used rather than the simple white-collar, blue-collar dichotomy. This latter classification, in fact, was presented here chiefly to allow the reader to observe some general patterns for himself.

Regardless of grade in school or socioeconomic status, black children score higher on political cynicism than white children. The partial correlations in the table's last column reveal that the magnitude of the difference in scores remains fairly constant from the elementary grades through high school. Like other investigators (Greenstein, 1965; Jaros, 1967; Lyons, 1970), we also find that children from white-collar families tend to be more cynical about politics than children from blue-collar families; and, children in the fourth through sixth grades are likely to be less cynical than children in the seventh through twelfth grades.

These results deal only with children who are in school, thus ignoring the large numbers of black children who drop out. If this

group of black children were included as part of our sample, the differences between black and white youngsters would probably be even more pronounced, particularly for the affect measures. This comment, of course, holds true for other items examined in the following sections as well.

Two other types of scales were created. The one, which we shall refer to as "government benevolence," following Greenberg's lead (1970a), is based on questions about whether a child feels that government at national, state and local levels helps improve conditions for people.⁶ The other scale measures the degree to which a child possesses a favorable image of the President.⁷ Table 2 contains the mean scores and partial correlations. Viewing the relationship between race and government benevolence, we find that black children are consistently less likely to feel that government is benevolent than white children, a difference appearing as early as the fourth through sixth grades and continuing through the junior and senior years of high school. Likewise, black children consistently rank lower than white with respect to their positive image of the President. In the case of both scales, the patterns of age and socioeconomic status differences are much less pronounced than they were in the case of the political cynicism measure.

In sum, the pattern of findings reveals that black children are apt to be much more

⁶ The government benevolence scale consisted of three items each asking: "Does the _____ government make things better for most people, make things sometimes better, sometimes worse for most people, make things worse for most people, or make no difference at all?" Of the three items, one referred to the U.S. government, one to state government, and one to local government. The range of scores for this scale was 2.18 to 8.72.

⁷ The President image scale, with a range of values from 1.53 to 5.63, consisted of the following items:

Would the President always want to help you if you needed help? (weight = .51)

Does the President protect you more than anyone else? (weight = .53)

When you write to the President do you think he cares about what you think? (weight = .49)

Two other scales were computed and included in this analysis yielding identical results to the President image scale. They were the image of the policeman scale and the image of the politician scale.

.Rich people are the ones who decide what goes on in government. (weight = .39)

The U.S. needs a complete change in its form of government. (weight = .51)

The range of scores for this scale was from 1.54 to 6.16. Our use of the phrase "political cynicism" to refer to these items follows the convention employed by political scientists who have used these, or similarly-worded items (Jaros, Hirsch and Fleron, 1968; Langton and Jennings, 1968; Lyons, 1970).

Table 1. Race, Grade in School, and Occupation of the Chief Wage Earner by Political Cynicism (mean scores on cynicism)

Grade in School	Chief Wage Earner Occupation	Race		Partial* r (Occupation Controlled)
		Black X	White X	
4	White Collar	1.93	.71	+.238**
	Blue Collar	1.56	.74	
5	White Collar	2.38	1.50	
	Blue Collar	1.70	1.01	
6	White Collar	2.16	1.85	
	Blue Collar	2.45	1.58	
7-8	White Collar	5.10	3.74	+.361**
	Blue Collar	5.14	3.59	
9-10	White Collar	5.59	5.06	+.230**
	Blue Collar	5.16	4.24	
11-12	White Collar	5.47	4.52	+.231**
	Blue Collar	5.37	4.01	

* The partial correlations reported in this and other tables included in this paper are always of a dependent political variable with race, with occupation of the chief wage earner controlled. In computing such correlations, blacks will always be considered as the high group, white as the low group. Thus, a positive partial correlation means that blacks score higher than whites, while a negative one means that blacks score lower than whites. In the case of political cynicism, for example, all signs are positive, meaning that black children score higher on cynicism than white children, with occupation of the chief wage earner controlled.

** Significant at .01 level. Tests of significance will be reported here and elsewhere in this paper for interested readers. Although we have no reason to believe that the schools included in our sample were in any way different from other schools attended by black and white children, save for the fact that middle-class youngsters are underrepresented, the principle of probability sampling underlying the application of significance tests is not met by our sampling procedures and, therefore, the meaning of these tests must be interpreted with caution.

disaffected than white children about the government and political figures like the President, a pattern paralleling the findings for black and white adults. Moreover such differences appear as early as the fourth grade and tend to remain constant in their intensity among children in the higher grades.

Political Knowledge and Participation. While a child's political sentiments largely shape his lifelong political orientation,

equally important are the levels of information and styles of political activity which he develops. Almond and Verba (1963) note, for instance, that children who have been encouraged to take an active role in political discussions early in life are more likely to be active in politics as adults. In terms of the racial differences being considered here, current readings on black and white adults suggest that, if children do differ, black youngsters should be more in-

Table 2. Race, Grade in School, and Occupation of the Chief Wage Earner by Government Benevolence and Image of the President (mean scores on benevolence and image)

Grade in School	Chief Wage Earner Occupation	Government Benevolence			Image of the President		
		Black \bar{X}	White \bar{X}	Partial r (Occupation controlled)	Black \bar{X}	White \bar{X}	Partial r (Occupation controlled)
4-6	White Collar	6.57	6.84	-.143*	2.87	3.19	-.151*
	Blue Collar	6.84	7.13		3.18	3.65	
7-8	White Collar	6.21	6.77	-.202**	2.47	2.95	-.248**
	Blue Collar	6.30	6.70		2.59	3.08	
9-10	White Collar	6.16	6.35	-.071	2.44	2.62	-.183**
	Blue Collar	6.18	6.42		2.52	2.96	
11-12	White Collar	5.70	6.19	-.098	2.51	2.81	-.150**
	Blue Collar	5.89	6.15		2.52	2.79	

* Significant at .05 level.

** Significant at .01 level.

clined to be active politically (Olsen, 1970; Sears, 1969).

The children in our study were asked a series of six standard political information items, designed to tap their levels of knowledge and awareness about politics. Among these was the question: "How many years does a United States Senator serve?"⁸ Answers to this and other questions were coded as right or wrong, scored as 1 and 0, respectively, then combined in a cumulative index with scores ranging from 0 to 6. Table 3 presents the mean scores on the political knowledge index and the partial correlations between race and political knowledge, with socioeconomic status controlled. Inspection of the results reveals a tendency for black children to score higher than white on the

knowledge index only in the grade clusters, fourth through sixth, and ninth and tenth; in the other two clusters there is virtually no difference between the scores of the black and white children. The age and socioeconomic differences are as one would anticipate: higher scores are found among older children from white-collar families.

To investigate possible differences in political participation, the children's scores on two other scales derived from factor analysis were also examined. The first scale, referred to as political discussion, consisted of three questions, one of which asked if the child discussed political candidates with his parents. The second scale, referred to as political participation, contained three items also, including a question about whether the child ever participated in a political demonstration.⁹ Table 3 presents the mean scores for

⁸ Items included in the political knowledge scale are as follows:

How many years does a U.S. senator serve?

What country does Marshall Tito lead?

How many members are there on the U.S. Supreme Court?

To what political party did President Franklin Roosevelt belong?

Who is the present governor of Illinois?

Who is the mayor of your city?

The values on this scale ranged from 0.00 to 6.00.

⁹ The political discussion items and weights are as follows:

Have you talked a lot with your friends about a candidate? (weight = .67)

Has your teacher told you about the candidates? (weight = .56)

Have you talked with your parents about a candidate? (weight = .74)

Table 3. Race, Grade in School, and Occupation of the Chief Wage Earner by Political Knowledge, Discussion, and Participation (mean scores on knowledge, discussion, and participation)

Grade in School	Chief Wage Earner Occupation	Political Knowledge			Political Discussion			Political Participation		
		Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)	Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)	Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)
4-6	White Collar	1.13	.84	+.090	3.07	2.84	+.153*	2.81	2.64	+.098
	Blue Collar	.90	.60		2.96	2.74		2.64	2.54	
7-8	White Collar	1.94	1.84	+.016	3.17	3.12	+.037	2.73	2.86	+.002
	Blue Collar	1.34	1.15		3.04	2.95		2.82	2.59	
9-10	White Collar	2.57	2.70	+.077	3.19	3.21	+.086	2.75	2.93	+.029
	Blue Collar	2.39	2.16		3.19	3.09		2.69	2.61	
11-12	White Collar	2.89	2.91	-.019	3.46	3.21	+.136**	2.66	2.59	+.170**
	Blue Collar	2.55	2.56		3.31	3.12		2.69	2.40	

* Significant at .05 level.

** Significant at .01 level.

both scales as well as the partial correlations between each and race, with socioeconomic status controlled. For both scales, the differences are of varying magnitudes; but black children, like their adult counterparts, tend to be the more apt to be involved in political discussion and participation, particularly among the very youngest and oldest. The age and socioeconomic patterns of differences found for political knowledge hold true for political discussion as well, as does the socioeconomic pattern for political participation; however, age seems not related to participation.

On balance, the magnitude of differences between black and white children on be-

havioral and cognitive dimensions of politics is weaker and less consistent across age groups than are the affect measures. The very youngest black children, those in the fourth through sixth grades, clearly possess an edge over their white counterparts on both dimensions: they are the more knowledgeable and active with regard to politics. And, among the very oldest children, black youngsters are more likely than white to discuss politics and participate in political activities. For no apparent reason, racial differences in the behavioral and cognitive dimensions are almost nonexistent among children in grades seven through ten.

Political Partisanship. The fourth and final dimension of politics examined is the evaluative, or partisanship. Like the other three dimensions, this one is thought to develop fairly early in life and to have important impact on later perceptions and actions; indeed, it filters the range of political information a person receives, shaping and directing it. As Greenstein (1965:85) remarks: "The likelihood that early learning will have a vital effect on later learning seems to be especially great in the case of partisan motivations. Party identification,

The scores on the discussion scale ranged from 1.97 to 3.94. The political participation items and weights are as follows:

Have you ever worn a campaign button for a candidate? (weight = .66)

Have you helped a candidate by doing things for him such as handing out buttons or papers with his name on them? (weight = .79)

Have you, yourself, ever been involved in a political demonstration, like a march, a picket line, or a strike, other than a union strike? (weight = .54)

The scores on the participation scale ranged from 1.99 to 3.98.

learned as it is uncritically and at an early age, can readily become an 'experiential filter.' Our examination so far would lead us to anticipate a difference between black and white children, and perhaps, to find the black children more partisan, as is the case for the typical adult (Olsen, 1970).

To determine the children's partisan identification, we developed a measure, party preference, based on responses to the query: "If you could vote, which of the following would you be?" The children were grouped into two clusters, those who chose to vote for one party, Democrat, Republican, or other, and those who indicated no definite party preference. The former responses were given a score of 1, and the latter a score of 0. The mean scores on the children's party preference, which can be found in Table 4, thus represent the proportions of children who expressed a definite choice for one party over another.

We find two obvious patterns: black children are more likely than white to express a definite party preference, irrespective of grade or socioeconomic status; and the difference in the proportions of black and white children who express a definite party choice increases dramatically among older

children. The latter is the result of opposing tendencies among black and white youngsters: as one moves from lower to higher grades, black children increasingly tend to choose a definite party, especially those from blue-collar backgrounds; while white children show a diminishing tendency for such a choice.

For added insight into the formation of party identification and preference among black and white children, we developed a scale designed to uncover the children's perception of differences between the two major political parties.¹⁰ Previous studies of early political socialization have suggested that

¹⁰ The items included in the party differences scale, which possessed values of 0.00 to 40.00, are as follows:

Which party does more for rich people?

Which party does more to keep us out of war?

Which party does more to help people without jobs?

Which party does more to protect the rights of citizens?

Which party does more for your family?

Which party does more for the country?

How much difference is there between Democrats and Republicans?

If the Democrats and the Republicans disagreed on important things, would it be bad for the country, make no difference, or be good for the country?

Table 4. Race, Grade in School, and Occupation of the Chief Wage Earner by Political Party Preference, Recognition of Party Differences and Image of Father (mean scores on preference, differences, and father's image)

Grade in School	Chief Wage Earner Occupation	Party Preference			Party Differences			Father's Image		
		Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)	Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)	Black \bar{X}	White \bar{X}	Partial r (Occupation Controlled)
4-6	White Collar	.55	.44	+.028	21.08	16.36	+.168*	3.97	4.17	+.028
	Blue Collar	.51	.52		20.83	16.95		4.04	3.86	
7-8	White Collar	.54	.47	+.120	20.82	16.31	+.211*	4.39	4.39	+.002
	Blue Collar	.67	.56		22.40	18.25		4.34	4.26	
9-10	White Collar	.45	.27	+.267*	20.59	16.49	+.199*	4.84	4.40	+.075
	Blue Collar	.62	.32		21.51	17.26		4.60	4.53	
11-12	White Collar	.63	.25	+.343*	24.04	15.76	+.264*	4.66	4.31	+.078
	Blue Collar	.56	.20		22.22	16.72		4.70	4.54	

* Significant at .01 level.

such a sense of party differences develops concurrently with the child's cognitive capacities; older children being more apt to perceive differences than younger ones (Greenstein, 1965). Among the items included in this scale were the following: "How much difference is there between Democrats and Republicans?" and "Which party does more to keep us out of war?" For the partisanship scale, a child was given a score of 5 for each highly partisan response, choosing one party over another or indicating large differences between parties, and a score of 0 if he responded with an answer of "no difference." A score of 3 was given when moderate differences were indicated. These scores then were summed to form a cumulative index. If we turn again to Table 4, we find that black children consistently score higher than white on their perception of differences between the two major political parties, a tendency more or less in keeping with their greater inclination to express a definite party preference.

Sources of Differences: Psychodynamic or Subcultural?

As Jaros and his colleagues (1968) so aptly point out, two general principles are thought to account for the early development of political beliefs and sentiments. These principles would help us pinpoint the sources of differences between black and white youngsters. The psychodynamic principle suggests that young children's political feelings reflect a tendency to generalize sentiments about authority figures in the home to authority figures in the political world. Easton and Hess (1962:242) put it most succinctly: "... children display a strong tendency to generalize attitudes developed in connection with authority beyond their knowledge and direct contact. . . . The child not only learns to respect and admire political authorities, but with regard to many characteristics sees them as parents writ large (see also Greenstein, 1965:45-7)." Our data do not support this theory. We find that children who, in general, express positive sentiments toward their fathers are no more likely to have highly favorable feelings for such political figures as the President than are children with less positive feelings toward their fathers.

If, however, this psychodynamic model validly explained the sources of black youngsters' more negative attitudes toward politics, we would expect to find those in the elementary grades consistently holding more negative images of their fathers than white children, as some literature on father-absence and authority relations in the homes of black children suggests (Pettigrew, 1964). The last set of scores and partials in Table 4, under the heading "Father's Image," refer to a scale of items similar in content to those comprising the earlier image scale on the President: higher scores on this scale represent more favorable attitudes toward a child's father. Examining these scores and partial correlations closely, we find that in grades four through eight, there is no clear or consistent difference in black and white children's attitudes toward their fathers; while in the higher grades black children tend to have slightly more favorable images of their fathers.

The second principle that might account for political differences between black and white children is what Jaros and his colleagues (1968) take to be a subcultural, or ethnic community, principle [see Olsen (1970) for a similar argument]. This theory suggests that children reflect the behavior and feelings taught them by their parents, peers and educational institutions; while the latter, in turn, transmit values which are part of a subculture within the larger society. This argument would seem germane to the political beliefs of black children in America especially in view of the growing sense of community and collective identity among black Americans. Furthermore, it would help explain similarities in the attitudes and behavior of black youths and adults. In particular, it suggests that such similarities occur partly because blacks both young and old are exposed to the values of the larger black subculture, and partly because black parents transmit such values to their children.

To test the subculture theory we developed a scale for identification with the black community or, more simply, "black consciousness."¹¹ Assuming that the sub-

¹¹ The items included in the black consciousness scale, which had values of 3.48 to 13.92, and their weights are as follows:

culture model was valid, we expected that black children who identified more strongly with the black community would more pronouncedly exhibit the same political behavior and beliefs as obtained in the comparison of black and white children. For example, since black children were more likely than white to participate actively in politics, we anticipated that children who scored high on our measure of black consciousness would be more active than those who scored low. (This analysis, it should be mentioned, could only be performed among black children in grades seven through twelve since the younger children were not asked questions pertaining to black consciousness. Furthermore, the partisan measures are not included among items in this analysis; for there was little theoretical rationale to expect a relationship between such measures and black consciousness as might occur between political affect, for example, and black consciousness.) The partial correlations between black consciousness and the various political measures, con-

trolling for socioeconomic status and within grades are presented in Table 5. Close inspection of the signs and magnitudes of the various coefficients in this table reveals the results to be generally consistent with expectations based on the subcultural hypothesis. We see, for example, that black children who score high on their identification with the black community are consistently more cynical as well.

In summary, greater support is found for the subcultural than the psychodynamic explanation of black and white youngsters' differences in political behavior and belief.

Comparisons With Other Studies

Since this analysis reveals important differences in the political sentiments, activity and identification of black and white children, even among the very young, it is useful to compare its findings with those of similar investigations. Green's study of black children in a parochial school in Chicago (1972), Greenberg's study of children in Philadelphia (1970a; 1970b), and Lyons' study of children in Toledo public schools (1970), each suggest that black children are the more cynical and disaffected regarding political institutions and figures in America. These findings parallel our own. Jaros' earlier study of children in Detroit (1967), in contrast, found that the sentiments of black and white children did not differ with respect to the figure of the President. Unlike our study and Lyons', both of which found no clear pattern of change in the race differences in political beliefs or behavior as children mature, Greenberg uncovered an acceleration of the race difference in degree of disaffection from the third through ninth grades; black children, in other words, tended to be markedly more disaffected than their white counterparts in the higher grades.

Three other studies are pertinent, though they ignore children in the elementary grades. A nationwide survey of tenth grade boys by Bachman and his associates (1970) reveals that, with socioeconomic status and IQ controlled, "blacks (particularly those in southern segregated schools) have political knowledge scores *relatively higher* than whites (158)," a result consistent with

For the black, equality and integration are not the same things. (weight = .44)

It is up to the government to make sure that everyone has a secure job and a good standard of living. (weight = .40)

If black people are not getting fair treatment in jobs and housing, the government should see to it that they do. (weight = .68)

The "Afro" or "natural" hairstyle is dignified. (weight = .52)

Black officials have a duty to be honest and should be more protective of the black community. (weight = .55)

The government in Washington should see to it that white and black children go to the same schools. (weight = .39)

Many police departments are trying to wipe out militant black organizations and their leaders. (weight = .50)

Four of the items which went into this scale, a, d, e and g, were taken from a forty item scale designed to elicit a sense of black consciousness (Banks, 1970). Our set of seven items was the result of performing separate factor analyses on the information obtained from the black and white children. For the black youngsters, these seven items represented the first of seven factors—thirty-six items were factor analyzed—while for white children, these items *did not* factor together and were spread over four of the seven factors, indicating an absence of consistent response. We may safely conclude, therefore, that among black youngsters the scale of items represents some underlying continuum which we prefer to label "black consciousness."

Table 5. Selected Items for Black Consciousness within Grade and Controlled for Chief Wage Earner Occupation, for Black Children in Grades 7-12.

Item	Grade		
	7-8	9-10	11-12
Cynicism	+.28*	+.28*	+.29**
Benevolence	-.12*	-.07	-.02
President's Image	-.15**	-.19**	-.14**
Knowledge	+.26**	+.06	+.16**
Discussion	+.08	+.14*	+.14*
Activity	+.05	+.11	+.09

* Significant at .01 level.

** Significant at .05 level.

the pattern among our older children. In an examination of tenth to twelfth grade children in a Detroit high school, Ehman (1969) discovers as we did that black children tend to be the more cynical and active with regard to politics. On the other hand, a nationwide study of the political socialization of high school seniors, reported on by Langton and Jennings (1968), finds higher political knowledge scores among whites, and negligible differences in the political cynicism, interest and discussion scores of black and white children, all in contrast to our own results.

Such disparities between findings should not be dismissed lightly. The differing results might be explained by the size and location of the samples involved. The sample used in the present study is, far and away, the largest of black children—the Langton and Jennings findings use an unweighted sample of 186 black children—but it also tends to underrepresent children in the middle socioeconomic status groups and draws only from Illinois communities. Nevertheless, even nationwide studies are not consistent, as a comparison of findings from the Bachman study and Langton and Jennings article demonstrates. To further complicate matters, some studies do not systematically partial out the effects of socioeconomic status.

Before leaving this discussion, it is use-

ful to note certain similarities between our results and those of Jaros and his colleagues (1968) who compared the political beliefs of children in a white Appalachian subculture with those of a nationwide sample of white children. Though the white Appalachian subculture obviously differs from the black subculture, both represent deviations from the normal patterns of political sentiments in America insofar as their adult members are disaffected politically. And, as we found for black children, Jaros finds the early appearance of patterns of cynicism among white Appalachian children as well as evidence that such sentiments are transmitted through subcultural rather than psychodynamic mechanisms.

Conclusions

That black and white children show a disparity in political orientations comparable to that of their adult counterparts is of considerable importance. This finding should focus empirical and theoretical attention on the timing and location of the origins of political orientations, matters which sociologists interested in ethnic politics have tended to disregard.¹²

Two lines of inquiry immediately suggest themselves. The first assumes that inasmuch as black adults and children deviate similarly from whites, their outlook reflects a similar exposure to white discrimination in America. Their disaffection is simply a response to such discrimination. Some might even be inclined to dismiss the similarities with such a claim. However, to do so would leave unanswered the critical matter of how white discrimination is translated into everyday experiences that shape the political outlooks of children only nine years old.

A second line of inquiry observes that small groups of people, primary groups, are vital in transmitting social and political norms to children. One must therefore try to locate the groups most salient in the political socialization of black youngsters and identify the values they transmit. One might claim of the similar political patterns

¹² Political scientists have given us virtually all the political socialization literature. The only exception, in fact, is the work done by Herbert Hyman (1959).

of black children and adults for example, that parents simply are passing their own beliefs to their children, unsullied and unchanged. Yet this facile claim might be off target altogether, for recent evidence suggests that the parent-child correspondence in political attitudes is fairly low (Jennings and Niemi, 1968a, 1968b). Other groups of people obviously can play an important role in the political socialization process; for black ghetto youngsters, one such group might be the gang or peer group.

The concept of a black subculture, and the evidence for it in our analysis is promising for the study of the foundations of black youngsters' political beliefs. By definition, subcultures are characterized by distinctive sets of norms and values which govern all types of behavior, including political behavior. Identification with, or integration into, such a subculture can be regarded as a variable condition: some individuals and groups are more strongly knit into the subculture than others and, consequently, are more apt to exhibit its characteristic norms. The children in our study, for instance, varied widely in the degree to which they identified with the black community, that is, expressed a sense of "black consciousness" and, in turn, in the degree to which they were disaffected from the political institutions of whites and actively involved in politics. Further assessment of how black youngsters acquire their political orientations must account for the conditions and settings which promote identification with the black community.

Besides recommending detailed examination of the etiology of black children's political orientations, the findings underscore the continuing thrust for social and political change in the black community in America. Young black children are acquiring a more marked disaffection from the larger political system than their white counterparts, and at the same time becoming more alert and informed about politics. Given such inducements as the increasing hostility and discrimination of white Americans, the twin conditions of expertise and disaffection might easily be transformed into more widespread and effective protest and change.

This analysis raises more questions than it answers. We hope, however, that, like

ourselves, other students of black and white politics in America would seek a more complete explanation of the mechanisms that produce radically disparate political orientations of blacks and whites so early in life. Without such answers, the study of these politics will remain mere empirical description.

REFERENCES

- Almond, Gabriel and Sidney Verba
1963 *The Civic Culture*. Princeton, New Jersey: Princeton University Press.
- Bachman, Jerald G.
1970 *The Impact of Family Background and Intelligence on Tenth Grade Boys*. Survey Research Center: Michigan.
- Banks, Henry A.
1970 "Black consciousness: a student survey." *The Black Scholar* (September):44-51.
- Dawson, Richard and Kenneth Prewitt
1969 *Political Socialization*. Boston: Little, Brown and Company.
- Easton, David and Jack Dennis
1969 *Children in the Political System*. New York: McGraw-Hill.
- Easton, David and Robert Hess
1962 "The child's political world." *Midwest Journal of Political Science*, 6 (August): 229-46.
- Ehman, Lee H.
1969 "An analysis of the relationships of selected educational variables with the political socialization of high school students." *American Educational Research Journal*, 6 (November):559-80.
- Green, Eugene
1972 "The political socialization of black inner-city children." Pp. 180-94 in Anthony M. Orum (ed.), *The Seeds of Politics: Youth and Politics in America*. Englewood-Cliffs, New Jersey: Prentice-Hall, Inc.
- Greenberg, Edward S.
1969 "Children and the political community: a comparison across racial lines." *Canadian Journal of Political Science* 2 (December): 471-92.
- 1970a "Children and government: a comparison across racial lines." *Midwest Journal of Political Science* 14 (May):249-75.
- 1970b "Black children in the political system." *Public Opinion Quarterly* 34 (Fall):333-45.
- 1970c "Orientations of black and white children to political authority." *Social Science Quarterly* 51 (December):561-71.
- Greenstein, Fred I.
1965 *Children and Politics*. New Haven, Connecticut: Yale University Press.
- Hess, Robert D. and Judith V. Torney
1967 *The Development of Political Attitudes in Children*. Chicago: Aldine Press.

- Hyman, Herbert
1959 *Political Socialization*. Glencoe: Free Press.
- Hyman, Herbert H. and Paul B. Sheatsley
1950 "The current state of American public opinion." Pp. 11-34 in *National Council for Social Studies Yearbook*.
- Jaros, Dean
1967 "Children's orientations towards the president: some additional theoretical considerations and data." *Journal of Politics*, 29 (May):368-87.
- Jennings, M. Kent and Richard Niemi
1968a "The transmission of political values from parent to child." *American Political Science Review* 62 (March):169-84.
1968b "Patterns of political learning." *Harvard Educational Review* 30 (Summer):443-67.
- Langton, Kenneth P. and M. Kent Jennings
1968 "Political socialization and the high school civics curriculum in the United States." *American Political Science Review* 62 (September):852-67.
- Lyons, Schley
1970 "The political socialization of ghetto children: efficacy and cynicism." *Journal of Politics* 32:288-304.
- Olsen, Marvin E.
1970 "Social and political participation of blacks." *American Sociological Review*, 35 (August):582-97.
- Orum, Anthony M.
1966 "A reappraisal of the social and political participation of Negroes." *American Journal of Sociology* 72 (July):32-46.
- Pettigrew, Thomas F.
1964 *A Profile of the Negro American*. Princeton, New Jersey: D. Van Nostrand Company, Inc.
- Sears, David O.
1969 "Black attitudes toward the political system in the aftermath of the Watts insurrection." *Midwest Journal of Political Science* 12 (November):515-44.

INCOME AND VETERAN STATUS: VARIATIONS AMONG MEXICAN AMERICANS, BLACKS AND ANGLOS

HARLEY L. BROWNING, SALLY C. LOPREATO AND DUDLEY L. POSTON, JR.

The University of Texas at Austin

American Sociological Review 1973, Vol. 38 (February):74-85

While the effects of social origin variables on the status attainment process of individuals are well recognized, the influence of career contingencies—events occurring subsequent to the determination of social origin status—are less well explored. Using the 1/100 Public Use Sample of the 1960 U.S. Census, we examine the effects of one career contingency—military service—with respect to current income for three ethnic groups, Mexican Americans, blacks, and Anglos in five Southwestern states. Contrary to expectations based on Anglo-dominated statistics in which nonveterans report higher average income than veterans, among both blacks and Mexican Americans, veterans have higher average income than nonveterans. The concept of a "bridging environment" as applied to military service is used to interpret the minority patterns.

MUCH of the history of sociology has resolved around attempts to identify the variables that determine an individual's position in the stratification system of his society. How a person becomes located in that system has become known as "the process of stratification," to use the phrase of perhaps the most influential work in the field, *The American Occupational Structure*, by Blau and Duncan (1967). Their basic path analytic model follows a socioeconomic life cycle approach. Beginning with social origin variables (e.g. father's occupation and education), the sequence proceeds through the man's educational attainment, his first occupation, and then one or more subsequent occupational statuses.

A number of investigators have worked to expand this basic model to include other variables that affect status attainment. One such variable Duncan and his associates (1972) have termed "career contingencies." Included here are events such as migration, disruption of marriage, child spacing, and military service, all of which occur subsequent to the determination of social origin (many after the individual's own educational attainment), but may still have a bearing on status attainment. These events are contingent because they do not affect all individuals, and because they can occur within a rather wide interval of the life cycle. Moreover, in comparison with most other variables in the basic model, career contingencies often

are nominal rather than interval variables—either a man is a migrant or a veteran or he is not (cf. Schnore, 1961:412–13). While career contingency variables have received less attention than others, they can have a substantial impact on the process of stratification. The present study focuses on one such contingency—military service—which, in view of the American penchant to become involved in extraterritorial armed conflicts requiring more than a volunteer military force, carries a high risk of occurrence for American males.

The connections between military service and status attainment of males can be fruitfully studied in conjunction with another topic of prominent and enduring concern in American sociology, the location of certain minorities, principally blacks and Mexican Americans, in the stratification system. In the investigation of stratification patterns one incontestable finding is that blacks and Mexican Americans fall far short of Anglo attainment in education, occupation and income. Generally, the locus for the failure of such groups to approximate the Anglo distributions in these indices is placed either in structural factors (including institutionalized discrimination) or in ethnic factors that impede assimilation (cf., for example, Grebler, *et al.*, 1970). Given the grossly different life circumstances of individuals from minority versus majority backgrounds, whatever their “causes,” the impact of any career contingency—in particular, military service—is likely to have a varying impact depending on that background. It is the differential effect of military service on the status attainment of blacks, Mexican Americans and Anglos that we will attempt to determine in this paper.

VETERAN STATUS, INCOME, AND ETHNICITY

Income is one of the most powerful indicators of an individual's status achievement, especially in terms of personal security and well-being; and it is used in this analysis as the dependent variable. An abundant literature on the relationship between veteran status and income has been developed by economists, whose general argument is that military service results in income penalties for the individual (Miller and Tollison,

1971; Bailey and Cargill, 1969; Davis and Palomba, 1968; Willett, 1968; Oi, 1967; Hansen and Weisbrod, 1967). One reason for this is the differential pay scale in military versus civilian employment; except perhaps for the very bottom ranks of civilian occupations, military pay is comparatively lower. Thus, the veteran may never quite “catch up” with his nonveteran peer who has several years advantage in the civilian labor market. Indeed, estimates of lost lifetime earnings run as high as \$9,000 for men who enter military service with a college degree (Miller and Tollison, 1971:930).

A second, and related, reason for anticipating an unfavorable comparison of veterans with nonveterans, whether based on lifetime earnings or on current income, concerns the interruption of continuity in the individual's work career. Two years or more of military service will often interfere with the completion of higher education, delay the fulfillment of an apprenticeship, or impede the acquisition of on-the-job training skills, all of which are convertible into higher income at some future time. In addition, most work positions, to one degree or another, provide remuneration on the basis of seniority; and this too acts to the advantage of nonveterans.¹

Taking the above considerations into account, it is understandable that economists have viewed veteran status as a depressant on both current and lifetime income. Several facts lead us to suspect, however, that individuals with low socioeconomic backgrounds might gain from military training and other less tangible attributes of military life the capacity to *increase* their post-military civilian earning power over non-veterans with similar backgrounds.² Given the significance

¹ The Selective Service Act of 1940 made it mandatory for employers to restore the veteran to the position he held *before* leaving, or to a position of like seniority, status, and pay *unless* “the employer's circumstances have so changed as to make it impossible or unreasonable to do so” (Selective Service System, 1949).

² We are not attempting to give an over-all picture of the advantages and disadvantages of military life for ethnic minorities. The most obvious example is that post-military advantages are partly offset by high casualty rates among lower socioeconomic groups in the military (Leigh and Berney, 1971).

of the primary variables of paternal educational and occupational attainments and the individual's own educational level in determining his subsequent occupational position and income, and given the historical and present-day context of discrimination in which these factors operate, black and Mexican American men do not enter the labor market on a par with Anglos. Among other things, they grow up in the ghettos or other segregated areas which isolate them from socialization into the mainstream of their society; and they attend inferior schools which provide them with a deficient educational background.

The mechanisms by which military service could actually alter the unfavorable mobility conditions that confront the great majority of Mexican Americans and blacks are several and may be summarily referred to as part of a "bridging environment," a term adapted from Broom and Smith's (1963) discussion of bridging occupations. According to these authors, a bridging occupation "is one which provides, through work experience, the conditions and opportunities for movement from one occupation or cluster of occupations to another" (Broom and Smith, 1963:322). Rather than viewing military service as a specific occupation, we believe it appropriate to consider the experience more broadly as an environment in which the individual may acquire new skills and abilities, which, after military service, could help him in his civilian career.

There are several ways in which military service could provide a bridging environment for members of ethnic minorities.³ One is through the radical discontinuity of civilian and military life, an abrupt and even traumatic experience that facilitates the resocialization of men to the modes of behavior of military organizations. "Americans from widely different class, ethnic, and religious backgrounds [are] stripped of old identities and coerced to accept new military roles . . ." (Vidich and Stein, 1960:498). One function of this process is to emphasize individual independence through the "severing or curtailment of primary ties to kin or community"

(Broom and Smith, 1963:323). As they suggest, "independency" is further facilitated by the fact that military service nearly always implies geographic mobility. The separation from family and kinship ties and neighborhood groupings is particularly important in the case of Mexican Americans who, as a group, have continued through the years to live in ethnic enclaves (Sena Rivera, 1971; Galarza *et al.*, 1969). Residential segregation no doubt has played a major part in forcing the continuance of this pattern, but the maintenance of strong ethnic boundaries by the group itself is also important (Mittelbach and Moore, 1968). These ethnic boundaries are precisely the ties which resocialization into military life weakens for the individual. Relatedly, the forced "integration" of blacks, Mexican Americans, and Anglos in the same living quarters, while it does not by any means imply either acceptance or acculturation, does represent profoundly different conditions of association for many minority men.

A second way in which military service constitutes a bridging environment is in the acquisition of further education and job training. The General Educational Development program of the armed services certifies 10,000 servicemen yearly with a high school equivalency diploma. According to Moskos (1970:61), "during peacetime (i.e., between Korea and Vietnam) between 20 and 25 percent of all Army enlisted personnel completed their high school education while serving on active duty." The Tuition Aid program helps pay fees for servicemen taking college resident courses during their off-duty hours; and beyond their time in the service, the G.I. bill has aided millions of veterans in continuing or completing their education.

Probably at least as important for ethnic minorities as the educational programs are the opportunities to improve one's job skills. Biderman and Sharp (1968), for example, report a high degree of congruence between military and civilian occupational structures. Drawing upon a 1963 Department of Defense survey, they cite a broad range of "transferable skills," which apply both to officers and enlisted men, although a closer relationship is reported between civilian and military skills for enlisted men than for officers. These observations parallel those of

³ This paper is concerned with advantages or disadvantages of military service for subsequent civilian employment, and not with the military as a career line for minority group individuals.

Lang (1964:45) who notes that "in terms of enlisted men's occupational distribution, the military establishment stands out as one of the more technologically advanced sectors of American society." Further, "the military had a higher representation in precisely those occupational groups which between 1940 and 1960 registered the greatest gains in the labor force . . ." (Lang, 1964: 45). Among such groups, Lang lists professional, technical, and kindred workers; managers and officials; clerical workers; and mechanics and repairmen.

Still a third way in which military service can positively affect the status attainment of ethnic minorities is by increasing their capability to cope with and manipulate the large-scale organizational structures that increasingly typify U.S. society. Vidich and Stein (1960:493) cogently argue that the military, as a prototype bureaucratic structure "stand[s] as a model, in point of historical origin and social efficiency, for the factory, the prison, the office, the department store, the corporation, and increasingly for the 'social system' as a whole."

This characteristic is of particular importance for blacks and Mexican Americans who, in order to make occupational strides, must effect a transition from segregated ethnic-centered backgrounds into ways of life that are highly specialized and organized. In the military, the individual learns to live within bureaucratic structures and to subvert and manipulate the rules and regulations to his own advantage. He can develop, as well, his organizational expertise in mobilizing other individuals. This knowledge eventually can be very valuable both in obtaining civilian jobs and in the coordination of ethnic activities.⁴

To summarize, for many, if not all, ethnic

minority men, military service can positively affect their subsequent chances in the opportunity structure of civilian society. Geographic mobility and personal independence, education, occupational training of various kinds, and experience in bureaucratic structures, all make it easier for the veteran to obtain those civilian jobs that provide better pay.

At this point we can state more succinctly our expectations regarding the relationship between income and veteran status among the ethnic groups. In the first place we predict that, in line with the findings of the economists, Anglo veterans will be at an economic disadvantage vis-a-vis Anglo nonveterans; that is, veterans will earn *less than* nonveterans. Conversely, among blacks and Mexican Americans, we anticipate that, on the average, veterans will earn *more than* nonveterans from the same group. Since earlier research has not differentiated individuals by ethnicity, the assumption seems to have been that military service incurs a "cost" for all men. The intent of this paper is to put this assumption to test.

Second, in line with our earlier discussion of the importance of career or job continuity, we predict that income differences between minority group veterans and nonveterans for specific occupations will be characterized as follows: (a) those occupations most affected by career continuity (professional, technical and kindred workers; managers, officials and proprietors; and craftsmen and kindred workers) will show the lowest or even negative income differentials in comparisons involving veterans and nonveterans; (b) the occupational categories least affected by career continuity (clerical workers; sales workers; operators; service workers; and laborers) will show the greatest positive income differentials comparing veterans and nonveterans. We suggest that in these latter occupations the effects of military life as a bridging environment will be manifested among blacks and Mexican Americans, and that an income benefit will exist for the veteran.⁵

⁴For instance, Galarza *et al.* (1969:51) note that "After World War II much of the organizing vitality among the Southwest's largest minority [the Mexican Americans] was provided by the young war veterans . . . [who] were seeking more effective and satisfying roles for Mexican Americans." The impact of military service on racial leadership among blacks is well recognized in the literature (Moskos, 1970:108-33; Coates and Pellegrin, 1965:337-54; and Chiricos, Pearson, and Fendrick, 1970). Much of this organizational activity is of course not related to jobs and income as such, but it does imply abilities which could have an impact for the minority individual in the labor market.

⁵The possibility of turning military experience into an income gain depends largely on the state of the receiving economy. This fact is most evident in comparing the 11% unemployment rate for Vietnam veterans with the 4.9% rate for returning World War II veterans (New Republic, 1971). In

THE NATURE OF THE SAMPLE

The data employed in this analysis were taken from the 1/100 1960 Public Use Sample (U.S. Bureau of the Census, 1971) and include all men fourteen years and over who were classified as veteran or nonveteran. The 1/100 Public Use Sample permits the investigator to engage in secondary analysis by tabulating veteran status with any other variable gathered in the census schedule. The investigator therefore is not dependent solely upon published tabulations.

This way of handling census data is important not only in terms of the analysis itself, but also in drawing the sample. We do not simply compare all veterans with all nonveterans by the three ethnic groups, for this could make for misleading conclusions. Essentially, we want to ensure that the veteran-nonveteran distinctions are as comparable as possible for all three ethnic groups; and this means eliminating a number of men from both veteran and nonveteran categories.

First, we have made an areal restriction by considering only men from the five southwestern states of Arizona, California, Colorado, New Mexico and Texas—a necessary procedure since only in these states were Spanish-surname data collected.⁶ For Anglo (white, non Spanish surname) and black men we had two alternatives: take only those workers from the Southwest or draw from the entire United States sample. We chose the former. Since there are regional differences in wages and hiring practices and of course the presence in significant numbers of blacks and Mexican Americans, we believe it preferable to have all three ethnic groups represented from the same region.

the first quarter of 1971, the unemployment rate for white war veterans was 10.8 compared to 15.1 for Negro and other races. We are engaged in a cohort analysis, using the 1970 Public Use Sample of veterans from World War II, the Korean War, and the Vietnam War, in order to examine the veteran-nonveteran differences as they are affected by changes in the economy and social structure.

⁶ Throughout this paper we shall refer to Mexican-American men rather than the more technically accurate designation of Spanish-surname men. In like manner we shall refer to blacks rather than Negroes. Within the Southwest, Anglo is the customary designation for whites who are not Mexican Americans.

The second basis for restriction is age. We included only men between the ages twenty-five and fifty. The lower limit was set because by that age a man will have either completed his military obligation or will not likely be called for service. By age twenty-five most men will be established in the labor force and their incomes should reflect this experience. The upper age limit of fifty was chosen to restrict the sample to veterans serving during World War II and after.⁷

The third exclusion was all men with farm occupations (farmers and farm managers, and farm laborers and foremen). Our conception of the impact of military service on ethnic males is based on drawing such men from urban, often ghetto, environments. It is not clear that the bridging environment would operate in the same manner for minority males from rural areas. In any event, the five southwestern states are heavily urban in 1960, with three of every four men living in metropolitan areas; so the number of men in farm occupations are quite small.

The fourth criterion for elimination was educational attainment. If education were not taken into account, the veteran-nonveteran comparison for the three groups would be affected by the lower educational attainment of the two minority groups. Since the military exclude functional illiterates from service, we eliminated all men from our sample with less than five years of schooling. Therefore, those men included in the nonveteran category are presumed to have been as capable of passing the Armed Forces educational entrance examinations as were the veterans.

The fifth and last criterion for exclusion is based on the part- and full-time worker distinction. Obviously, any study of income differences among groups is bound to be affected by differing proportions of part-time workers. If one group is characterized by a higher proportion of part-time workers than another, one could argue that the lower income of this group is due to its heavier representation of part-time workers and not

⁷ Another reason for the age limit of fifty is that few minority group individuals over that age in 1960 had served in the U.S. armed services. Note also that the veteran sample includes men with peace-time service, not just men with war experience.

some other group characteristic such as veteran status. To preclude such an interpretation, our sample is restricted to full-time workers.⁸

The five bases for excluding men from the sample were established to make the three ethnic groups more comparable, so that when the veteran-nonveteran distinction is made, there will be as few confounding effects as possible. Doubtless this effort has not been entirely successful, but we are confident that it makes for more meaningful comparisons than would have been possible had *all* veterans and *all* nonveterans been included.

Of course, the constraints we have introduced have greatly effected the proportion of men eligible for the veteran-nonveteran comparisons. In Table 1 data are presented which show the proportion of the three ethnic groups who qualified on all five counts. For all occupations the figures are 55.5 per-

cent for Anglos, 47.6 percent for blacks and 44.3 percent for Mexican Americans. In other words, roughly one-half of the men in the 1/100 Public Use Sample are included in our analysis. There is considerable variation by occupational categories, however. For example, among the Mexican Americans nearly two-thirds of professional men are included; whereas only one-fourth of the laborers are included, the difference being accountable mainly to education and full versus part-time employment.

The data in Table 1 clearly indicate that the populations to be investigated in this report are in many ways different from the total populations. Therefore, generalizations from our sample to the total population of male workers is unjustified. It will be important to keep this in mind throughout the analysis, and we shall return to this point in the concluding section.

FINDINGS

This analysis predicts that the relationship between veteran status and income should vary by ethnic group. Specifically, Anglo veterans should be at an income disadvantage

⁸ Since the Census enumeration schedule does not include a direct question on full-time employment, we included all men (veterans and nonveterans) who both: (1) worked forty or more weeks in 1959, and (2) worked thirty-five or more hours in the week preceding April 1, 1960, the date of the 1960 census enumeration.

Table 1. Sample Composition as Percentage of the Public Use Sample

Occupation	In Sample			Not in Sample		
	Mexican American	Black	Anglo	Mexican American	Black	Anglo
Professional	65.4 (174)	55.1 (76)	65.9 (4,231)	34.6 (92)	44.9 (62)	34.1 (2,189)
Managers	57.1 (157)	51.4 (36)	60.3 (3,918)	42.9 (118)	48.6 (34)	39.7 (2,579)
Clerical	57.2 (167)	69.3 (115)	51.4 (1,703)	42.8 (125)	30.7 (51)	48.6 (1,611)
Sales	50.7 (103)	51.2 (21)	53.9 (2,029)	49.3 (100)	48.8 (20)	46.1 (1,736)
Craftsman	52.7 (505)	46.8 (170)	57.6 (5,778)	47.3 (453)	53.2 (193)	42.4 (4,247)
Operatives	44.1 (615)	53.8 (436)	52.9 (3,999)	55.9 (780)	46.2 (375)	47.1 (3,561)
Service	35.6 (171)	43.4 (315)	40.9 (940)	64.4 (310)	56.6 (410)	59.1 (1,357)
Laborers	25.8 (248)	39.1 (305)	33.7 (688)	74.2 (714)	60.9 (475)	66.3 (1,354)
All occupations	44.3 (2,140)	47.6 (1,474)	55.5 (23,286)	55.7 (2,692)	52.4 (1,620)	44.5 (18,634)

in comparison with their nonveteran counterparts, a prediction consistent with the findings of economists. However, the two minority groups, blacks and Mexican Americans, should display the opposite relationship, that is, the veterans should have higher incomes than the nonveterans.

To test this argument the income difference between veterans and nonveterans by ethnic status was calculated after controlling for the effects of occupational and educational composition.⁹ In other words, we wanted to determine the average dollar difference between the incomes of veterans and nonveterans that would exist if both groups had the same educational and occupational compositions.

The results for each of the three ethnic groups are presented in Table 2. For Anglos, the gross mean income difference between veterans and nonveterans is over \$100 in favor of the veteran (column 1). But when we ask what the income difference would be

if both groups were characterized by the same compositional features, the sign shifts and the nonveteran shows a \$167 advantage over the veteran (column 3). This finding is in agreement with earlier research (e.g., Miller and Tollison, 1971).

The findings in Table 2 further support the prediction that for both blacks and Mexican Americans, there is an economic advantage to being a veteran. The gross differences of \$344 and \$711 respectively are substantially reduced by compositional effects, but black veterans still have an advantage of \$163 over nonveterans, and the comparable figure for Mexican Americans is more than double that figure, \$387. These dollar differences should be interpreted in the context of the large differences in total income for Anglos as compared to the two minorities. Since black and Mexican American incomes are relatively similar (Browning and McLemore, 1964:61), we may conclude that the income gains accruing to Mexican American veterans are substantially greater than those to black veterans.¹⁰

At this point we turn to the second major prediction, which is focused on occupational groups. We have argued that occupation-specific differences between veteran and nonveteran incomes should be minimal (perhaps even reversed in favor of nonveterans) in

⁹ If we let v represent mean income of veterans and V represent mean income of nonveterans, the gross mean differences may be represented by

$$(1) v - V = \sum_{ij} n_{ij} v_{ij} - \sum_{ij} N_{ij} V_{ij}$$

where n_{ij} represents the proportion of nonveterans in the i th occupational category and in the j th level of educational attainment, and N_{ij} and V_{ij} represent the corresponding proportions and mean incomes for nonveterans. We expand (1) to

$$(2) v - V = \sum_{ij} n_{ij} v_{ij} + \sum_{ij} N_{ij} v_{ij} - \sum_{ij} N_{ij} V_{ij} - \sum_{ij} N_{ij} v_{ij}$$

This formula may be regrouped and rewritten as

$$(3) v - V = \sum_{ij} N_{ij} (v_{ij} - V_{ij}) + \sum_{ij} v_{ij} (n_{ij} - N_{ij})$$

The first term on the right side of (3) is the sum over all nonveterans of the veteran-nonveteran mean income differences within each occupational category and level of educational attainment. The second term of (3) is that portion of the gross differences due to occupational and educational composition. This methodology is adapted from Siegel (1965:54); see also Kitagawa (1955:1168-94).

¹⁰ One possible reason for the veteran advantage in income is a difference in age distribution between the two groups. If, on the average, veterans are older than nonveterans, one could argue that their greater skills and seniority would provide them with higher incomes than the younger nonveterans. We examined the age distributions of the two groups by ethnic status and found that for all categories veterans were somewhat younger than nonveterans. This suggests that had we controlled for age composition, the income differences would have increased rather than decreased, thereby lending greater force to our argument.

Table 2. Decomposition of Mean Differences between Veteran and Nonveteran Income for Three Ethnic Groups

Ethnic Group	Mean Difference Veteran-Nonveteran Income	Mean Income Difference Attributable to Composition	Mean Income Difference Net of Composition
	(1)	(2)	(3)
Mexican Americans	\$711	\$324	\$387
Blacks	\$344	\$181	\$163
Anglos	\$122	\$289	-\$167

those occupations requiring career continuity where absence due to military service may well handicap a man's income. For the other occupational categories we expect income differences between veterans and non-veterans to be considerably larger and always to the advantage of the veterans.

Following the same procedure used in comparing all veterans and nonveterans by ethnic status (Table 2), we present in Table 3 the occupation-specific income differences between veterans and nonveterans exclusive of the effects of educational composition. In other words, we compute for each occupational category the income differences that would be obtained between veterans and nonveterans if both groups were characterized by the same educational compositions.¹¹

¹¹ The methodology is similar to that described

Directing attention first to Mexican Americans (panel 1 of Table 3), we see that the three occupations emphasizing career development (professionals, managers, and craftsmen) are characterized as well by low income differences, positive or negative, between veterans and nonveterans. For these three occupations the minority veteran has no economic advantage over his nonveteran counterpart. However, among the sales, clerical, operative, service and laborer occupations, the veteran demonstrated a decided economic gain compared to the non-veteran, as predicted. In sum these findings are quite divergent from the economists' reports. Not only do Mexican American

in footnote 9 with the exception that the *i* subscript is deleted; only educational composition of the veteran and nonveteran groups is held constant.

Table 3. Decomposition of Mean Differences between Veteran and Nonveteran Income in Specific Occupational Groups

Occupational Group	Mean Veteran Income	Difference in Veteran, Nonveteran Income	Difference Attributable to Education	Difference Net of Education
Mexican American				
Professionals	\$7175.	\$207.	\$134.	\$ 73.
Managers	6533.	175.	274.	- 99.
Clerical	5089.	967.	142.	825.
Sales	6001.	1222.	367.	855.
Craftsman	5355.	131.	158.	- 27.
Operatives	4807.	490.	151.	339.
Service	4471.	1133.	323.	810.
Laborers	4891.	1225.	287.	938.
Total		711.	324.	387.
Blacks				
Clerical	4831.	648.	482.	166.
Craftsman	4492.	57.	- 14.	71.
Operatives	4025.	191.	128.	63.
Service	3492.	364.	64.	300.
Laborers	3994.	434.	218.	216.
Total		344.	181.	163.
Anglos				
Professionals	8892.	453.	143.	310.
Managers	8762.	-716.	408.	-1125.
Clerical	5956.	- 62.	44.	- 106.
Sales	7502.	74.	269.	- 195.
Craftsman	6421.	6.	87.	- 81.
Operatives	5797.	178.	95.	83.
Service	5584.	185.	157.	28.
Laborers	5075.	238.	114.	124.
Total		122.	289.	- 167.

veterans earn greater incomes on the average than nonveterans, but the income advantages are greatest in precisely those occupations where the bridging attributes of the military environment, i.e., geographic mobility, exposure to bureaucratic operations, and skill acquisitions, have the most impact.

In the case of the black-veteran-non-veteran comparison (panel 2) the pattern is not as clear, partly because the categories of professionals, managers and sales contain insufficient cases to warrant reporting.¹² For craftsmen, the one category for which we can assume career continuity to have an effect, we find a positive but low (\$71) income difference. In the four occupations where we would expect veterans to exhibit definite income advantages vis-a-vis non-veterans, the evidence is supportive. Although the income difference for operatives is quite small, the differences in the other three categories are substantial. As with overall income differences, the dollar discrepancy for blacks by occupations is not as great as it is for the Mexican Americans. Nevertheless, black veterans in clerical, service and laborer occupations do show definite economic advantages over their non-veteran counterparts.

Returning to the dollar differences for the eight occupational categories of the Mexican Americans, we note an odd pattern that requires comment. It is understandable that the clerical and sales categories show considerable differences between veterans and nonveterans because the service experience should be good preparation for these kinds of jobs, but why should veterans at the very lowest levels of the occupational hierarchy—service and laborers—do so well in comparison with nonveterans? We believe this effect is mainly an artifact of our sample of veterans and nonveterans. Recall that only 35.6 percent of service workers and 25.8 percent of laborers were selected (Table 1). These low proportions result from the educational and full-time work criteria used which excluded those men with the most deficient education and sporadic work patterns. Pre-

sumably the men included in our sample represent the elite of the low-skill workers.

Finally, a survey of the occupation-specific findings for Anglos is in order (panel 3 of Table 3). Note first, that the positive income difference attributed to veterans in the professional category (\$310) is in complete contradiction to prior research and our own expectation. On closer scrutiny, we found that the mean income of physicians and dentists who were veterans was approximately \$4,000 higher than the mean income of their nonveteran counterparts. Moreover, the veteran sample of professionals contained twice as many physicians and dentists (6.7 percent) as did the non-veteran sample of professionals (3.6 percent). It is likely, therefore, that the positive income differential of \$310 is in part attributable to the difference in mean earnings of physician and dentist veterans and non-veterans. Whatever the explanation, it is obvious that more detailed analysis of veteran professionals would lead to modification of the assumption that veteran status necessarily deflates the post-military earnings of this group.

The next three occupational categories show an income detriment for veterans, but the differential narrows from the higher to lower categories. The bottom three groups (operatives, service workers and laborers) reveal a positive income increment for Anglo veterans in comparison to Anglo non-veterans, although it is not as great as increments for the minority groups. This finding is consistent with our theoretical context. The same bridging environment which works in a positive manner for minority group individuals could just as well do so for Anglo individuals of lower socioeconomic backgrounds—the source of most of the men in the last three occupations. These findings are indeed significant, for they indicate a positive aspect of military life even for segments of the adult male Anglo population.

DISCUSSION

Overall, the data support our contention that income differences between veterans and nonveterans vary by ethnic status. The two minority groups examined, blacks and Mexican Americans, display a pattern discrepant with that of Anglos. With the exception of

¹² The Southwest regional restriction was necessary to make the three ethnic groups comparable. A study of veteran-nonveteran differences among blacks and whites is underway employing the 1970 Public Use Sample for the entire country.

two cases—Mexican American managers and craftsmen—the veteran has an income advantage compared to the nonveteran for both minority groups. This finding is in contrast to the literature on the subject, but it can be accounted for by the general practice of restricting analysis to Anglos or of taking all men without controlling for ethnic background. In the latter case the Anglos dominate the statistics.

The evidence also substantiates the second major prediction: for those occupational categories which have an observable career sequence, that is, where one's prior training and seniority insure a progressive increment in income, there is little or no advantage for blacks or Mexican Americans in being a veteran. For the other occupational categories there is a distinct advantage. (The minority comparisons are handicapped by the restricted number of occupational categories that could be run for the blacks—five compared to eight for the Mexican Americans.) The dollar advantage accruing to Mexican Americans is much greater than for blacks, even after allowing for the somewhat higher incomes that Mexican Americans have for the same occupational categories (cf. Table 3).

The fact that Mexican American veterans do better in their comparisons with nonveterans than do black veterans poses a question about which we can only speculate. Basically, the thrust of our argument is that the advantages accruing to veterans of both minorities are a consequence of the bridging environment that service experience provides. As noted, the military can provide further education, acquisition of skills and valuable experience in managing large-scale bureaucratic environments. It also takes the men from their ghetto and ethnic environments and more fully exposes them to the bureaucratic Anglo world.

But if this is true, why should Mexican Americans derive more advantage from the experience—at least as measured in dollar differences—than do blacks? We cannot say for sure. Perhaps the "independency" that military service provides has a greater impact on Mexican Americans than on blacks. It may be that the ties of the kinship network and of the neighborhood are strange for Mexican Americans. Language is also

a factor. In the Southwest the language of the *barrio* is Spanish, and many men may speak very little English during the course of the day. In fact, inability to speak English, or to use it readily, is often cited as the one most handicapping trait for Mexican Americans (cf., for example, the NEA—Tucson Survey, 1966). It is more difficult to continue this linguistic practice in the military, so one can assume that most Mexican American veterans have a greater command of English on leaving the service than entering it.

We can also presume that a black veteran will have more difficulty getting a good job than will a Mexican American veteran. Both groups are subject to a variety of discriminatory practices, but these may be more severe and unyielding for blacks (Broom *et al.*, 1971). Relatedly, research on residential segregation in southwestern cities has shown higher segregation between blacks and whites than between Mexican Americans and whites (cf., Meier and Feagin, 1972).

It is worth reiterating once again that our analysis does not compare all veterans with all nonveterans for the three ethnic groups. Only about one-half of all men ages 25–49 were drawn for our sample. What would be the results if we had included all men? We believe that the veteran-nonveteran differences would have widened because the bases for exclusion, especially low educational attainment and part-time employment, would have let more lower paid workers into the sample, with most falling in the nonveteran category. Consequently, our findings would have been strengthened rather than weakened.

Some readers might be tempted to infer from this analysis that the proper course of action, if blacks and Mexican American men are to overcome the handicaps of discrimination and inequality of opportunity, is to get as high a proportion of them as possible into the service. This is *not* our position. As previously noted, we have been addressing one aspect of the military experience—its effect on income. Obviously other aspects are to be taken into account. Most veterans, whatever their ethnic status, do not have fond memories of their service experience. They are prone to recall the various constraints and irritants of military life; and even

though they may have been provided with skills for surviving in bureaucratic environments, they are scarcely made happier with bureaucracies thereby, whether military or civilian.¹⁸

We wish to emphasize the importance of bridging environments in fostering social mobility. The evidence here indicates that military service is one such environment. But it should be obvious that the bridging attributes of military service discussed here could be achieved in other institutional contexts. Certainly the "independency" effect, the acquisition of education and job skills, and even exposure to bureaucratic mechanisms are all experiential and learning processes that can be effected in environments other than the military. The important point is that, despite a handicapping background, the income rank of individuals may be affected by career contingencies. Herein lies the most immediate challenge for persons interested in the relative social and economic status of minority group individuals—or more broadly, any individual from a low socioeconomic background.

REFERENCES

- Bailey, Duncan and Thomas F. Cargill
1969 "The military draft and future income." *Western Economic Journal* 7 (December): 365-70.
- Biderman, Albert D. and Laura M. Sharp
1968 "The convergence of military and civilian occupational structures." *American Journal of Sociology* 73 (January):381-99.
- Blau, Peter M. and Otis D. Duncan
1967 *The American Occupational Structure*. New York: John Wiley and Sons, Inc.
- Broom, Leonard and J. H. Smith
1963 "Bridging occupations." *The British Journal of Sociology* XIV (December):321-34.
- Broom, Leonard, Cora Martin and Betty Maynard
1971 "Status profiles of racial and ethnic populations." *Social Science Quarterly* 52 (September):379-88.
- Browning, Harley L. and S. Dale McLemore
1964 *A statistical profile of the Spanish-surname population of Texas*. Austin: The University of Texas (Bureau of Business Research).
- Chiricos, Theodore, Michael A. Pearson and James M. Fendrich
1970 "Status inconsistency, militancy and black identification among black veterans." *Social Science Quarterly* 51 (December): 572-86.
- Coates, Charles H. and Roland J. Pellegrin
1965 *Military Sociology*. University Park, Md.: The Social Science Press.
- Davis, J. Ronnie and Nell Palomba
1968 "On the shifting of the military draft as a progressive tax-in-kind." *Western Economic Journal* 6 (March):150-3.
- Duncan, Otis D., D. L. Featherman and B. Duncan
1972 *Socioeconomic Background and Achievement*. New York: Seminar Press.
- Galarza, Ernesto, Herman Gellagos and Julian Samora
1969 *Mexican-Americans in the Southwest*. Santa Barbara: McNally and Loftin.
- Grebler, Leo, Joan W. Moore and Ralph Guzman
1970 *The Mexican-American people*. New York: The Free Press.
- Hansen, W. Lee and Burton A. Weisbrod
1967 "Economics of the military draft." *Quarterly Journal of Economics* 81 (August): 395-421.
- Kitagawa, Evelyn M.
1955 "Components of a difference between two rates." *Journal of the American Statistical Association* 55 (December):1168-94.
- Lane, John H.
1968 *Voluntary Associations among Mexican Americans in San Antonio, Texas: Organizational and Leadership Characteristics*. (Unpublished Ph.D. dissertation), University of Texas at Austin.
- Lang, Kurt
1964 "Technology and career management in the military establishment." Pp. 39-81 in *The New Military*, Morris Janowitz (ed.). New York: Russell Sage Foundation.
- Leigh, Duane E. and Robert E. Berney
1971 "The distribution of hostile casualties on draft-eligible males with differing socioeconomic characteristics." *Social Science Quarterly* 51 (March):932-40.
- Meier, Michael and Joe Feagin
1972 "A comparative analysis of Black and Mexican American Residential segregation in southwestern cities." (Unpublished paper), University of Texas at Austin.
- Miller, James C. and Robert Tollison
1971 "The implicit tax on relevant military recruits." *Social Science Quarterly* 51 (March):924-31.
- Mittelbach, Frank G. and Joan W. Moore
1968 "Ethnic endogamy—the case of Mexican Americans." *American Journal of Sociology* 74 (July):50-62.
- Moskos, Charles C., Jr.
1970 *The American Enlisted Man*. New York: Russell Sage Foundation.
- N.E.A.—Tucson Survey
1966 *The Invisible Minority*. Washington, D.C.:

¹⁸ In a recent study of Mexican American voluntary association leaders in San Antonio, though military service was an obvious stimulus to the leaders' organizing ability, only 6.5% of them "place primary emphasis on the contribution made by military experience to their leadership preparation. . . ." (Lane, 1968:144).

- Department of Rural Education, National Education Association.
New Republic
1971 Unemployment Statistics. Vol. 164 (January, 30).
Oi, Walter Y.
1967 "The economics of the draft." *American Economic Review* 57 (May):39-63.
Schnore, Leo F.
1961 "Social mobility in demographic perspective." *American Sociological Review* 26 (June):407-23.
Selective Service System
1949 Reemployment and Selective Service. Special Monograph No. 13, Vol. I. U.S. Government Printing Office, Washington, D.C.
Sena Rivera, Jaime
1971 "Social structure and system in the Mexican American community." Paper delivered to the American Sociological Association, Denver, Colorado, August. (Mimeographed copy.)
Siegel, Paul M.
1965 "On the cost of being a Negro." *Sociological Inquiry* 35 (Winter):41-57.
U.S. Bureau of the Census
1971 One in a 100, A public use sample of basic records from the 1960 census (April).
Vidich, Arthur J. and Maurice R. Stein
1960 "The dissolved identity in military life." Pp. 493-506 in *Identity and Anxiety*, Maurice R. Stein, Arthur J. Vidich and David M. White (eds.), Glencoe: The Free Press.
Willett, Thomas D.
1968 "Another cost of conscription." *Western Economic Journal* 6 (December):425.

MINORITY GROUP STATUS AND FERTILITY: THE IRISH

ROBERT E. KENNEDY, JR.

University of Minnesota

American Sociological Review 1973, Vol. 38 (February):85-96

Can minority group status exert an independent effect on fertility? The Irish situation suggests that it can when: the minority is relatively large, the minority's size is politically important, the minority is economically disadvantaged, and the cohesiveness of the minority is strong. Even when such conditions exist, as they apparently do in Northern Ireland, the impact of minority group status on fertility is a less important determinant of fertility than such factors as religion, rural/urban residence, or the selective impact of migration. The question is relevant not only to nations with relatively large minority groups; it also applies to countries in which minorities, while small in proportion nationally, are concentrated locally in certain states or cities.

THE purpose of this paper is to specify conditions under which minority group status can exert an independent effect on fertility. Membership in a distinct minority group is often assumed to be an important determinant of behavior, but the question remains: just how important is it? Fertility measures make it possible to quantify the question.

The link between minority status and fertility is also important because of prac-

tical problems in designing policies to reduce fertility. Here the question is not only whether fertility is influenced by a person's minority membership, but how and under what conditions? Population programs may prove more effective if we have added insight into the basic relationships between minorities and the majority, or among large minorities. The issue applies not only to countries with relatively large minority groups; it is also pertinent to countries in which minorities, while small in proportion nationally, are locally concentrated. A prime example is the United States where blacks are not distributed equally throughout the nation but constitute relatively large proportions of some southern states and some northern cities.

A minority group is defined simply as any

* For his suggestions on both style and substance, I am indebted to William Petersen. I also wish to thank the following for their helpful criticisms: William Pratt, H. Y. Tien, Shirley Foster Hartley, and Reuben Hill. An earlier draft of this paper was presented at the Institute for Comparative Sociology's 1972 Conference on Comparative Family and Fertility Research held at Ohio State University.

distinct group comprising less than 50 percent of a total population. Such a straightforward numerical definition is necessary for discussing the question being asked: "Under what conditions is the fertility of a group influenced by its numerical share of the total population?" As Peterson (1964:216-47) has pointed out, however, the sociological concept of "minority group" is more than a simple aggregate of persons who happen to share the same characteristic. It is a coherent subculture whose members interact with one another and distinguish themselves from the rest of the population. For the purposes of this paper, all these other factors will be treated as "conditions."

My thesis is that minority group fertility will be higher than otherwise expected when two sets of conditions exist: (1) the group's members believe they can increase their political influence by increasing their share of the total population; and (2) the group's members believe their chances for individual upward social mobility are much less than that enjoyed by the rest of the population. In order to state whether such conditions exist, one needs to examine the following circumstances: what the minority group defines as the "total population," the relative size of the minority group, whether the minority group is economically subordinate, and the cohesiveness of the minority group. No linkage of numerical status and fertility would be expected without such a constellation of conditions. But in societies where such conditions do exist, the stage would be set for the operation of a pronatalist force.

Few minority couples, of course, would have children solely to increase the size of their group. The argument runs the other way: such minority couples would be less likely to plan rationally to have small families. The assumption is that large families will result, on an average, if no rational fertility control is exercised. The political importance of the group's relative size is a pronatalist force which can counter, to some degree, the various advantages to a couple in having a small family. Occupational or income discrimination against a particular minority group would reduce the importance of social mobility as an antinatalist force for that group, and also lead to some couples

being more likely to "let nature take its course."

The Relevant Comparison for Test Purposes. The crux of the problem in specifying conditions under which minority group status can exert an independent influence on fertility lies in the phrase, "than otherwise expected." What is the norm? With whom should the fertility of the minority group be compared? The tendency of assimilation and acculturation studies is to compare the minority with the majority. In a review of the literature on assimilation, minority status, and fertility in the United States, Goldscheider and Uhlenberg (1969) analyzed studies comparing the fertility of the United States "majority" with that of several American sub-groups: Negroes, Jews, Japanese Americans, and Catholics. Minority group status has an independent effect on fertility, they argued, since important differences remained after other variables were held constant. But this analysis was questioned by Sly (1970), who showed that in 1960 in the United States, at least for non-whites living outside the South, minority status did *not* have a measurable effect on fertility because race interacted with other fertility-related factors. In other words, comparing a minority with the majority can be faulty because other differences between these two sectors might affect fertility more than minority status alone.

One way to avoid the pitfalls of making minority/majority comparisons is to compare the same group under different conditions: when they are the majority (or a large minority), and when they are a small minority. In the United States, for example, this would mean comparing the behavior of blacks living in states where they constitute a large minority, with that of blacks living in states where they are a small minority. Then it can be more readily determined whether the black numerical share of a state's population has any impact on black behavior.

This approach of comparing the same group under different conditions is in the tradition of cross-cultural studies of Catholicism and fertility. It was the method used by Van Heek (1956) and Van 't Veer (1972) to study Dutch and German Catholics, Burch (1966) to study Catholics in the

United States and Canada, and Day (1968), Catholic minorities throughout the world. It should be kept in mind, however, that in this paper the primary focus is on the independent effect of numerical minority status, and not on the impact of Catholicism as such on Irish fertility (for a sociological analysis of the impact of Catholicism on fertility in the Republic of Ireland, see Kennedy, 1973).

This approach has two major limitations: statistical data on the group in different settings may not be sufficiently comparable, and what appears to be the same group living under different conditions may, in fact, be fundamentally different groups. For example, ethnic differences between Canadian and United States Catholics may affect fertility patterns more than the apparent similarities in either religion or minority status. Comparing a foreign-born minority with their home country, an alternate approach, is not appropriate because the movement itself confounds the analysis by the possibly independent effect of migration on fertility (see, for example, Macisco, et al., 1969).

This paper compares the fertility of Catholics in Northern Ireland where they are the minority, with that of Catholics in the Republic of Ireland where they are the

majority, and makes a similar comparison of the fertility of non-Catholics in the two parts of Ireland. It would be inappropriate to attempt to study the impact of numerical minority group status on Irish fertility by making minority/majority comparisons *within* each part of Ireland because of the major differences between Roman Catholicism and the various Protestant denominations concerning sex and family life. The Catholic/Catholic and Protestant/Protestant comparisons presumably hold constant the impact of religion and reveal the separate influence, if any, of numerical minority status on fertility.

We are more confident in the validity of comparisons between co-religionists across the Northern Ireland/Republic of Ireland border because of the recency of the border itself. After the war between Ireland and England, Ireland was partitioned in 1921. Through no personal choice of their own, and without their leaving their homes, the status of Roman Catholics living in what became Northern Ireland was transformed from majority to minority, and the non-Catholics in Northern Ireland simultaneously changed from minority to majority (Table 1). Under the appropriate conditions, our

Table 1. Catholics as a Percentage of the Total Population, Ireland, Northern Ireland, and the Republic of Ireland, 1861-1961

Year	Ireland (32 Counties)		Northern Ireland		Republic of Ireland	
	Total (000's)	Percentage Catholic	Total (000's)	Percentage Catholic	Total (000's)	Percentage Catholic
1861	5,798	77.7	1,396	40.9	4,402	89.4
1881	5,175	76.6	1,305	38.0	3,870	89.5
1901	4,459	74.2	1,237	34.8	3,222	89.3
1926	4,229	75.0	1,257	33.5	2,972	92.6
1946/1951 ^a	4,332 ^b	1,371	34.4	2,955	94.3
1961	4,243	74.8	1,425	34.9	2,818	94.9

^aNo census was taken in Northern Ireland in 1946; and while in the Republic censuses were taken both in 1946 and in 1951, the question of religion was asked only on the 1946 Census. Therefore, the religious composition of Northern Ireland refers to 1951, and that of the Republic to 1946. The combined population of the 32 Counties refers to 1951.

^bNot available.

SOURCES: Northern Ireland, 1961a:LI; Ireland, 1950:31, 1961b:1, 1968:20.

thesis predicts that the Northern Irish Catholics should develop a higher level of fertility than the Republic Catholics who maintained their majority status. In this paper "Ireland" and the "Irish" refer to the entire island, with "Northern Ireland" and the "Irish," and "Republic of Ireland" and the "Republic Irish," designating the new divisions.

FERTILITY PATTERNS

Fertility comparisons by religion between the two parts of Ireland are restricted by the limited amount of detailed data available for Northern Ireland. The Northern Irish patterns must be derived from the published tables of age/sex compositions of the major religious groups beginning with the 1937 Census, and from a special tabulation in the 1961 Census. Before the 1937 Census, we can only speculate about fertility differences by religion in Northern Ireland because religion was not cross-classified by age in the 1911 Irish Census or the 1926 Northern Irish Census. This means that while a detailed analysis can be made for 1961, a

major concern of this paper—fertility trends over time—can be studied only indirectly by using the available age/sex compositions.

From the age/sex compositions the child/woman ratios for each religious group can be calculated for women in their child-bearing years for all marital statuses and for married women only (Table 2). According to the total child/woman ratio, Catholics had higher fertility in Northern Ireland than in the Republic; and the gap was wider in the 1950's than the mid-1930's. The marital child/woman ratio also shows a higher Northern Irish Catholic fertility, but changes over time are not revealed since the measure first became available for both parts of Ireland only in 1946/51. The fertility difference among married women, however, was smaller and indicates that the higher Northern Irish Catholic fertility was partly due to earlier marriage. In 1961, for example, the percentage single among women age twenty-five to twenty-nine was thirty-nine in Northern Ireland, and forty-five in the Republic (Ireland, 1961b; Northern Ireland, 1961a). While the focus of this paper is on fertility, it should be noted that the earlier

Table 2. Child/Woman Ratios by Religion, Northern Ireland, 1937 to 1961; Republic of Ireland, 1926 to 1961

Religion and Year	Number of Children Aged 0-4 Years per:					
	100 Women Aged 20-44 Years		N.I. as a Percentage of R.I.	100 Ever-Married ^a Women Aged 20-44 Years		N.I. as a Percentage of R.I.
	N.I.	R.I.		N.I.	R.I.	
Catholic:						
1926	.. ^b	61	... ^b	... ^b	... ^b	... ^b
1936/1937 ^c	56	53	106	119	... ^b	... ^b
1946/1951 ^d	69	61	113	131	121	108
1961	85	76	112	137	126	109
Non-Catholic:						
1926	.. ^b	39	... ^b	... ^b	... ^b	... ^b
1936/1937 ^c	42	33	127	76	... ^b	... ^b
1946/1951 ^d	46	43	107	69	78	88
1961	55	49	112	74	75	99

^aTotal women in age group less single women.

^bNot available.

^c1936--Republic of Ireland; 1937--Northern Ireland.

^d1946--Republic of Ireland; 1951--Northern Ireland.

SOURCES: Calculated from: Northern Ireland, 1937:12-13, 1951:19-20, 1961a:23-24; Ireland, 1926:99, 1936:103, 1946b:32, 34, 1961b:36, 38.

Table 3. Estimated Legitimate Birth Rates and Crude Birth Rates by Religion, Northern Ireland, 1950-52 and 1960-62; Republic of Ireland, 1946 and 1960-62

Religion and Period	Legitimate Birth Rate ^a		N.I. as a Percentage of R.I.	Crude Birth Rate ^b		N.I. as a Percentage of R.I.
	N.I.	R.I.		N.I.	R.I.	
Catholic:						
1946/1950-52	281	275	102	25.9	23.4	111
1960-62	288	255	113	28.3	22.0	128
Non-Catholic:						
1946/1950-52	150	179	84	18.3	16.0	114
1960-62	163	151	108	19.5	13.2	148

^aEstimated legitimate live births per 1,000 married women aged 15-44 years in each religion.

^bEstimated total live births per 1,000 total population in each religion.

SOURCE: Adapted from Walsh, 1970:9, 13.

marriage pattern of the Northern Irish Catholics is consistent with the thesis being tested.

Among non-Catholics the fertility patterns were mixed. The Northern Irish child/woman ratios were higher than those in the Republic, but the reverse was true for marital child/woman ratios in 1946/1951; while in 1961 there was no meaningful difference in marital child/woman ratios. Once again the incidence of postponed marriage was less among the Northern Irish: the percentage single among women age twenty-five to twenty-nine in 1961 was twenty-six in Northern Ireland and thirty-eight in the Republic. Among non-Catholics, the Northern Irish married earlier, but did *not* have families larger than their co-religionists in the Republic.

Fertility is only roughly measured by child/woman ratios in part because the measure is affected by infant and childhood mortality. The impact of this factor in Ireland, however, was small because of the low levels of infant mortality in both parts of Ireland. In 1946 there were 5.4 infant deaths per 100 live births in Northern Ireland, and 6.5 per 100 in the Republic; while in 1961 the rates were 2.7 and 3.1 respectively (Ireland, 1950, 1964). This would have meant about one more child per 100 surviving to age one in Northern Ireland than in the Republic in 1946, and only 0.4 per 100 more in 1961. At both times, it should be noticed, the rate was higher in the Republic and resulted in

the Republic child/woman ratios being slightly smaller than they otherwise would have been.

Countering the influence of mortality differences is the fact that access to contraceptive materials was much better in Northern Ireland than in the Republic. British laws concerning contraception prevailed in Northern Ireland, while in the Republic laws banning contraceptives were passed in 1929 and 1935 (Censorship of Publications Act, 1929, Section 16; and Criminal Law Amendment Act, 1935, Section 17). The greater access to contraceptive information and materials in Northern Ireland no doubt resulted in Northern Irish child/woman ratios among both Catholics and non-Catholics being lower than they otherwise would have been.

Other indirect measures of fertility by religion in Ireland can be calculated by distributing births occurring in or near a census year according to the religious distribution of persons under one or two years of age as enumerated in the census. Using this procedure, Walsh (1970) estimated legitimate birth rates and crude birth rates by religion for both parts of Ireland in 1946/1950-52 and in 1960-62 (Table 3). Although the exact percentages differ, the general pattern presented by the marital child/woman ratios and by Walsh's estimated legitimate birth rates are similar. Both procedures indicate that among Catholics fertility

was higher in Northern Ireland than in the Republic. Walsh's figures indicate that the difference in marital fertility among Catholics was greater in 1961 than in 1946/1950-52. Among non-Catholics, Table 3 shows that the reversal in marital fertility patterns during the 1960's was even more pronounced than indicated by marital child/woman ratios alone.

A question can be raised about the selective impact of emigration on the fertility measures. The greater emigration of single persons and of married persons with small families, can result in a residual population with high levels of fertility (Kennedy, 1973: Chapter VIII). This process cannot explain the higher fertility of Catholics in Northern Ireland, however, because the estimated average annual rate of net emigration of Catholics between 1946/1951 and 1961 was higher in the Republic than in Northern Ireland among females and about the same in both areas among males (Walsh, 1970: 17-18).

Emigration does have an important effect on the *total* rate of population growth of the religious groups. In both Northern Ireland and the Republic, Catholics had higher estimated rates of net emigration than did non-Catholics between 1946/1951 and 1961 (Walsh, 1970:17-18). The greater Catholic emigration offset the natural increase expected from their higher estimated crude birth rates (Table 3) and resulted in a slower growth in the proportion Catholic in both areas (Table 1) than might otherwise have happened. The question of minority group status and emigration is beyond the scope of this paper (see Kennedy, 1973, Chapter VI for an analysis of Protestant emigration from the Republic). It is clear, nevertheless, that the demographic effect of the higher Catholic fertility in Northern Ireland was muted by the action of the Catholic emigrants.

The estimated crude birth rates reveal two significant patterns. Among both Catholics and Protestants, the Northern Irish had higher birth rates; the differences were greater in 1960-1962 than in 1946/1950-52. Second, while the crude birth rate for Republic Catholics declined, the rate for Northern Irish Catholics rose to a level unusually high for contemporary Western Europe.

Ideally, fertility differences by religion could be studied by using a measure which is not affected by possible differences in age composition, marriage patterns, or infant mortality. Such a measure is available only from the 1961 Censuses of the two parts of Ireland, and concerns only married women forty-five years or older in 1961. While this measure does not reveal current fertility or changes over time, it does confirm the conclusion drawn up to now that fertility was higher among Catholics in Northern Ireland than in the Republic. The effect of possibly different age compositions among married women is held constant by considering the completed family size of women all married at the same age and having marriages of the same duration. Among women in their late twenties at marriage, and married from twenty to twenty-four years in 1961, the completed family size of Catholics was about half a child larger in Northern Ireland than in the Republic (Table 4). For this marriage cohort there was no meaningful difference in completed family size among non-Catholics.

The comparable completed fertility of England and Wales is included in Table 4 to acknowledge the fact that Irish fertility, in both regions and for both major religions, was higher than might be expected from the English experience. England is more highly urbanized than either part of Ireland, and hence has a smaller proportion of its population in higher fertility agricultural occupations. Between the two Irish areas, Northern Ireland is more highly urbanized although just how much more cannot be determined due to differing definitions of "urban" (Northern Ireland, 1961a; Ireland, 1961a). In both Irish areas, of course, Catholic fertility was much higher than the non-Catholic. In other words, under certain conditions minority group status may affect fertility, but it is relatively less important than other fertility determinants such as religion, rural residence, or selective migration. The actual completed family size of 4.76 children among Northern Irish Catholics is about one child less than would result from *no* effort to limit births: the comparable marriage cohort at the time of the 1911 Irish Census averaged 5.77 children (Ireland, 1946 and 1951). The question remains whether the

Table 4. Average Number of Children Born Alive per Woman Aged 25-29 Years at Marriage and Married from 20-24 Years, in England and Wales, and by Religion, Northern Ireland, and the Republic of Ireland, 1961

Country	Total	Religion	
		Catholic	Non-Catholic
England and Wales ^a	1.69 ^b ^b
Northern Ireland ^c	3.06	4.76	2.45
Republic of Ireland ^d	4.12	4.25	2.47
Minority's Excess Fertility	0.51	0.02

^aWomen with uninterrupted first marriage.

^bNot available.

^cWomen married once only and enumerated with their husbands; excluding women for whom a religious denomination was not stated in the census returns.

^dAll married women (of this age at and duration of marriage), whether or not their husbands were residing with them; excluding children of previous marriages.

SOURCES: Northern Ireland, 1961b:24-40; Ireland, 1961c:6, 131; England and Wales, 1961:26.

observed fertility differences were due to the conditions specified in our thesis.

CONDITIONS

The relevant political region is the one so defined by the minority. Local leaders of the minority might encourage a higher fertility in the hope of becoming the majority of a city, state, or province even when the group has no realistic chance of tipping the balance nationally. In the case of the Irish, the political regions established over fifty years ago are used today by both major religious groups for defining which group has majority or minority status. This is not to say, even today, that all Irish persons accept the partition. Recent civil strife in Northern Ireland shows, among other things, that some Irish persons actively support the movement to rejoin the two parts of the island into one nation.

Political Importance of Minority's Size. Van Heek (1956) and Day (1968) both emphasized that the relative size of the minority can be politically important. One contribution to higher minority fertility, then, is a belief by the members of the group that their political influence will increase as their

proportion of the total population rises. Presumably this condition is most relevant for those nations with universal (or universal male) suffrage; it also applies to such nations as Northern Ireland, where during the period covered in this paper (it has since changed), the right to vote was based on the property one owned and more than one vote was allowed to certain persons (Inglis, 1965). In such a situation minority members may still believe their chances of winning new political rights will be enhanced by numerical strength. Other things being equal, the political importance of the minority's size should be greatest when there is a strong possibility of its becoming the majority. As a corollary effect, the fertility of the majority may also be higher than otherwise because of the group's efforts to maintain the existent numerical advantage.

In the creation of Northern Ireland from only six of the original nine counties of the Province of Ulster, the link between religious composition and politics was a prime consideration: if all nine counties had been included the Protestant majority would have been only 57 percent instead of the 67 percent recorded in the 1926 Censuses (Northern Ireland, 1926:57; Ireland, 1926:10).

Since the partition, the relative size of the Catholic segment of the population, and the higher Catholic fertility, have been widely recognized by both Catholics and Protestants as having important political implications. For example, the former Northern Irish Prime Minister, Capt. Terence O'Neill, made the following comments in a radio interview following his resignation from office in 1969:

The basic fear of the Protestants in Northern Ireland is that they will be outbred by the Roman Catholics. It is as simple as that. It is frightfully hard to explain to a Protestant that if you give Roman Catholics a good job and a good house they will live like Protestants, because they will see neighbours with cars and television sets. They will refuse to have eighteen children. But if the Roman Catholic is jobless and lives in a most ghastly hovel, he will rear eighteen children on national assistance (Wallace, 1971:73).

Over 85 percent of a small number of Northern Irish college students surveyed in 1970 believed that Catholics would become the majority in from twelve to twenty-seven years (Compton and Boal, 1970). According to professional estimates, Catholics may constitute more than half the total population of Northern Ireland in about forty to ninety-five years, depending on the assumptions made (Compton and Boal, 1970; Walsh, 1970).

The political importance of the relative size of a minority should decrease as its share of the total population decreases, other things being equal. In the Republic of Ireland, the non-Catholic minority is so small that they constitute no threat to the numerical dominance of the Catholics (Table 1). As Chubb (1970:53) pointed out, "In politics, religious divisions are comparatively unimportant largely because of the small size of the non-Catholic element . . . The Protestant community is politically absorbed." The first set of conditions linking minority status with fertility existed in Northern Ireland after the partition, but not in the Republic of Ireland.

Individual Upward Social Mobility Versus Minority Cohesion. Van Heek (1956), Day (1968), and Goldscheider and Uhlenberg (1969) have argued that minority cohesion is maintained to mitigate its disadvantaged status. A second condition contributing to higher minority fertility, then, is the prece-

dence of group over individual goals. When a minority is disadvantaged in competing for status, a group member may be more concerned with enhancing the minority's political influence, and less interested in aiding his own individual upward mobility through postponing marriage or rationally limiting fertility. But when minority group members are allowed to compete more generally, they may try to offset some of their disadvantages by deferring or limiting childbearing (Goldscheider and Uhlenberg, 1969:370-1). The fertility of any minority thus is influenced by the cohesion of the group and, on the other hand, by the group members' real chances for individual upward mobility. The link between minority status and higher fertility should be strong for homogeneous groups with little chance of upward mobility, and weak for heterogeneous groups with good opportunities for upward mobility.

Turning to the Irish, the question is which minority was more homogeneous and also had fewer chances for upward mobility—the Northern Irish Catholics or the non-Catholics of the Republic? Nominally the Catholics are much more homogeneous than the non-Catholics: the former are all members of the same denomination at least to the degree of having reported themselves as Catholic on the census returns, while the latter belong to several denominations. It is possible that differences in religiosity may exist between Catholics in the two regions. But in order to explain the higher Catholic fertility in Northern Ireland, one would have to assume the Republic Irish were the more secular Catholics, and that the religiosity difference was great enough to overcome the better access to birth control materials in Northern Ireland. Regarding non-Catholics it is known that the Northern Irish include a much smaller proportion of Episcopalians and a much larger proportion of Presbyterians than the Republic Irish (Ireland, 1969). Such denominational differences among the non-Catholics may have an independent impact on fertility, obscuring any effect of minority status alone.

The extent of mixed marriages across religious lines can indicate the degree of group cohesion. The small proportion and small absolute number of Republic non-Catholics increases the likelihood of marriage across

traditional boundaries. Although no data are published on the matter, Walsh (1970: 27-8) used an indirect method based on marriage rates, marriages registered by form of ceremony, and the age distribution of the married population. He estimated that among Republic non-Catholics in 1961 roughly 30 percent of the grooms and 20 percent of the brides married Catholic partners. In Northern Ireland he found no meaningful degree of inter-marriage and attributed it either to the lower level of social interaction between the two groups or to the much larger absolute and relative size of the religious minority in Northern Ireland.

The presence of separate social or cultural activities also can indicate the salience of any separation of a population into majority and minority (Van 't Veer, 1972). In Northern Ireland, as Mogeey has described (1955: 11), it was not unusual for even very small towns to duplicate much of everyday life along religious lines: duplicate music festivals, drama festivals, sports meetings, retail stores, and school systems. A similar situation existed in some parts of the Republic before partition, but by the mid-twentieth century few distinctly Protestant social organizations had survived. Chubb (1970:99) lists only two areas of social activity where the duplication still exists: separate teachers' associations and separate schools, and separate boy scout organizations. The evidence suggests that group cohesion was much stronger among the Northern Irish Catholics than among the Republic non-Catholics.

Social mobility comparisons by religion in the two parts of Ireland are restricted by the limited amount of detailed data for Northern Ireland. In all published reports of the Northern Irish censuses from 1926 through 1961, there is only one cross-tabulation of religion with a measure of socioeconomic status—the 1961 fertility report relates the husband's occupational group for women of the major religions in certain marriage cohorts. This data does not indicate the current occupational distribution of all persons, but only of certain older persons; it does not reveal social mobility through time or from father to son, but gives the distribution at only one point in time.

One advantage of the data, on the other

hand, is that it gives the occupational distribution of *exactly* the same women whose fertility is shown in Table 4. Since all the women in this selected marriage cohort had been born between 1908 and 1916, and partition took place in 1921, they passed their entire childbearing period in the post-partition epoch—an important point for a test of the thesis. Among these 9,694 women, Catholics were clearly underrepresented in upper and middle status occupations, and overrepresented in lower status and agricultural occupations (Table 5).

While not all the differences shown in Table 5 were necessarily due to the minority status of Catholics, job discrimination against them was at times openly advocated by some important Northern Irish leaders. In 1934 Sir Basil Brooke, later prime minister of Northern Ireland, said,

I recommend those people who are loyalists not to employ Roman Catholics, ninety-nine percent of whom are disloyal. . . . You people who are employers have the ball at your feet. If you don't act properly now, before we know where we are we shall find ourselves in the minority instead of the majority (Wallace, 1970:70).

Non-Catholics in the Republic, in contrast, were a favored minority. Occupational data by religion were not published in the Republic Irish 1961 Census in a format that permits a direct comparison with the data in Table 5, but the available data leave no doubt that the Republic non-Catholics enjoyed relative advantages. Though only 5.5 percent of all males fourteen years and over who were gainfully employed in 1961 were non-Catholic, they constituted 31 percent of the "directors, managers, and company secretaries," 18 percent of "commercial travelers and manufacturers' agents," 16 percent of "engineers, surveyors, and architects," and 14 percent of "physicians, surgeons, and dentists" (Ireland, 1961b:75-6). At the other extreme, only 1.3 percent of the Republic's 48,000 "laborers and unskilled workers" were non-Catholic. Earlier censuses show that Republic non-Catholics used to be even more overrepresented in the higher status occupations (Kennedy, 1973:Chapter VI).

To see whether the half-child larger completed family of the Northern Irish Catho-

Table 5. Percentage Distribution of Women Aged 25-29 Years at Marriage and Married 20-24 Years, by the Socioeconomic Group of Husband and Religion, Northern Ireland, 1961

Occupational Group of Husband	Total	Catholic Women	Non-Catholic Women	Catholic as a Percentage of Non-Catholic
Upper status	100.0% (N = 1,657)	13.6	86.4	15.7
Middle status	100.0% (N = 3,825)	20.2	79.8	25.3
Lower status	100.0% (N = 2,281)	36.2	63.8	56.7
Agricultural	100.0% (N = 1,370)	39.2	60.8	64.5
Other	100.0% (N = 561)	36.5	63.5	57.5
Total	100.0% (N = 9,694)	26.5	73.5	36.1

Upper status = Employers and managers (large and small establishments); and professional workers (self-employed and employees).

Middle status = Intermediate and junior non-manual workers; foremen; supervisors of manual workers; and skilled manual workers.

Lower status = Semi-skilled and unskilled manual workers.

Agricultural occupations = Farmers (employers, managers, and own account); and other agricultural workers.

SOURCE: Calculated from Northern Ireland, 1961b:24-40.

lics shown in Table 4 was possibly due to their concentration in lower status and agricultural pursuits, the Catholic/Catholic comparison must be made *within* each occupational group. While not all occupational categories were identical in the two 1961 Irish censuses, they were sufficiently similar to make comparisons. Within categories the fertility of Catholics was higher in Northern Ireland than in the Republic for nine of eleven major occupational groups (Table 6). The two exceptions presumably include many persons highly concerned with individual upward mobility: employed professional workers and intermediate nonmanual workers (white-collar workers). There was no consistent pattern of fertility differences within occupational groups for non-Catholics. In six categories the Northern Irish, and in the remaining five the Republic Irish, had higher fertility.

The persistence of higher Northern Irish Catholic fertility within occupational groups

in this marriage cohort supports the minority group/higher fertility thesis. Economic insecurity among lower status workers may lead to higher fertility in both parts of Ireland; but this fact does not explain why some lower, middle, and upper status Catholic workers in Northern Ireland had completed families from one-fifth to three-quarters of a child larger than their opposite numbers in the Republic. The Irish rural family system presumably is essentially the same on both sides of the 1921 border; yet Northern Irish Catholic farmers in this marriage cohort had almost one more child, on an average, than the Catholic farmers in the Republic.

CONCLUSION

Conditions were appropriate in Northern Ireland for minority status to contribute to higher fertility. Political importance was placed on the relative size of the Catholic

Table 6. Average Number of Children Born Alive per Woman Aged 25-29 Years at Marriage and Married 20-24 Years, by Religion and Socioeconomic Group of Husband, Northern Ireland and the Republic of Ireland, 1961

Social Group of Husband	Catholics			Non-Catholics		
	N.I.	R.I.	N.I.-R.I.	N.I.	R.I.	R.I.-N.I.
Upper status:						
Employers & managers	4.39	3.65	+0.74	2.19	2.02	-0.17
Self-employed professional	3.86	3.64	+0.22	2.12	2.18	+0.06
Professional (employees)	3.82	4.11	-0.29	2.07	2.37	+0.30
Middle status:						
Intermediate non-manual	3.69	3.73	-0.04	2.25	2.00	-0.25
Junior non-manual	4.11	3.95	+0.16	1.97	2.88	+0.91
Skilled	4.62	4.05	+0.57	2.38	2.44	+0.06
Lower status:						
Semi-skilled	4.45	4.15	+0.30	2.49	2.12	-0.37
Unskilled	5.18	4.50	+0.68	2.80	3.50	+0.70
Agricultural:						
Farmers	5.66	4.86	+0.80	3.36	3.29	-0.07
Agricultural workers	4.84	4.28	+0.56	3.37	2.88	-0.49
Other occupations & unknown	4.28	3.63	+0.65	2.42	1.94	-0.48
All occupations	4.76	4.25	+0.51	2.45	2.47	+0.02

NOTE: In the Republic some categories were given different names, and while comparable, they should not be considered identical with those in Northern Ireland. These categories are: higher professional (rather than self-employed professional); lower professional (rather than professional--employees); and farmers, farmers' relatives, and farm managers (rather than farmers--employers, managers, and own account). One category in the Republic, "Salaried Employees," had no direct counterpart in the Northern Irish system; in this category Catholics had 3.60 children and non-Catholics had 2.19.

SOURCES: Ireland, 1961c:203; and calculated from Northern Ireland, 1961b:24-40.

population, and the Catholic minority was expected eventually to become the majority. The limited available data indicate that Catholics were disadvantaged, at least among certain older marriage cohorts, and that the degree of cohesiveness among Catholics as a group was strong. The fertility measures consistently revealed higher fertility among Catholics in Northern Ireland than in the Republic in spite of better access to birth control materials in the North. The fertility differences were small, however, indicating that minority group status was a less important determinant of fertility than, for example, religion.

Conditions did not exist which would have led to higher fertility among the minority non-Catholics of the Republic. Their number was too small, their composition too heterogeneous, their chances for individual upward mobility too good. Furthermore, the fertility of Northern Irish non-Catholics may have

been higher than otherwise because some members of this group encouraged larger families in order to maintain their numerical dominance. In contrast to the consistent pattern for the Catholics, the various fertility measures produced mixed results for the non-Catholics.

The specific thesis examined in this paper was whether numerical minority status, under certain conditions, can have an independent effect on fertility. The Irish situation suggests that it can, but that even under the appropriate conditions, minority group membership is apparently a secondary determinant of fertility. Nevertheless, from this added confidence that there can be a link between minority status and fertility, it can be argued that fertility analyses of large minorities in other nations, or in sub-national areas such as states or cities, are not complete without a consideration of the minority's numerical share of the popula-

tion of reference, the minority's degree of group cohesion, and its relative chances for upward social mobility.

REFERENCES

- Beckett, J. C.
1966 *The Making of Modern Ireland 1603-1923*. London: Faber and Faber.
- Burch, Thomas K.
1966 "The fertility of North American Catholics: a comparative overview." *Demography* 3:174-87.
- Compton, P. A. and F. W. Boal
1970 "Aspects of intercommunity population balance in Northern Ireland." *Economic and Social Review* 1 (July):455-76.
- Chubb, Basil
1970 *The Government and Politics of Ireland*. Stanford, California: Stanford University Press.
- Day, Lincoln H.
1968 "Natality and ethnocentrism: some relationships suggested by an analysis of Catholic-Protestant differentials." *Population Studies* 22 (March):27-50.
- England and Wales
1961 Census, "Fertility Tables."
- Goldscheider, Calvin and Peter R. Uhlenberg
1969 "Minority group status and fertility." *American Journal of Sociology* 74 (January):361-72.
- Inglis, Brian
1965 *The Story of Ireland*, 2d ed. London: Faber and Faber.
- Ireland
1926 Census of Population, Volume III, Part I.
1936 Census of Population, Volume III, Part I.
1946a Census of Population, Volume II.
1946b Census of Population, Volume III, Part I.
1946 and 1951 Censuses of Population, "General Report."
1950 Statistical Abstract.
1961a Census of Population, Volume I.
1961b Census of Population, Volume VII, Part I.
1961c Census of Population, Volume VIII.
1964 Statistical Abstract.
1968 Statistical Abstract.
1969 Statistical Abstract.
- Kennedy, Robert E. Jr.
1973 *The Irish: Emigration, Marriage and Fertility*. Berkeley, California: University of California Press.
- Macisco, John J., Jr., Leon F. Bouvier and Martha Jane Renzi
1969 "Migration status, education and fertility in Puerto Rico, 1960." *Milbank Memorial Fund Quarterly* 47 (April):167-87.
- Mogey, J. M.
1955 "Ulster's six counties." Pp. 1-13 in Thomas Wilson (ed.), *Ulster Under Home Rule*. London: Oxford University Press.
- Northern Ireland
1926 Census of Population, "General Report."
1937 Census of Population, "General Summary."
1951 Census of Population, "General Report."
1961a Census of Population, "General Report."
1961b Census of Population, "Fertility Report."
- Park, A. T.
1962-3 "An analysis of human fertility in Northern Ireland." *Journal of the Statistical and Social Inquiry Society of Ireland* XXI: 1-13.
- Petersen, William
1964 *The Politics of Population*. Garden City, New York: Doubleday.
- Robinson, Alan
1961 "The geography of human fertility in Northern Ireland." *Irish Geography* V: 302-10.
- Sly, David F.
1970 "Minority group status and fertility: an extension of Goldscheider and Uhlenberg." *American Journal of Sociology* 76 (November):443-59.
- Van Heek, F.
1956 "Roman-Catholicism and fertility in the Netherlands: demographic aspects of minority status." *Population Studies* 10 (November):125-38.
- Van 't Veer, Anthon J.
1972 "Roman Catholic fertility in Tudderen: an analysis of one factor," (William Petersen, Translator). Pp. 342-6 in William Petersen (ed.), *Readings in Population*. New York: Macmillan.
- Wallace, Martin
1971 *Northern Ireland: 50 Years of Self Government*. Newton Abbot, Devon: David & Charles.
- Walsh, Brendan M.
1970 "Religion and demographic behavior in Ireland." Dublin: Economic and Social Research Institute, Paper No. 55 (May).

OCCUPATIONAL EXPERIENCE AND PSYCHOLOGICAL FUNCTIONING: AN ASSESSMENT OF RECIPROCAL EFFECTS¹

MELVIN L. KOHN AND CARMİ SCHOOLER

National Institute of Mental Health

American Sociological Review 1973, Vol. 38 (February):97-118

The central issue of this paper is whether men's adult occupational experiences affect or only reflect their psychological functioning. Our analysis isolates a small set of occupational conditions, twelve in all, which defines the structural imperatives of the job. These occupational conditions are found to be substantially related to men's psychological functioning, off as well as on the job. We argue that the relationships between occupational conditions and psychological functioning result from a continuing interplay between job and man, in which the effects of job on man are far from trivial. This argument is borne out by an assessment of the reciprocal effects of the substantive complexity of the work (a critically important occupational condition, for which we have the requisite longitudinal data) and several facets of psychological functioning. Substantive complexity has a decidedly greater impact on psychological functioning than the reverse.

OUR thesis is that adult occupational experience has a real and substantial impact upon men's psychological functioning. This argument, although familiar to social science at least since Marx's early writings, has never to our knowledge been empirically appraised. A widely-believed contrary argument is that all correspondence between men's occupations and personalities results from processes of selective recruitment and modification of the job to meet incumbents' needs and values. This view seems to underlie, for example, the logic of personnel testing, where the object is to select job applicants whose personalities match those of successful job incumbents. This perspective may also underlie the greater attention sociologists have given to occupational choice than to occupational effects.

The question of whether occupation affects or only reflects personality has come up twice before in our research. The issue first arose in our analysis of social class,

occupational self-direction, and orientation (Kohn and Schooler, 1969; Kohn, 1969: 139-203). We found that occupational conditions conducive to the exercise of self-direction in one's work—namely, freedom from close supervision, substantively complex work, and a non-routinized flow of work—are empirically tied to valuing self-direction and to having an orientation to oneself and to the outside world consonant with this value.

It could be argued that these findings reflect the propensity of men who value self-direction to seek out jobs that offer them opportunity to be self-directed in their work and, once in a job, to maximize whatever opportunities the job allows for exercising self-direction. But we know that occupational choice is limited by educational qualifications, which in turn are greatly affected by the accidents of family background, economic circumstances, and available social resources. Moreover, the opportunity to exercise greater or lesser self-direction in one's work is circumscribed by job requirements. Thus, an executive must do complex work with data or with people; he cannot be closely supervised; and his tasks are too diverse to be routinized—to be an executive requires some fairly large degree of self-direction. Correspondingly, to be a semi-skilled factory worker precludes much self-direction. The substance of one's work

¹ We are indebted, for advice and essential help to our associates—Elizabeth Howell, Margaret Renfors, Carrie Schoenbach, and Lindsley Williams; for carrying out the survey on which the research is based, to Paul Sheatsley, Eve Weinberg, and the staff of the National Opinion Research Center; and, for their creatively critical reading of an earlier draft of this paper, Hubert M. Blalock, Otis Dudley Duncan, and Stephen M. Olsen.

cannot be especially complex; one cannot escape some measure of supervision; and if one's job is to fit into the flow of other people's work, it must necessarily be routinized. The relationship between being self-directed in one's work and holding self-directed values would thus seem to result not just from self-directed men acting according to their values, but also from men's job experiences affecting these very values.

The issue of the direction of the effects arose again in an analysis of bureaucracy (Kohn, 1971), which showed that, contrary to popular preconception, employees of bureaucratic organizations are more intellectually flexible, more open to new experience, and more self-directed in their values than are men who work in nonbureaucratic organizations. It is, of course, possible that bureaucracies hold a special attraction for such men. But an explanation based on self-selection assumes that men have more complete and accurate knowledge of working conditions in bureaucratic organizations, before starting to work there, than is usually the case. This assumption seems especially unlikely in light of widely held stereotypes about the inflexibility of bureaucracy. An explanation based on self-selection also assumes that men have a wider range of choice in the type of firm or organization for which they will work than is often true. It would thus appear that an important part of the explanation for employees of bureaucratic organizations differing psychologically from those of nonbureaucratic organizations is that they experience different conditions of occupational life—principally, greater job protection, higher income, and substantively more complex work.

In this paper, we go beyond these essentially *a priori* arguments to an empirical appraisal of the reciprocal effects of man on job and job on man. Since we have interviewed our respondents only once, we lack the measurements of change required for a longitudinal analysis of the continuing interplay between job conditions and psychological functioning. Still, our unusually complete occupational data enable us to delineate those facets of occupation that are most closely related to psychological functioning at the present time. This makes it possible for us to examine systematically the major

alternatives to our preferred interpretation—that there is a continuing interplay of man affecting job and job affecting man—and to estimate the magnitudes of the reciprocal effects.

METHODS

The key to this analysis is our focus on *dimensions* of occupation. By contrast, the main tradition in the sociology of work has been to focus on a particular occupation, explicitly or more often implicitly comparing it to all other occupations or to those occupations believed to highlight its unique characteristics. Cottrell's (1940) classic study of railroaders, for example, pointed out a multitude of ways that the job conditions of men who operate trains differ from those of men in many other occupations—including the unpredictability of working hours, geographical mobility, precision of timing, outsider status in the home community, and unusual recruitment and promotion practices. Since all these conditions are tied together in one occupational package, it is not possible to disentangle the psychological concomitants of each. More recent comparative studies face similar interpretative problems. For example, Blauner's (1964) study of blue-collar workers in four industries, chosen to represent four technological levels, showed that differences in working conditions are systematically associated with the stage of technological development of the industry. But these differences, too, come in packages; printing differs from automobile manufacture, for example, not only in technology and in the skill levels of workers, but also in pace of the work, closeness of supervision, freedom of physical movement, and a multitude of other conditions.²

We attempt to disentangle the intercorrelated dimensions of occupation by securing a large sample of men who work in many occupations, inventorying their job

² Other particularly pertinent studies of occupational experience and psychological functioning are: Chinoy, 1955; Freidson, 1970; Goldthorpe, et al., 1968, 1969; Kornhauser, 1965; Pearlin, 1962; and Turner and Lawrence, 1965. A remarkably prescient analysis of the research literature to its time is Herbert Menzel's (1950) M.A. thesis.

conditions, and differentiating the psychological concomitants of each facet of occupation by statistical analysis. Accordingly, the study is based on a sample of 3,101 men, representative of all men employed in civilian occupations in the United States. These men were interviewed for us by the National Opinion Research Center in the spring and summer of 1964. (For a general description of the sampling methods, cf. Sudman and Feldman, 1965; for more specific information on sample and research design, cf. Kohn, 1969:235-64.)

There are serious limitations to securing occupational data by interviewing a representative sample of men. One is that men's descriptions and evaluations of their job conditions may be biased—a problem with which we shall deal later. Another is that men may have only limited information about some aspects of their jobs, such as the overall structure of the organization in which they work. Moreover, a sample of men scattered across many occupations and many work places does not contain enough people in any occupation or any work place to trace out interpersonal networks and belief systems. Similarly, the method is not well adapted for studying the industrial and technological context in which the job is embedded.³

The method is useful, though, for studying the immediate conditions of a man's own job—what he does, who determines how he does it, in what physical and social circumstances he works, subject to what risks and rewards. In the interviews, we attempted to secure pertinent information about all these aspects of occupational experience, emphasizing those that we had any reason to believe might influence psychological functioning.⁴ In all, we indexed

more than fifty separable dimensions of occupation, including such diverse aspects of work experience as the substantive complexity of the work, the routinization or diversity of the flow of work, relationships with co-workers and with supervisors, pace of work and control thereof, physical and environmental conditions, job pressures and uncertainties, union membership and participation, bureaucratization, job protections, and fringe benefits (for complete information, cf. Kohn, 1969:236, 244-53). These indices provide the basis for a broad descriptive picture of the principal facets of occupation, as experienced by men in all types of industry and at all levels of the civilian economy. We shall shortly see which of these many aspects of occupational experience matter most for men's psychological functioning.

INDICES OF PSYCHOLOGICAL FUNCTIONING

From the myriad aspects of psychological functioning that might be affected by occupational experience, we limit this inquiry to ten, chosen because of their intrinsic importance and because together they cover a wide sweep. These ten deal with subjective reactions to the job itself, valuation of self-direction or of conformity to external authority, orientation to self and to society, and intellectual functioning.

Subjective Reactions to Occupation

Although our principal interest is in the possible effects of the job on *off-the-job* psychological functioning, we include two indices of men's subjective reactions to their jobs. We see these phenomena as a sort of way-station between the concrete realities of the job and men's orientations to non-occupational realities. If occupational experience has psychological pertinence, we should certainly expect it to affect men's views of the job itself.

One index is of *occupational commitment*, meant to measure men's dedication to their occupations, as distinct from their satis-

³ For pertinent studies of interpersonal, industrial, and technological context, cf. Blau, 1960; Blauner, 1964; Chinoy, 1955; Walker, 1957; Walker and Guest, 1952; and Whyte, 1961.

⁴ Our search was extensive: we used such diverse sources as past research, our own and our colleagues' occupational experiences, our reading of novels, plays, and even the *Dictionary of Occupational Titles* (U.S. Dept. of Labor, 1949), and—most valuable of all—semi-structured pretest interviews with a considerable number of men in a wide range of occupations.

In addition to several of the studies previously noted, the following were particularly helpful in

our search for relevant dimensions of occupation: Becker and Carper, 1956; Blau, 1955; Edwards, 1959; Foote, 1953; Hughes, 1958; Lipset, et al., 1956; Mills, 1953; Morris and Murphy, 1959; and Rosenberg, 1957.

faction or dissatisfaction with the particular jobs they hold. The index is based on a Guttman Scale of four questions about men's willingness to change occupations, their appraisal of what it takes to be good at their occupation, and their sense of the moral worth of their occupation (cf. Kohn, 1969:180).

Another index, *job satisfaction*, attempts to measure men's satisfaction or dissatisfaction with those aspects of their jobs they deem important. We asked each respondent to rate the importance of many aspects of work—from how interesting it is to how clean it is (cf. Kohn, 1969:77, 250). We then asked him to tell us how satisfied he is, in his present job, with every aspect of work he considers important. Our index is an average of these judgments, weighting a man's satisfaction with an aspect of work he deems very important twice as much as his satisfaction with an aspect of work he considers only fairly important.⁵

Valuation of Self-direction or of Conformity to External Authority

By values, we mean standards of desirability—criteria of preference (cf. Williams, 1960:402–3). Our study includes indices of men's values for themselves and for their children, in both cases the focus being their valuation of self-direction or of conformity to external authority. In this analysis, we use only the index of values for children, for it is better constructed.⁶ It is based on a factor analysis of men's rankings of the relative desirability for a child of a particular sex and age of a number of generally-valued characteristics. As thus indexed, val-

uing self-direction means regarding as most desirable such characteristics as an interest in how and why things happen, consideration, good sense, and responsibility; valuing conformity means giving priority to such qualities as neatness, manners, being a good student, and obedience.

Self-conception and Social Orientation

We attempt to measure five facets of self-conception and social orientation, all of them based on a factor analysis of a set of fifty-seven questions, mainly of the "agree-disagree" and "how often?" types (cf. Kohn, 1969:265–9 for the derivation of these indices).⁷ These are:

Anxiety, by which we mean the intensity of consciously felt psychic discomfort. It is indexed by such questions as: How often do you feel that you are about to go to pieces? How often do you feel downcast and dejected? How frequently do you find yourself anxious and worrying about something? How often do you feel uneasy about something without knowing why?

Self-esteem, an index created by combining scores for two factors, self-confidence and self-deprecation. Self-confidence is indexed by agreement or disagreement with such assertions as: I take a positive attitude toward myself. I feel that I'm a person of worth, at least on an equal plane with others. I am able to do most things as well as other people can. Self-deprecation is indexed by agreement or disagreement with: I wish I could have more respect for myself. At times I think I am no good at all. I feel useless at times.

Stance toward change, that is, men's receptiveness or resistance to innovation and change. It is indexed by responses to such questions as: Are you generally one of the first people to try out something new or do you wait until you see how it's worked out for other people? and by agreement or disagreement with such assertions as: It generally works out best to keep on doing things the way they have been done before.

Criteria of morality, by which we mean

⁵ The correlation between this index of job satisfaction and answers to the single question, "All things considered, how satisfied are you with the job as a whole?" is 0.67. For a review of studies of job satisfaction, and a discussion of the inadequacies of a single-question index, cf. Blauner, 1966. For a host of alternative indices, cf. Robinson, et al., 1969:99–143.

⁶ For the derivation of these indices and an assessment of their adequacy, cf. Kohn, 1969:56–9, 73–5. In all analyses using the index of valuation of self-direction/conformity for children, the number of cases is reduced to 1,499, because these questions were asked only of men having one or more children between the ages of three and sixteen living at home.

⁷ Much more complete information about these indices, including their relationship to other established measures of these and similar concepts, is given in the cited reference.

a continuum of moral positions, from believing that morality consists of strict adherence to the letter of the law and keeping out of trouble, to defining and maintaining one's own moral standards. This dimension is indexed by answers to such questions as: Do you believe that it's all right to do whatever the law allows, or are there some things that are wrong even if they are legal? and by agreement or disagreement with such assertions as: It's all right to do anything you want as long as you stay out of trouble. It's all right to get around the law as long as you don't actually break it.

Authoritarian conservatism, that is, men's definition of what is socially acceptable—at one extreme, rigid conformance to the dictates of authority and intolerance of non-conformity; at the other extreme, open-mindedness. It is indexed by agreement or disagreement with such assertions as: The most important thing to teach children is absolute obedience to their parents. Young people should not be allowed to read books that are likely to confuse them. People who question the old and accepted ways of doing things usually just end up causing trouble.

Intellectual Functioning

We study two aspects of intellectual functioning. The first, *intellectual flexibility*, is evidenced by performance in handling cognitive problems that require weighing both sides of an economic or a social issue, in differentiating figure from ground in complex color designs, and in drawing a recognizably human figure whose parts fit together in a meaningful whole.⁸ We also in-

clude the interviewers' evaluations of the respondents' "intelligence" and a simple count of their propensity to agree with agree-disagree questions. All these we take to reflect, in some substantial part, intellectual flexibility. A factor analysis of these various manifestations of intellectual flexibility yields two dimensions, one primarily perceptual, the other ideational. The latter provides the index used in these analyses.⁹

We measure another aspect of intellectual functioning by examining *the demands men put on their intellectual resources*, no matter how great or limited these resources may be. This index, based on a factor analysis of questions about a wide range of leisure-time activities, focuses on how intellectually demanding are those activities. The relevant factor contrasts spending a large amount of one's leisure time watching TV and reading popular magazines, with engaging in such intellectually active pursuits as going to museums and plays, reading books, and working on hobbies.¹⁰ Some of the latter activities are facilitated by education and income; we take this fact into account in our analyses.

OCCUPATIONAL CONDITIONS AND PSYCHOLOGICAL FUNCTIONING

Virtually all of the many occupational conditions included in our inquiry are significantly related to one or another of the indices of psychological functioning enumerated above. But many of these relationships may be artifacts: they may merely reflect the interrelatedness of occupational conditions with one another and with education. To distinguish those occupational conditions whose relationship with psychological functioning is both independent and pronounced, we employ the following criteria:

⁸ Specifically, we asked: "Suppose you wanted to open a hamburger stand and there were two locations available. What questions would you consider in deciding which of the two locations offers a better business opportunity?" and "What are all the arguments you can think of for and against allowing cigarette commercials on TV? First, can you think of arguments *for* allowing cigarette commercials on TV? And can you think of arguments *against* allowing cigarette commercials on TV?" The perceptual test consists of a portion of Witkin's (1962) Embedded Figures Test, selected by Witkin. The Figure-drawing Test (cf. Witkin, 1962:117-29) consists simply of asking the respondent to draw a figure of a man on a standard-size card with a standard pencil. Respondents are reassured that artistic ability is not required. The meaningful coherence of the figure is what we appraise.

⁹ It is based primarily on the interviewer's appraisal of the respondent's intelligence (0.69), the cigarette-commercials problem (0.61), the respondent's "agree" score (-0.56), the hamburger-stand problem (0.54), and the Embedded Figures Test (0.52).

¹⁰ The factor loadings are: frequency of visits to plays, concerts, and museums (-0.60); number of books read in the past six months (-0.54); time spent working on hobbies (-0.35); amount of magazine reading (0.61); and time spent watching television (0.35).

(1) We statistically control education—because education in most cases precedes and is often a prerequisite for a job.¹¹ (This may be an overly stringent criterion, because it does not allow for the possibility of a causal chain in which education is a determinant of job conditions, which in turn affect psychological functioning.)

(2) In assessing each occupational condition, we statistically control all the others.¹² This we do to measure the independent effects of each occupational condition, uninfluenced by its interrelatedness with other aspects of the job.

(3) Finally, we require an occupational condition to be significantly related to more than one facet of psychological functioning—so that we may limit the analysis to occupational conditions whose impact on psychological functioning has some degree of generality.

Twelve of the more than fifty occupational conditions we have indexed meet all these criteria (Table 1). The magnitudes of the relationships between these twelve occupational conditions and the several facets of psychological functioning are generally not very large;¹³ but since our controls are so stringent, the phenomena they depict are exceedingly precise. When we find, for example, that the substantive complexity of the job is related to self-esteem, we can be reasonably sure that substantive com-

plexity is at issue, not education or such intercorrelated occupational conditions as closeness of supervision, routinization, and time-pressure. Moreover, the overall (canonical)¹⁴ correlation between occupational conditions and psychological functioning is sizeable: with education controlled, it is 0.41. Even if subjective reactions to the job are excluded, the correlation is 0.34. Whatever interpretation one draws, the relationship between job conditions and psychological functioning is large enough to be taken seriously.

Although few in number, the twelve occupational conditions that meet our criteria are sufficient to define the *structural imperatives of the job*, in that they identify a man's organizational locus, his opportunities for occupational self-direction, the principal job pressures to which he is subject, and the principal uncertainties built into his job. Of all these occupational conditions, those that determine occupational self-direction prove to be the most important: they are significantly related to all facets of psychological functioning, in most cases more strongly so than any other occupational conditions. But organizational locus, job pressures, and job uncertainties all make some independent contribution to the overall relationship between the structural imperatives of the job and psychological functioning.

Our more specific findings, and the indices on which they are based, are as follows:

Organizational Locus

Ownership/nonownership. All men who have any substantial share in the ownership of the firm, in which they are employed are here treated as owners. The advantageous position of owners is reflected in their greater occupational commitment, greater job satisfaction, and lesser anxiety. Owners are also more self-directed in their values and authoritarian conservative in orientation than are employees.

¹¹ Using multivariate analysis of variance (Clyde, et al., 1966), we also tested the interactions between education and each of the occupational conditions. We found them to be generally small and statistically nonsignificant. While doing this, we tested the linearity of each occupational condition in its relationships with each facet of psychological functioning. They proved to be preponderantly linear.

¹² For this analysis, we used the multiple regression program developed by Nie, et al. (1968). Using multivariate analysis of variance, we also tested interactions between pairs of occupational conditions, wherever we had reason to expect them to occur. The interactions were generally small and unpatterned. Even those few that were statistically significant do not affect the essential conclusions drawn from the multiple regression analyses.

¹³ In Table 1, the magnitude of relationship between any occupational condition and any given facet of psychological functioning is given by the standardized beta-coefficient, which indicates "how much change in the dependent variable is produced by a standardized change in one of the independent variables when the others are controlled" (Blalock, 1960:345).

¹⁴ A canonical correlation is a multiple correlation of one or a set of independent variables with a set of dependent variables. More precisely, the canonical correlation employed here is the maximum correlation between linear functions of the two sets of variables (cf. Cooley and Lohnes, 1962:35).

Table 1. Subjective Reactions to Job, Values, Orientation, and Intellectual Functioning, by the Principal Dimensions of Occupation (Controlling Education)

	Occupational commitment (i = committed)	Job satisfaction (+ = satisfied)	Parental valuation of self-direction (+ = high)	Anxiety (+ = anxious)	Self- esteem (+ = high)	Stance toward change (+ = receptive)	Criteria of morality (+ = moral)	Authoritarian conservatism (+ = authoritarian)	Intellectual flexibility (+ = flexible)	Intellectual demandingness/ leisure time (+ = demanding)
Standardized beta- coefficients										
1. Organizational locus										
a. Ownership	.19	.10	.05	-.12	---	---	---	.05	---	---
b. Bureaucratization	-.07	---	.04	-.06	.05	.06	.09	---	.10	.05
c. (High) position in hierarchy	.05	.10	---	---	---	---	-.04	---	---	---
2. Occupational self- direction										
a. Closeness of supervision	-.10	-.18	-.07	.07	-.09	-.06	-.13	---	-.07	---
b. Modernization of work	-.13	-.06	---	---	---	---	-.06	.07	-.11	-.07
c. Substantive com- plexity	.20	.16	.08	---	.09	.09	.07	-.07	.10	.18
3. Job pressures										
a. Time pressure	---	-.10	.10	.11	---	.11	---	-.06	.06	---
b. Heaviness of the work	---	---	---	---	.07	---	---	-.04	---	.04
c. Dirtiness of the work	---	-.10	---	.07	-.05	---	---	.07	-.05	-.09
4. Uncertainties										
a. Likelihood of "dramatic change"	.05	---	---	.09	---	.07	---	---	---	---
b. Frequency of being held responsible for things outside of one's control	-.07	-.12	---	.09	---	---	---	---	---	---
c. Risk of loss of job or business	-.05	-.14	---	.06	-.06	---	---	---	---	-.04
Multiple-partial correlations^a										
1. Organizational locus	.18	.13	.07	.13	.05	.07	.10	.07	.12	.07
2. Occupational self- direction	.23	.24	.10	.07	.12	.10	.16	.09	.18	.14
3. Job pressures	---	.14	.10	.12	.07	.11	.04	.09	.08	.09
4. Uncertainties	.09	.18	---	.15	.06	.08	---	---	.04	.07
5. All occupational conditions	.36	.40	.19	.24	.18	.22	.20	.15	.26	.24

N = 3101, except for parental valuation of self-direction, where N = 1499.

^a Controlling education and all other facets of occupation.^b Beta-coefficients and correlations of less than ± 0.04 are omitted from this table; they would not be statistically significant.

Note: The "positive" end of each occupational dimension is implicit in its name; in the case of job pressures and job uncertainties, the "positive" end is assigned to greater pressure or greater uncertainty.

Bureaucratization is here indexed by the number of formal levels of supervision. (For the derivation of the index, cf. Kohn, 1971.) Employees of bureaucratic firms or organizations are less committed to their occupations (and more to the organizations that employ them) than are employees of non-bureaucratic firms. Bureaucratization also has broad ramifications for off-the-job functioning, employees of bureaucratic firms and organizations being more intellectually flexible and making more intellectually demanding use of their leisure time, having more personally responsible moral standards, being more receptive to change, being less anxious, having greater self-esteem, and being more self-directed in their values, than are employees of nonbureaucratic firms. All these findings are consonant with our earlier analyses (Kohn, 1971), which showed the conditions of work in bureaucratic organizations to be conducive to both intellectual and attitudinal flexibility.

Position in the supervisory hierarchy is indexed by the number of people over whom a man has direct or indirect supervisory authority. Higher position is associated with job satisfaction and occupational commitment, but position in the supervisory hierarchy has little pertinence for off-the-job functioning.

Occupational Self-direction

By occupational self-direction we mean the conditions that facilitate or restrict the use of initiative, thought, and independent judgment in work. We conceive three occupational conditions to be critical (cf. Kohn, 1969:139-40; or Kohn and Schooler, 1969: 671).

Closeness of supervision is indexed by a Guttman Scale based on five questions about how much latitude the supervisor allows and how supervisory control is exercised (cf. Kohn, 1969:153).

Routinization of the work (which we earlier called the complexity of organization of the work) is measured by the repetitiveness of the work tasks and the complexity of the "units" of which work is comprised (cf. Kohn, 1969:159-60).

The substantive complexity of the work is based on detailed questioning of each re-

spondent about his work with things, with data or ideas, and with people (cf. Kohn, 1969:153-5, 271-6). The index is a composite of seven ratings: our appraisals of the complexity of a man's work with data, with things, and with people; our appraisal of the overall complexity of his work; and his estimates of the amount of time he spends working in each type of activity. A factor analysis of these seven ratings yields a single index of substantive complexity.¹⁵

All three aspects of occupational self-direction are broadly pertinent for psychological functioning, men whose job conditions facilitate self-direction consistently having more positive reactions to job, self, and society and consistently evidencing more effective intellectual functioning. Of the three components of occupational self-direction, the substantive complexity of the work is clearly the most important; but closeness of supervision and routinization of work add significantly to the total impact.

Job Pressures

Our indices of job pressures are based mainly on the respondents' own appraisals of their situations.¹⁶ Three types of job pressure prove to be pertinent:

Frequency of time-pressure. Seeing oneself as working under great pressure of time is associated with job dissatisfaction and anxiety. But working under time-pressure is also associated with valuing self-direction, being receptive to change, being open-minded, and being intellectually flexible. Time-pressure may be unpleasant, but it may nevertheless be conducive to flexibility.

Heaviness of work. Doing heavy work may be a source of pride—it is primarily associated with high self-esteem.

Dirtyness of work represents how dirty a man gets while doing his job. Doing dirty work has unpleasant concomitants—the dirtier a man's work, the greater his job dis-

¹⁵ The factor loadings are: complexity of work with data (0.85), with people (0.82), with things (-0.26); overall complexity of the work (0.80); time spent working with data (0.65), with people (0.57), with things (-0.68).

¹⁶ The questions used to elicit men's appraisals of job pressures are questions #11 (heaviness), 29 (time pressure), and 31 (dirtyness) in Kohn, 1969: 245-7.

satisfaction, the greater his level of anxiety, and the lower his self-esteem. Doing dirty work is also associated with authoritarian conservatism, lack of intellectual flexibility, and not making intellectually demanding use of one's leisure time.

Uncertainties

Finally, there are three aspects of occupational reality (as appraised by the respondents)¹⁷ that are less immediate, more indefinite in scope and time than are job pressures; all deal with the possibility of a potentially threatening change in a man's basic job situation:

The likelihood, in this field, of there occurring a sudden and dramatic change in a man's income, reputation or position. The probability of such a change, whether it is likely to be for the better or the worse, is associated with anxiety. If there is some possibility that the change can be for the better, it is also associated with being generally receptive to change and with occupational commitment.

The frequency of being held responsible for things outside one's control. Perceiving this to be a serious possibility is associated with job dissatisfaction, anxiety, and lesser commitment to one's occupation.

The risk of loss of one's job or business. This threat has the most entirely expectable psychological concomitants of all, primarily job dissatisfaction, anxiety, and lessened self-esteem.

Numerous other facets of work that are only ancillary to the structural imperatives of the job seem to have little independent relevance for psychological functioning. Take, for example, interpersonal relatedness. Those aspects of interpersonal relatedness that are directly involved in occupational self-direction—that is, closeness of supervision and the substantive complexity of men's work with people—prove to have widespread psychological ramifications. But aspects of interpersonal relatedness not involved in occupational self-direction, such

as whether the man works primarily alone or in the company of others and, if with others, with how many others, whether he is involved in a team operation, the competitiveness of his relations with fellow employees, and his participation in union or other work-related group activities, prove to have little independent relevance. It may be, of course, that our methods are inadequate for assessing the full impact of interpersonal relatedness. But we believe that the explanation lies elsewhere, that on-the-job interpersonal relationships do not greatly affect off-the-job psychological functioning; it is the structural imperatives of the job that affect men's ways of dealing with the larger world.

We have found, then, a network of statistical relationships between the structural imperatives of men's jobs and their psychological functioning, off as well as on the job. All these relationships are meaningful, in the sense that all could be explained straightforwardly as resulting from job affecting man via the direct process of men's generalizing from occupational experience to other realms. Not only is stressful occupational experience associated with job dissatisfaction and anxiety, but job conditions—however affectively toned—that elicit effort and flexibility are associated with favorable evaluations of self, an open and flexible orientation to others, and effective intellectual functioning.

ALTERNATIVE INTERPRETATIONS

Our preferred interpretation of the links between the structural imperatives of the job and men's psychological functioning is that there is a continuing interplay between job and man, in which job conditions both affect and are affected by men's psychological functioning. Our objective is to assess the magnitudes of these reciprocal effects. But before doing this, we must consider a number of alternative interpretations, each of which might explain our findings without according any importance to the effects of job on man. We start with the simplest—that our findings might reflect some inadequacy in our occupational indices—reserving until last the most complex and most important—that our findings

¹⁷ The questions used to elicit men's appraisals of job uncertainties are questions #38 (held responsible), 46 and 49 (risk of loss of job or business), and 69 (sudden and dramatic change) in Kohn, 1969:248-52.

might result from men seeking out or modifying jobs to fit their personalities.

The Adequacy of Our Occupational Indices

The simplest alternative explanation is that our findings reflect a systematic tendency for respondents to see their conditions of work through the distorting lens of their own needs and values. Our data suggest otherwise.

Consider first the index of substantive complexity. We can compare this index, which is precisely tailored to the specifics of each respondent's description of his own job, with the assessments given in the *Dictionary of Occupational Titles* (U.S. Department of Labor, 1965) for every occupation in the American economy. The *Dictionary's* ratings of the complexity of work with things, with data, and with people are averages for entire occupations, so they lack the specificity of ours; but since they are based on observations by trained occupational analysts, they can serve as a source of external validation. We find the multiple correlation between our index of substantive complexity and the independently coded *Dictionary*-ratings to be 0.78—sufficiently high to assure us that our appraisals of substantive complexity accurately reflect the reality of men's work.

For no other index do we have such a clearcut standard of comparison. But it is at least possible to ask whether, on the average, men's assessments of their occupational conditions seem realistic. For this analysis we focus on those occupations for which our sample contains at least thirty men, arbitrarily defining an "occupation" as any occupational grouping to which the *Dictionary of Occupational Titles* assigns a unique identifying number. The technique of analysis is to rank these occupations in order of the median rating their incumbents give to any given occupational condition, to see if this rank-ordering is consonant with our general knowledge of occupational realities. Insofar as it is, we are assured that respondents' appraisals of this occupational condition are essentially unbiased. In fact (Table 2), these evaluations conform closely to what we should expect them to be. Thus, it is improbable that systematic biases in

respondents' reports about their jobs contribute much to explaining the relationships between occupational conditions and psychological functioning.

Income and Occupational Status

Another possible interpretation of our findings is that actual working conditions are not important in themselves, but only as a reflection of such extrinsic aspects of the job as the income and status it provides. This possibility can be tested by statistically controlling income and occupational status to see whether the relationships between conditions of work and psychological functioning are markedly reduced.

For several facets of psychological functioning (specifically, occupational commitment, job satisfaction, parental valuation of self-direction, and the intellectually demanding use of leisure time), controlling income and occupational status does reduce the multiple correlations noticeably, though not by nearly enough to render them statistically nonsignificant (Table 3). The correlations between job conditions and all other facets of psychological functioning are hardly at all reduced. It thus seems clear that the relationships between occupational conditions and psychological functioning do not simply reflect income and status.¹⁸

Moreover, now that we have simultaneously controlled three principal components of social class—education, occupational status, and income—we can conclude that our findings are not simply another manifestation of the pervasive influence of class.

Social Selectivity in Recruitment and Retention

Still another possible explanation of our findings is that they reflect social selectivity in occupational recruitment and retention. This could occur if occupations recruit their members from different segments of the society—drawing more heavily from urban or from rural populations, from particular races, religions, and nationalities, from older

¹⁸ In fact, income and status, controlled on education and occupational conditions, are much less strongly related to psychological functioning than are occupational conditions, controlled on education, income, and occupational status.

Table 2. Rank-orders of Median Ratings of Occupational Characteristics Given by the Members of the 14 Most Populous Occupations in the Sample

Occupations	N=	Occupational Characteristics									
		Closeness of supervision	Routinization	Substantive complexity	Time pressure	Heavy-ness	Dirty-ness	Likelihood of dramatic change	Freq. of being "held responsible"	Risk of loss of job or business	
1. Accountants and auditors	(32)	11	8	2	3	14	14	10	8	12	
2. Carpenters (construction industry)	(43)	2	9	12	12	3½	5	8	7	5	
3. Farmers (general)	(126)	14	14	9	10	3½	3	1	6	1	
4. Farmers (grain)	(31)	12	12	7	9	5½	1½	2	9	2	
5. Machinists	(34)	5	11	10	5	10	6	8	14	7	
6. Managers and officials, service industries	(41)	13	7	4	2	11	13	4	2	3	
7. Managers and officials, wholesale and retail trade	(62)	10	6	3	1	12	11	3	1	6	
8. Mechanics	(50)	7	13	8	7½	5½	1½	8	11½	10	
9. Packers and materials handlers	(43)	1	1	13	13	1	8	13	11½	9	
10. Porters and cleaners	(39)	4	2	14	14	8½	9	14	13	11	
11. Salesmen--drivers	(38)	8	3	5	7½	8½	10	6	3	13	
12. School teachers (secondary school)	(36)	9	5	1	11	13	12	12	5	14	
13. Service station attendants	(31)	6	10	6	6	7	4	5	4	4	
14. Truck-drivers (heavy trucking)	(35)	3	4	11	4	2	7	11	10	8	

Table 3. Multiple-partial Correlations between Occupational Conditions and Psychological Functioning

Facet of Psychological Functioning	Multiple-partial Correlation between the Set of 12 Occupational Conditions and the Specified Facet of Psychological Functioning; Controlling		
	Education	Education plus Occupational Status and Income	Education plus Background Characteristics ^a
Occupational commitment	.36	.25	.33
Job satisfaction	.40	.32	.38
Parental valuation of self-direction	.19	.13	.17
Anxiety	.24	.22	.21
Self-esteem	.18	.16	.19
Stance toward change	.22	.20	.23
Criteria of morality	.20	.17	.16
Authoritarian conservatism	.15	.14	.12
Intellectual flexibility	.26	.22	.23
Intellectual demandingness of leisure-time activities	.24	.17	.22

Note: N = 3101, except for parental valuation of self-direction, where N = 1499. All correlations shown are statistically significant.

^a Age, race, urbanicity of principal place where the man was raised, religious background, and national background.

or from younger segments of the work force. In fact, we find that there are consistent (albeit not very powerful) links between men's social characteristics and their conditions of work. For example, men who do substantively complex work are disproportionately white, older, urban, members of "liberal" religious denominations, and of northern or western European background.

So there is a distinct possibility that the relationships between occupational conditions and psychological functioning reflect the social characteristics of the workers. For example, substantive complexity may be related to self-esteem because men who do substantively complex work come from the more advantaged segments of society. To test this and similar possibilities, we statistically control age, race, urbanicity, religious background, and national background, just as we earlier did occupational status and income, to see whether the relationships between occupational conditions

and psychological functioning are appreciably reduced. They are not (Table 3). We conclude that social selectivity in occupational recruitment contributes little to our findings.

Occupational Self-selection and Job-molding

It could also be argued that occupational conditions are related to psychological functioning because of individual selectivity in recruitment and retention—because employers hire men they think are qualified and because men search out jobs that meet their needs and desires. Moreover, men often leave jobs, voluntarily or otherwise, when the jobs do not match their needs, desires, or talents. (To make matters more complicated, men may be promoted out of jobs to which they are especially well suited.) Finally, once in a job, men may mold their conditions of work to meet their preferences.

We readily grant that these processes of

"occupational self-selection" and "job-molding" do occur and may well contribute to our findings. Intellectually flexible men, for example, may seek and be sought for jobs that allow maximum opportunity to do substantively complex work; and once in their jobs, they may make the most of whatever opportunities the jobs provide to exercise occupational self-direction. Similarly, men who hold authoritarian beliefs may gravitate to jobs in which they are closely supervised. It is even possible that men who disparage themselves may welcome dirty jobs that confirm their negative self-evaluation. But not all our findings can be explained by such processes. Few men, for example, choose to be held responsible for things outside of their control. And although anxious men may create conditions of time-pressure, it is at least as reasonable to expect them to avoid getting into time-pressured situations.

Moreover, as we have previously argued and can now demonstrate empirically, occupational self-selection and job-molding take place within rather narrow confines: occupational conditions are structurally interrelated. Thus, a man who does substantively complex work stands a greater risk of being held responsible for things outside his control than does a man who works at simpler tasks: the correlation of substantive complexity with such a risk is 0.32. The risk increases if the job is not only substantively complex but also time-pressured (the multiple correlation being 0.36), and increases further if the man stands high on the supervisory ladder or is an owner (multiple $r = 0.40$). From this perspective, an increased risk of being held responsible for things outside one's control is the price one pays for holding an interesting and responsible job. Similarly, jobs that offer freedom from close supervision are likely to be substantively demanding, are somewhat unpredictable, and are also likely to entail some risk of losing one's job or business (multiple $r = 0.54$). We need not proliferate examples of the structural interrelatedness of occupational conditions. All point to the same lesson: it is not as if one could make a series of independent decisions—to be self-directed, not to be under great time-pressure, to work in a nonbureaucratic firm;

their structural interrelatedness means that one has to accept some occupational conditions as the price for securing others.

The data provide a second reason for believing that occupational self-selection and job-molding cannot sufficiently explain the relationships between occupation and psychological functioning. If these processes fully accounted for the relationships, then statistically controlling an index of men's job preferences should substantially reduce the magnitude of the correlations. We can test this possibility, for we had asked the men to evaluate the importance of many occupational conditions, ranging from pay and job security to the chance to use one's abilities. A factor analysis yields two dimensions to these judgments, one emphasizing the intrinsic, the other emphasizing the extrinsic (cf. Kohn, 1969:75-8). Using factor scores based on these dimensions, we statistically control both. The relationships between job conditions and psychological functioning are only slightly reduced.

A third reason for believing that occupational self-selection and job-molding do not provide an adequate explanation of our findings comes from an examination of time-pressure, an occupational condition that varies notably in the degree to which it is subject to workers' control. Having asked the men to tell us who or what controls the pace of their work, we can compare those who say they control the pace of their own work with those who say they work at a pace enforced by a boss, co-workers, the speed of machinery, or some other external agency. We find that, on the whole, the correlations between time-pressure and psychological functioning are as great for men who work at an externally enforced pace as for men who determine their own pace of work (Table 4). This being the case, it can hardly be that these findings simply reflect men's ability to select or to mold their occupational conditions to suit their needs and values.

The evidence consistently suggests that, although men undoubtedly do choose and mold their jobs to fit their personal requirements, it is not likely that these processes alone can sufficiently explain the relationships between occupational conditions and psychological functioning. The correlations

Table 4. Partial Correlations between Time-pressure and Psychological Functioning, Separately for Men Who Do and Men Who Do Not Control the Pace of Their Work (education controlled)

Partial Correlation of Time-pressure with	Men Who Control the Pace of Their Own Work	Men Whose Work-pace is Controlled by Boss, Co-workers, Speed of Machinery, or Other External Agent
Occupational commitment	.04 (n.s.)	.02 (n.s.)
Job satisfaction	-.06	-.12
Parental valuation of self-direction	.13	.11
Anxiety	.14	.12
Self-esteem	.02 (n.s.)	.04 (n.s.)
Stance toward change	.17	.11
Criteria of morality	.03 (n.s.)	.05 (n.s.)
Authoritarian conservatism	-.07	-.08
Intellectual flexibility	.07	.17
Intellectual demandingness of leisure-time activities	.08	.12
N ^a =	(1730)	(818)

^aExcept for parental valuation of self-direction, where N = 320 and 410.

between occupation and psychological functioning must also reflect the impact of the job on the man.

THE RECIPROCAL EFFECTS OF MAN ON JOB AND JOB ON MAN

We have tried to show that the relationships between occupational conditions and psychological functioning do not simply reflect some inadequacy in our indices; or the effects of education, occupational status, and income; or selectivity in recruitment and retention; or men's efforts to mold their jobs to fit their needs and values. This negative argument has been necessary to establish the plausibility of an interpretative model that assumes reciprocal effects of job on man and man on job. Now we can assess the relative magnitudes of these reciprocal effects.¹⁹

¹⁹ One might think it possible to assess the impact of job on man by measuring increases in the magnitudes of the correlations between occupational conditions and psychological functioning as men have been in their jobs for longer times. But the expectation that the correlations should be a linear

We want to partition the correlations between occupation and psychological functioning into their component parts to assess how large a part of each correlation results from the effects of the job on the man and how large a part from self-selection, job-molding, and such other processes as can together be thought of as the effects of the man on the job. Statistical procedures that assume causal effects to be unidirectional are of course inappropriate to this task. But, fortunately, the simultaneous-equations techniques that econometricians have developed for dealing with reciprocal effects are well suited to our needs (cf. Goldberger, 1964; Blalock, 1971; Miller, 1971; Dun-

(or even some more complicated) function of time in job assumes that the most important difference between men who have been in their jobs a short time and those who have been in their jobs a longer time is time itself. This assumption disregards the highly selective processes of occupational mobility, both vertical mobility within a single occupation and mobility from one occupation to another. It also fails to take adequate account of the social psychological effects of prior jobs and leaves out of consideration other time-related processes, notably aging.

can, Haller, and Portes, 1968). Specifically, we employ the technique called two-stage least squares, a relatively simple method for estimating coefficients in simultaneous equations (cf. Blalock, 1971; Mason and Halter, 1968; Goldberger, 1964:329-38).²⁰

The two-stage procedure, in effect, attempts to "purge" each variable of the effects of all others with which it is reciprocally related, by estimating from other pertinent data what each individual's score on that variable would have been if the other variables had not had an opportunity to affect it. These estimated scores are then used for assessing each variable's impact on the others. Clearly, the analysis is hypothetical, albeit no more so than are analyses based on such conventional statistical techniques as partial correlation and test-factor standardization. And, clearly, the meaningfulness of the analysis depends on the accuracy of the estimated scores.

In doing two-stage least squares analyses, one first computes the multiple regression of each of the reciprocally-involved variables (which are called "endogenous" variables in the nomenclature of econometrics) on a set of "predetermined" variables, i.e. variables that are believed to have some causal effect on one or more of the endogenous variables, but which cannot themselves have been affected by any of the endogenous variables. "Predetermined" variables can be of two types—"exogenous" variables, whose explanation lies outside of the explanatory model, e.g. social background characteristics; and "lagged endogenous" variables, i.e. values of one or more of the endogenous variables from some earlier time or times. The unstandardized beta-coefficients from these equations (together with the respondents' scores on the predetermined variables) provide the basis for creating "purged" estimates of the endogenous variables. These estimated scores are used in the stage-two regression equations, which assess the causal impact of each of the endogenous and of relevant predetermined variables. (For a more detailed

description of the procedure and of the criteria that must be met in using it, cf. Mason and Halter, 1968; Blalock, 1971.)

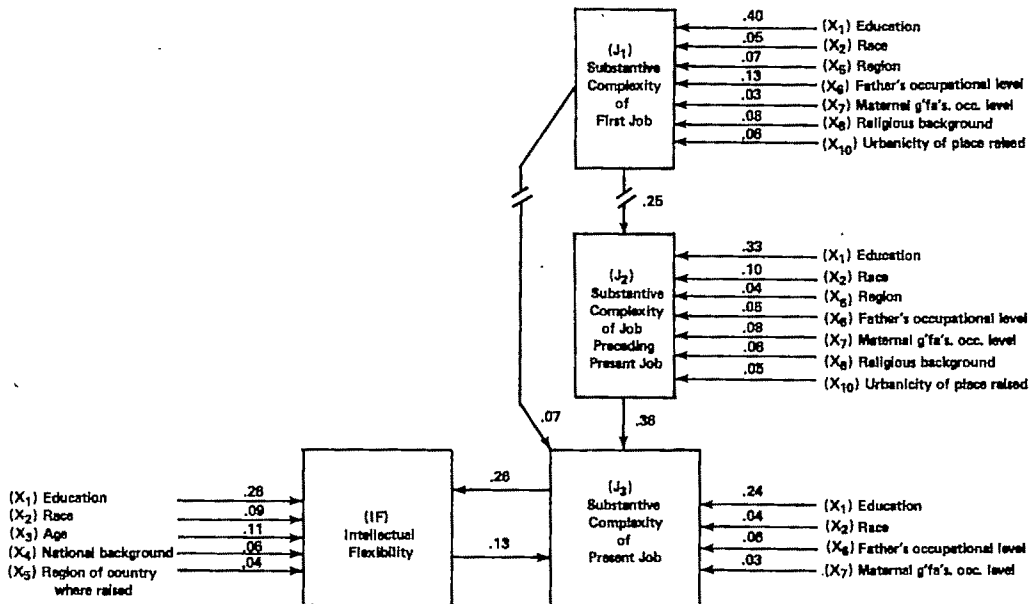
In our analysis, the endogenous variables are job conditions and the several facets of psychological functioning. The exogenous variables are all those background characteristics that we had *a priori* reason to think would have an impact on, and in fact proved to be empirically related to, one or another endogenous variable. These alone, without data on past job conditions, do not provide enough information to construct adequate estimators of some of the endogenous variables, notably of current job conditions; nor would an analysis that did not take past job conditions into account be particularly instructive. So we also need data about pertinent "lagged endogenous" variables, i.e. past job conditions.

Fortunately, our data include a history of each man's past jobs. This information is insufficient for inventorying all pertinent facets of each past job, but it does enable us to index the most important facet of the job, its substantive complexity. To create this index, we make use of the finding, reported earlier, that our measure of the substantive complexity of the particular job is highly correlated with the *Dictionary of Occupational Titles'* appraisals of the complexity of work with data, with things, and with people for the occupation as a whole. Because of this correspondence between our and the *Dictionary's* ratings, we can compute an approximate index of substantive complexity for any past job from a knowledge of the job title alone.²¹ Such an index has obvious limitations. It assumes that the levels of complexity of the many occupations in the American economy were

²⁰ We are indebted to Hubert M. Blalock for suggesting that we use this technique, and to Ralph Bryant, Roger Craine, and Richard Porter of the Federal Reserve Board for helping us develop the actual model used in our analyses.

²¹ The approximate index of substantive complexity is equal to the sum of the *Dictionary of Occupational Titles* score for complexity of work with data, weighted .133, plus the score for complexity of work with people, weighted .119, minus the score for work with things, scored 1 if the man does any significant work with things and 2 if he does not, weighted .711. (These weights represent the unstandardized beta-coefficients from the multiple regression of substantive complexity on the *Dictionary* scores for data, people, and things, with the score for things dichotomized. The weight for "things" is larger than the other two because this score has been dichotomized.) The smaller the total score, the more complex is the man's work.

Figure 1. THE RECIPROCAL EFFECTS OF THE SUBSTANTIVE COMPLEXITY OF THE JOB AND INTELLECTUAL FLEXIBILITY



(Figures represent standardized beta-coefficients from the second-stage of two-stage least squares regression equations. Signs are deleted; the direction of all relationships is the same as those in the corresponding zero-order correlations.)

similar some years or decades ago to what they are today. Being based on average levels of substantive complexity for entire occupations rather than precise levels of complexity for particular jobs, the *Dictionary*-based index is also less powerful than the index of substantive complexity we have thus far employed. But since it extrapolates from the observations and evaluations made by trained occupational analysts, it has the advantage that it cannot be influenced by our respondents' styles of reporting or of job performance.

As a prototypic analysis, we examine the reciprocal effects of substantive complexity and intellectual flexibility, the latter deliberately chosen because it appears to offer us the toughest test: Intellectual flexibility is obviously pertinent to recruitment into substantively complex jobs, and it might be expected to be the most resistant to change of all facets of psychological functioning we have studied. As depicted in Figure 1, the model assumes that intellectual flexibility and job complexity have reciprocal effects; the purpose of the analysis is to assess the relative magnitudes of these effects. Lacking a measure of intellectual flexibility from

earlier stages of the men's lives, we cannot estimate the impact of intellectual flexibility on the substantive complexity of *past* jobs. It would be unfair to measure the impact of past jobs on present intellectual flexibility when we cannot measure the reciprocal process. We therefore exclude from the model any consideration of the effects of past jobs on present intellectual functioning.²² Thus, the model attempts only to assess the reciprocal effects of the substantive complexity of the *present* job and intellectual flexibility at the *present* time. In so doing, though, we can and must

²² The reader may think that including these effects in the second-stage multiple-regression equation would reduce the effect of the substantive complexity of the *present* job on intellectual flexibility; but precisely the opposite occurs.

In the development of the model presented in Figure 1, we experimented with alternate models in which prior jobs and even education were treated as endogenous variables. These models are statistically reasonable and yield conclusions broader than but entirely consonant with those of the present analysis. But they have the logical defect of treating present intellectual flexibility and past occupational (and educational) conditions as if contemporaneous.

take into account the effects of past jobs on the present job.

Putting this discussion more formally, we see that the stage-two multiple regression equations are:

1. $IF = f_1(\hat{J}_3, X_1, X_2, X_3, X_4, X_5)$
2. $J_3 = f_2(\hat{IF}, J_1, J_2, X_1, X_2, X_6, X_7)$
3. $J_2 = f_3(J_1, X_1, X_2, X_5, X_6, X_7, X_8, X_{10})$
4. $J_1 = f_4(X_1, X_2, X_5, X_6, X_7, X_8, X_{10})$

Where IF and J_3 are endogenous variables, J_1 and J_2 are lagged endogenous variables, the X 's are exogenous variables, and:

- IF = Intellectual flexibility, as actually measured
 \hat{IF} = Intellectual flexibility, as estimated from the predetermined variables in the first-stage regression equation²³
 J_3 = Substantive complexity of present job, as actually measured
 \hat{J}_3 = Substantive complexity of present job, as estimated from the predetermined variables in the first-stage regression equation
 J_2 = Substantive complexity of the job immediately prior to the present job
 J_1 = Substantive complexity of the first job that lasted six months or longer
 X_1 = Education
 X_2 = Race
 X_3 = Age at time of interview
 X_4 = National background (linear approximation)²⁴

²³ The first-stage equations for estimating intellectual flexibility (\hat{IF}) and substantive complexity (\hat{J}_3) are based on all the predetermined variables, even though four of the exogenous variables proved not to be significant in the stage-two equations for IF and J_3 . We include these four variables in the stage-one equations because of their theoretical pertinence and because our earlier empirical analyses demonstrate their relevance to intellectual functioning. Essentially the same results obtain whether we use this procedure or either of two logical alternatives—dropping the nonsignificant exogenous variables from the stage-one equations, or including them in the stage-two equations.

²⁴ The indices of national background, region, and religious background are linear approximations to these nonlinear concepts. In our present use, these linearized indices represent slight underestimates of what would be shown in a more complicated dummy variable analysis. The rationale for these linearizations is given in Schooler, 1972. Essentially, all three indices are ordered in terms of modernity: national

X_5 = Region of the country in which the man was raised (linear approximation)

X_6 = Father's occupational level

X_7 = Maternal grandfather's occupational level

X_8 = Religious background (linear approximation)

X_9 = Father's education

X_{10} = Urbanicity of principal place where the man was raised

X_{11} = Number of children in parental family

The model is presented pictorially in Figure 1, which shows the reciprocal effects of substantive complexity and intellectual flexibility, as well as the effects of those "predetermined" variables that have some significant impact.²⁵ The numbers shown are standardized beta-coefficients from the second-stage equations. These figures bear out our contention that substantive complexity and intellectual flexibility do have substantial reciprocal effects.

The most important determinant of the *substantive complexity* of the present job, as would be expected, is the substantive complexity of the immediately preceding job. Education has considerable contemporaneous importance, above and beyond its historical importance as the prime determinant of the substantive complexity of prior jobs. Next in importance is intellectual

background, on the basis of how long it has been since the social organization of the nation's agriculture passed beyond feudalism; region of the United States, on the basis of industrialization and expenditures for education; and religion, on the basis of fundamentalism.

²⁵ As previously noted, the effects of past jobs on current intellectual flexibility are deliberately excluded from the model. So too is the effect of age on the substantive complexity of the present job, for including age in the equation controls the very historical processes whose effects we are trying to measure, thereby overemphasizing the conditions making for mobility among an age cohort.

To be certain that excluding these variables does not result in a misspecification of the stage-two equations, we followed the advice of Robert M. Hauser and used the procedures described in Duncan, Haller, and Portes (1968) to test that the correlations between these variables and the residuals of the pertinent endogenous variables are small. All are negligible. We also computed the correlation between the residuals of the two endogenous variables, which (at 0.18) seems to be within acceptable limits.

flexibility, which has a greater impact on job complexity than any exogenous variable other than education. Still significant, even well into men's careers; are race, father's occupational level, and even maternal grandfather's occupational level. Other background characteristics—notably religious background and the region and urbanicity of the principal place of residence during childhood—have had some historical importance in shaping earlier career stages, but by the third or later job no longer have any significant direct bearing.

The most important determinants of *intellectual flexibility* are education and the substantive complexity of the work. Their impact greatly surpasses that of age, race, national background, and the region of the country in which the man was raised, all of which have some pertinence for intellectual flexibility well into men's careers. No other exogenous variable in our model continues to have a significant impact at this stage of career.

The crucial comparison this model enables us to make is of the relative magnitudes of effect of substantive complexity on intellectual flexibility and of intellectual flexibility on substantive complexity. The ratio is exactly two to one. In terms of their *present* reciprocal relationship, the impact of job on intellectual flexibility is greater than the reverse.

It must be emphasized that this conclusion refers only to the present time, not to the historical process. It is but a simple extrapolation from our findings to conclude that the reciprocal relationship between intellectual flexibility and the substantive complexity of the present job must have been true for past jobs, too. (And there is every reason to believe that in the lives of these men intellectual flexibility once bore the same type of reciprocal relationship to education that it now bears to the substantive complexity of the job.) Without measurements of the men's intellectual flexibility at the relevant times in the past, we cannot assess the magnitudes of the reciprocal effects of intellectual flexibility and substantive complexity over the course of men's occupational careers.²⁶ The *present*

relationship between the two, though, results more from job complexity affecting intellectual flexibility than the reverse.

We can now extend the model to consider the relationships between substantive complexity and all the other facets of psychological functioning we have measured (Table 5).²⁷ As before, the crucial comparison is of the effect of the substantive complexity of the job on a particular facet of psychological functioning, as compared to the effect of that facet of psychological functioning on substantive complexity. *In all cases, job affects man more than man affects job.*²⁸ In fact, only four of the eight

we have not been able to use either successfully. In the first, one estimates past intellectual flexibility in a manner analogous to the procedures we used for estimating "purged" endogenous variables. We tried this, but we had insufficient data, so our estimates were too highly correlated with present scores to be useful in statistical analysis. In the second approach, which has been successfully employed by Duncan (1968), one uses other sources of data to fill in gaps in the correlational matrix that forms the basis for multiple regression analysis. But we could not find data adequate to our needs. There comes a point where only longitudinal data will do.

One might think our comparison unbalanced, in that we control past jobs in evaluating the effect of intellectual flexibility on current job complexity, but do not control past intellectual flexibility in evaluating the reciprocal process. To test this possibility, we take the extreme step of treating education as a "proxy" for past intellectual flexibility, ignoring education's importance in its own right and as a prerequisite for the job. Thus treated, education would still be included in the stage-two equation for intellectual flexibility (now as a lagged endogenous variable), but would be excluded from the stage-two equation for job complexity. This would increase the apparent effect of intellectual flexibility on job complexity from 0.13 to a maximum of 0.40, making it possibly larger than the reciprocal effect of 0.26 but not so large as to dwarf it.

²⁷ As before, the stage-one equations for estimating the endogenous variables are based on all the predetermined variables, whether or not they are significant in the stage-two simultaneous equations. The stage-two equations assessing the impact of substantive complexity on the various facets of psychological functioning include only those exogenous variables that are significant. But the results are virtually identical when all the exogenous variables are included in the stage-two equations.

²⁸ To be certain that this conclusion is not an artifact of the hypothetical nature of two-stage least squares analysis, we performed a comparable analysis, using ordinary least squares regression equations. One cannot rely on ordinary least squares, since the equations are underidentified and thus

²⁶ We know of two possible approaches to making such an assessment without longitudinal data, but

Table 5. The Reciprocal Effects of the Substantive Complexity of the Job and Psychological Functioning

(Figures represent standardized beta-coefficients from the second stage of two-stage least squares regression equations)			
Facet of Psychological Functioning:	Effect of Specified Facets of Psychological Functioning on Substantive Complexity ^a	Effect of Substantive Complexity on the Specified Facet of Psychological Functioning	Exogenous Variables Included in Equation (all others non-significant)
Occupational commitment	.02 (n.s.)	.16	Race (.04), Age (.17), Region of origin (.09), Urbanicity (.07).
Job satisfaction	.13	.39	Education (-.22) ^b , Race (.04), Urbanicity (-.05).
Parental valuation of self-direction	-.07 (n.s.) ^b	.14	Education (.21), Race (.04), Age (.05), National background (.07), Region of origin (.06), Father's education (.04).
Anxiety	.04	.07	Age (.12), Urbanicity (.04).
Self-esteem	.04	.10	Age (.05), National background (.06), Urbanicity (.08).
Stance toward change	-.03 (n.s.)	.14	Age (.16), Region of origin (.09).
Criteria of morality	.01 (n.s.)	.16	Education (.09), Race (.06), Age (.14), National background (.10), Religious background (.14), Urbanicity (.05).
Authoritarian conservatism	.01 (n.s.)	.17	Education (.21), Age (.19), National background (.05), Region of origin (.07), Religious background (.06), Urbanicity (.04).
Intellectual flexibility	.13	.26	Education (.26), Race (.09), Age (.11), National background (.06), Region of origin (.04).
Intellectual demandingness of leisure-time activities	.14	.28	Education (.17), Race (-.07), Region of origin (.08), Father's occupational level (.04), Religious background (.07), Father's education (.04), Urbanicity (.05).

^a Substantive complexity of prior jobs, education, father's occupational level, and maternal grandfather's occupational level are always included in the equations. Standardized beta-coefficients for these variables remain essentially the same no matter which facet of psychological functioning is included in the equation.

^b A minus sign signifies a reversal of direction from the zero-order correlation.

facets of off-the-job functioning have a statistically significant effect on job complexity, and of these four only the two measures of intellectual functioning—intellectual flexibility and the intellectual demands men put upon their leisure-time activities—are more than just barely significant. By contrast, substantive complexity significantly affects all facets of psychological functioning, in several cases to as large a degree as does the most powerful exogenous variable. In short, the substantive complexity of the job is consistently important for psychological functioning, and consistently more so than is psychological functioning for substantive complexity.

All this is based on but one dimension of occupation, its substantive complexity. We cannot say whether it is true for the job as a whole, as it is for substantive complexity, that the impact of job on man is consistently greater than the reverse. But we can conclude that in the continuing interplay between man and job, the effects of job on man are far from trivial.

DISCUSSION

The central issue of this paper is whether men's adult occupational experiences affect or only reflect their psychological functioning. We believe that the job does play a part in shaping the man, that there is a continuing interplay throughout his career between man affecting job and job affecting man. In support of this belief, we have presented several strands of evidence, none definitive but all consistent in indicating that the relationships between the conditions men experience in their work and their psychological functioning do not simply reflect some inadequacy in our data; or the effects of education, status, and income; or men's efforts to mold their jobs to fit their needs and values; or selectivity in recruitment and retention. In fact, for the one occupational condition about which we have sufficient historical data to make a precise assessment—the substantive complexity of the work—we find that the job has a substantially greater impact on

men's psychological functioning than the reverse.

Several limitations of our study need be recognized. First, in the absence of longitudinal data, which would be both difficult and expensive to obtain, we can assess only the *current* reciprocal effects of occupational conditions and psychological functioning, not the historical process. Second, our treatment of the data has been broad-brush: we have described the main effects of some principal occupational conditions on the total male civilian work force, ignoring for this analysis the possibility that there may be important variations on the themes we have sketched, and even some exceptions, among various segments of this population. Third, our study says nothing about the effects of occupational experience for women. Finally, our methods may be less suited for studying organizational, interpersonal, and technological contexts than for studying the immediate conditions of the man's own work.

These limitations notwithstanding, we believe that our findings have several important implications. Most concretely, they buttress and extend our earlier conclusions about the social psychological importance of conditions that facilitate or inhibit the exercise of occupational self-direction. We had earlier found (Kohn and Schooler, 1969; Kohn, 1969: Chapters 9 and 10) that occupational self-direction is of critical importance for understanding the impact of social class on men's values and orientation. We can now say that, in addition to its relevance for explaining the social psychological impact of class, occupational self-direction has the most potent and most widespread effects of all the occupational conditions we have examined. In terms of psychological effects, the central fact of occupational life today is not ownership of the means of production; nor is it status, income, or interpersonal relationships. Instead, it is the opportunity to use initiative, thought, and independent judgment in one's work—to direct one's own occupational activities. To put the matter a little more generally, our findings emphasize the social psychological importance of the structural imperatives of the job—those aspects of the job that impinge on the man most directly, insistently,

lead to biased estimates (cf. Blalock, 1971:156). It is nevertheless reassuring that ordinary least squares analysis yields precisely the same conclusion as does two-stage least squares.

and demandingly. Not only the conditions that determine occupational self-direction, but all structural imperatives of the job that elicit effort and flexibility, are conducive to favorable evaluations of self, an open and flexible orientation to others, and effective intellectual functioning. Men thrive in meeting occupational challenges.

These findings also provide some insight into the processes by which occupational experience affects psychological functioning. The findings argue for a generalization model, in contrast to a reaction-formation or compensatory model (cf. Breer and Locke, 1965). That is, the specific links between particular occupational conditions and particular facets of psychological functioning suggest that men's ways of coping with the realities of their jobs are generalized to non-occupational realities. Men whose jobs require intellectual flexibility, for example, come not only to exercise their intellectual prowess on the job but also to engage in intellectually demanding leisure-time activities. Nowhere in these data is there evidence that men turn their occupational frustrations loose on the nonoccupational world or try to find compensation in nonoccupational realities for occupational lacks and grievances.

Finally, our findings bear on the issue of whether men similarly located in the structure of society come to share beliefs and values because they experience similar conditions of life or because of some interpersonal process of value-transmission. Marx and the structuralists would have us believe that the former is basic; theorists as diverse as the "human relations in industry" and "culture of poverty" schools stress the latter. Our findings, emphasizing as they do the structural imperatives of the job and deemphasizing the importance of interpersonal relatedness, support the argument of the structuralists. A man's job affects his perceptions, values, and thinking processes primarily because it confronts him with demands he must try to meet. These demands, in turn, are to a great extent determined by the job's location in the larger structures of the economy and the society. It is chiefly by shaping the everyday realities men must face that social structure exerts its psychological impact.

REFERENCES

- Becker, Howard S. and James W. Carper
1956 "The development of identification with an occupation." *American Journal of Sociology* 61 (January):289-98.
- Blalock, Hubert M., Jr.
1960 *Social Statistics*. New York: McGraw-Hill.
1971 "Simultaneous equations techniques." Pp. 153-7 in H. M. Blalock (ed.), *Causal Models in the Social Sciences*. Chicago: Aldine-Atherton.
- Blau, Peter M.
1955 *The Dynamics of Bureaucracy: A Study of Interpersonal Relations in Two Government Agencies*. Chicago: University of Chicago Press.
1960 "Structural effects." *American Sociological Review* 25 (April):178-93.
- Blauner, Robert
1964 *Alienation and Freedom: The Factory Worker and His Industry*. Chicago: University of Chicago Press.
1966 "Work satisfaction and industrial trends in modern society." Pp. 473-87 in Reinhard Bendix and Seymour Martin Lipset (eds.), *Class, Status, and Power*. Glencoe, Illinois: Free Press. Second Edition.
- Breer, Paul E. and Edwin A. Locke
1965 *Task Experience as a Source of Attitudes*. Homewood, Illinois: The Dorsey Press.
- Chinoy, Ely
1955 *Automobile Workers and the American Dream*. Garden City, New York: Doubleday.
- Clyde, Dean J., Elliot M. Cramer and Richard J. Sherin
1966 *Multivariate Statistical Programs*. Coral Gables, Florida: Biometry Laboratory of the University of Miami.
- Cooley, William W. and Paul R. Lohnes
1962 *Multivariate Procedures for the Behavioral Sciences*. New York: John Wiley.
- Cottrell, W. Fred
1940 *The Railroader*. Stanford, California: Stanford University Press.
- Duncan, Otis Dudley
1968 "Ability and achievement." *Eugenics Quarterly* 15 (March):1-11.
- Duncan, Otis Dudley, Archibald O. Haller and Alejandro Portes
1968 "Peer influences on aspirations: a reinterpretation." *American Journal of Sociology* 74 (September):119-37.
- Edwards, G. Franklin
1959 *The Negro Professional Class*. Glencoe, Illinois: Free Press.
- Foote, Nelson N.
1953 "The professionalization of labor in Detroit." *American Journal of Sociology* 58 (January):371-80.
- Freidson, Elliot
1970 *Profession of Medicine: A Study in the Sociology of Applied Knowledge*. New York: Dodd, Mead, and Co.
- Goldberger, Arthur S.
1964 *Econometric Theory*. New York: Wiley.

- Goldthorpe, John H., David Lockwood, Frank Bechhofer and Jennifer Platt
 1968 *The Affluent Worker: Industrial Attitudes and Behavior*. Cambridge: University Press.
- 1969 *The Affluent Worker in the Class Structure*. Cambridge: University Press.
- Hughes, Everett Cherrington
 1958 *Men and Their Work*. Glencoe, Illinois: Free Press.
- Kohn, Melvin L.
 1969 *Class and Conformity: A Study in Values*. Homewood, Illinois: The Dorsey Press.
- 1971 "Bureaucratic man: a portrait and an interpretation." *American Sociological Review* 36 (June):461-74.
- Kohn, Melvin L. and Carmi Schooler
 1969 "Class, occupation and orientation." *American Sociological Review* 34 (October): 659-78.
- Kornhauser, Arthur
 1965 *Mental Health of the Industrial Worker: A Detroit Study*. New York: John Wiley.
- Lipset, Seymour Martin, Martin A. Trow and James S. Coleman
 1956 *Union Democracy: The Internal Politics of the International Typographical Union*. Glencoe, Illinois: Free Press.
- Mason, Robert and Albert N. Halter
 1968 "The application of a system of simultaneous equations to an innovation diffusion model." *Social Forces* 47 (December): 182-95.
- Menzel, Herbert
 1950 *The Social Psychology of Occupations: A Synthetic Review*. Unpublished M.A. Thesis, Indiana University.
- Miller, Alden Dykstra
 1971 "Logic of causal analysis: from experimental to nonexperimental designs." Pp. 273-94 of H. M. Blalock (ed.), *Causal Models in the Social Sciences*. Chicago: Aldine-Atherton.
- Mills, C. Wright
 1953 *White Collar: The American Middle Classes*. New York: Oxford University Press.
- Morris, Richard T. and Raymond J. Murphy
 1959 "The situs dimension in occupational structure." *American Sociological Review* 24 (April):231-9.
- Nie, Norman H., Dale H. Bent and C. Hadlai Hull
 1968 *Statistical Package for the Social Sciences: Provisional Users Manual*. Chicago: National Opinion Research Center (mimeo).
- Pearlin, Leonard I.
 1962 "Alienation from work: a study of nursing personnel." *American Sociological Review* 27 (June):314-26.
- Robinson, John P., Robert Athanasiou and Kendra B. Head
 1969 *Measures of Occupational Attitudes and Occupational Characteristics*. Ann Arbor, Michigan: Survey Research Center, Institute for Social Research (mimeo).
- Rosenberg, Morris
 1957 *Occupations and Values*. Glencoe, Illinois: Free Press.
- Schooler, Carmi
 1972 "Social antecedents of adult psychological functioning." *American Journal of Sociology* 78 (September):299-322.
- Sudman, Seymour and Jacob J. Feldman
 1965 "Sample design and field procedures." Pp. 482-5 of Appendix 1 in John W. C. Johnston and Ramon J. Rivera (eds.), *Volunteers for Learning: A Study of the Educational Pursuits of American Adults*. Chicago: Aldine.
- Turner, Arthur N. and Paul R. Lawrence
 1965 *Industrial Jobs and the Worker: An Investigation of Response to Task Attributes*. Boston: Harvard University Graduate School of Business Administration.
- United States Department of Labor
 1949 *Dictionary of Occupational Titles*. Washington, D.C.: U.S. Government Printing Office. Second edition.
- 1965 *Dictionary of Occupational Titles*. Washington, D.C.: U.S. Government Printing Office. Third edition.
- Walker, Charles R.
 1957 *Toward the Automatic Factory: A Case Study of Men and Machines*. New Haven: Yale University Press.
- Walker, Charles R. and Robert H. Guest
 1952 *The Man on the Assembly Line*. Cambridge: Harvard University Press.
- Whyte, William Foote
 1961 *Men at Work*. Homewood, Illinois: The Dorsey Press.
- Williams, Robin M., Jr.
 1960 *American Society: A Sociological Interpretation*. New York: Knopf. Second edition.
- Witkin, H. A., R. B. Dyk, H. F. Faterson, D. R. Goodenough and S. A. Karp
 1962 *Psychological Differentiation: Studies of Development*. New York: John Wiley.

MANAGERIAL MOBILITY MOTIVATIONS AND CENTRAL LIFE INTERESTS

DANIEL R. GOLDMAN
Wayne State University

American Sociological Review 1973, Vol. 38 (February):119-126

Career anchorage points and central life interests have been measured separately in a number of studies. Using 489 middle managers and specialists in seven American industries, I have related the two measures to each other. The data indicate that regardless of age, education, level of labor force entry and present position, upwardly anchored managers and specialists are more work-oriented than are ambivalently or downwardly anchored managers and specialists. The evidence supports the conclusion that the commitment to work as central life interest depends, at least in part, on the individual manager's and specialist's career orientation anchorage.

IN 1965 the idea of career anchorage points and the anchorage of career perspectives was introduced into the literature (Tausky and Dubin, 1965). Dubin (1956) originated a measure of the central life interests which he applied to a sample of midwestern industrial workers. Both measures have been incorporated in more current studies. But until recently each idea had been used in separate studies and applied to different levels within the American occupational system.

Goldman (1968) used both measures on the same sample of middle level managers and specialists, thus enabling us to see how the same sample of respondents reacted to the two measures. However, the relation of these measures to one another has not been studied. That is, orientation toward a particular career mobility perspective might have an impact on the individual's commitment or noncommitment to work as a central life interest. This study finds such a relationship and discusses its implications.¹

Sociological literature contains a number of formulations dealing with the motivation for occupational mobility. These statements can be classified into two types. The first views the actor in a stratified occupational

system as motivated to attain the most highly rewarded positions within his occupational system (Veblen, 1953; Lipset and Zetterberg, 1956; Parsons, 1954). "Limited success" models assume the actor to be satisfied with lesser positions which offer security and which are not overly difficult to attain (Mills, 1956; Whyte, 1956; Riesman, 1961). While theorists in either framework may have begun from different premises, each appears to have arrived at the same theoretical position toward the orientation to occupational mobility within their particular framework.

Our position is that the two models are not competing, that both incorporate the same motivational mechanism, the anchorage of career perspectives.

The underlying idea of career anchorage is that some individuals value top-level positions highly and strive for them throughout their occupational lives, while others value occupational progress already experienced. Thus the alternatives for evaluating one's career are either to anchor the point reference on a level of ultimate possible attainment, or on a career origin. These two polar orientations were designated as "upward" and "downward" anchorage (Tausky and Dubin, 1965:726). Scale types also indicated a third category: ambivalent anchorage.² Both Tausky and Dubin (1965:729) and Goldman (1968:37) found only a minority of middle level managers and specialists who appeared to be com-

¹ The underlying theoretical positions for each idea are fully discussed in the original studies. See Tausky and Dubin (1965:725-6) on career anchorage and Dubin (1956:131-3) on central life interests. See also a study of the central life interests of middle managers and specialists using the same theoretical position (Dubin and Goldman, 1972: 133-41).

² See the methods section of this report for an explanation of ambivalent career anchorage.

mitted to a career perspective anchored on organizational peaks. The largest percentage in both studies were either ambivalently or downwardly anchored, viewing a career in terms of movement from an occupational starting point. Endo (1971:3653-A) confirmed these findings in a study of Japanese middle managers in Kanazawa, Japan. Bowin's (1971:4958-A) study of 348 male middle management personnel in Portland, Oregon, generally confirmed these findings.³

In the last several years studies of central life interests have become more numerous. These studies have attempted to assess the value of Dubin's original formulation of the CLI. An individual may be said to have a central life interest in work when he chooses the work setting as the preferred locale for behaviors that have an approximately equal likelihood of being carried out in nonwork settings. The original research indicated that for almost three out of every four industrial workers studied, work and the work-place were not central life interests (Dubin, 1956: 131). Elsewhere, Dubin suggested that work for probably a majority of workers and even for management, may represent an institutional setting that is not a central life interest for its participants (Dubin, 1958: 161).

Results of several studies of the central life interests of middle level managers and specialists lend support to Dubin's position. Goldman (1968) reported that for almost three out of every five managers and specialists studied, the workplace was not their central life interest. Bowin (1971:4958-A) indicated that middle managers in banking and manufacturing were more non-work oriented. Maurer (1968:331-2) found that

³ The statistical breakdown of three of the four studies is as follows:

	Tausky & Dubin (%)	Gold- man (%)	Endo (%)
Career Anchorage			
Upward Anchorage	10	14	7
Ambivalent Anchorage	43	43	50
Downward Anchorage	47	44	43
(N)	(308)	(493)	(...)

Endo (1971) indicates that his data did not differ significantly from the previous studies of American middle managers. Bowin did not report his statistical breakdowns. They, therefore, are not compared.

54% of the 331 first-through-third level supervisors he studied in six Michigan firms were work-oriented as regards their central life interest. Finally Endo (1971:3653-A) found that 82% of the Japanese middle managers surveyed were work-oriented. He concluded that apparently Japanese societal values and organizational practices were effective in developing a greater commitment to work as a central life interest.

METHOD

In the summer of 1966 a questionnaire containing both the Career Orientation Anchorage Scale (COAS) and the Central Life Interest Instrument (CLI) were distributed in seven business firms, six located in the Midwest and one in the Northwest. Six companies were engaged in either light or heavy manufacturing and one was a financial concern. They include two steel companies, a vertically integrated lumber and paper corporation, a maker of paints, waxes, finishes, and wallpaper, a firm producing large plastic signs for national corporations, a big city bank, and a mattress and boxspring manufacturer. Using a mailback system with two followups, 489 male, middle managers and specialists, representing 85% of all those contacted, returned usable questionnaires.⁴ Operationally, middle managers included men below the level of vice-president and above the level of clerk (Tausky and Dubin, 1965:726). Since all middle level personnel were included, this permitted some comparison between line and staff personnel. An official of each firm in our sample prepared a listing of all respondents who met our criterion. In each individual case, the official of the firm designated whether the participant was a manager or specialist.

Career anchorage orientations were measured by the original Tausky and Dubin

⁴ No statistical analysis was done to determine the characteristics of nonrespondents since the nonresponse rate was within acceptable limits. A review of the rosters of personnel indicated that the least response rate from any organization was 82%. This occurred in the organization with the largest population. In addition, the rosters indicated that the nonresponse rate was somewhat higher in the lower management positions. We feel that the sample is representative of the participants in the seven organizations.

scale (Tausky and Dubin, 1965:726-8). The COAS is a six item, Guttman-type scale with a reproducibility coefficient of .87 in both studies. COAS responses indicated, as in the original study, that three categories of career orientations existed: upwardly anchored, downwardly anchored, and ambivalently anchored. The ambivalent category is a statistical artifact, consisting of scale scores in the middle of an ordinal scale. The lack of a clear preponderance of either upwardly or downwardly-oriented choices indicates uncrystallized career perspectives with respect to anchorage.

The CLI questionnaire consists of thirty-two items, eight being used in each of four sectors of behavior: in informal personal relations; in areas of general life experience; in formal organizations; and in relation to technical environments. Each item in the instrument specifies a single behavior. Alternative settings for carrying out that behavior are posed; in particular, a work versus a nonwork setting. The respondent is, for example, asked to select the setting in which he prefers to enact that behavior (thus: When I take a vacation I prefer to do so with: people from work; my family; anyone). A third response to each item representing indifference was also possible. Scores indicating work or non-work orientation were obtained for each respondent in each sector of experience and a summary score was ascertained for the entire thirty-two items.

A job-oriented score was determined in the following way. At least half of all the items had to be scored job-oriented in each sector for the individual to be categorized in this manner. If three-quarters of the responses were equally divided between the job-oriented and indifferent categories, the individual also was classed as job-oriented (for each sector this meant that there must be three job-oriented and three indifferent responses among the eight items in order to score job-oriented). Summary scores not meeting these criteria resulted in a non-job-oriented classification. The summary job-oriented categorization for the entire instrument met the criterion when either half of all the items were answered with a job-oriented choice, or when any twenty-two of the total thirty-two items were answered with

a minimum combination of thirteen job-oriented and nine indifferent responses (Dubin and Goldman, 1972:136-7).

RESULTS

Table 1 indicates the basic relationship between career anchorage points and central life interests. It is clear that the commitment to work as a central life interest is strongly related to upward career anchorage. Almost two out of three upwardly anchored managers and specialists could be classified job-oriented as regards their central life interests. On the other hand, slightly less than two out of five downwardly and ambivalently anchored managers could be so classed.

A logical question to ask of the data in Table 1 and the tables that follow is, "How does the mobility experienced effect the relationship between career orientation anchorages and central life interests?" The data collected for this study did not attempt to assess the amount of mobility which had already been experienced, though it is clear, as is the case of blue collar labor force entrants, that occupational mobility had taken place in the history of many respondents. Career anchorages are motivational mechanisms which measure the anchorage of career perspectives. The anchorage of career perspectives does not assess the actual mobility experienced.

In order to determine the nature of the relationship between career anchorages and central life interests, two lambdas were run on the data in Table 1. While lambdas do not generally yield high correlations, they are useful in the analysis of 2 x 3 tables where one is interested in the impact of two variables on each other. Our correlations

Table 1. Percentage of Central Life Interests by Career Anchorage

Career Anchorages	Central Life Interests		
	Work	Nonwork	(N)
Upward	64%	36%	(66)
Ambivalent	38	62	(209)
Downward	39	61	(214)

are no exception in that the score value is not high. The evidence supports the position taken in this paper that the commitment to work as a central life interest depends in part on the individual's orientation anchorage, $L_{xy} = .09$. That $L_{yx} = .00$ does not support the reverse, that commitment to work reinforces upward career anchorage. While very little of the central life interest of middle managers and specialists is explained by its relationship to career anchorages, L_{xy} did predict in 62 percent of the cases.

In Tables 2-5, present position, initial level of labor forces entry, age and education are examined for their effects on the basic relationship between career orientation anchorages and central life interests. Our attention is particularly focused on the impact of these variables on upward career anchorage and central life interests. Examination of Table 1 suggests that the commitment to work as a central life interest depends in part on career anchorages. At the same time a re-examination of data on central life interests suggests that the above variables also affected orientations to work (Goldman, 1968). For example, as age increased, the percent committed to work as a central life interest decreased. Sixty-one percent of those aged forty-six years and older were non-work-oriented (Goldman, 1968:123). The percent work-oriented among blue collar, lower white collar, and management trainee entrants into the labor force was quite low, ranging between 35% and 40%. On the other hand, one half of all specialists entrants indicated a work orientation (Goldman, 1968:117). If the four variables affect the basic relationships, then regardless of the career orientation anchorage, our results should be similar to our prior findings and the relationship in Table 1 would be spurious.

Table 2 examines the relationship between the current position held in middle management, career anchorages, and central life interest. Regardless of present position, the upwardly anchored middle managers and specialists are substantially more work-oriented than are either the ambivalently or downwardly anchored middle managers and specialists. Relatively small variations in orientation occur between middle

Table 2. Percentage of Central Life Interests by Career Anchorages and Present Position

Career Anchorages and Present Position	Central Life Interests		
	Work	Nonwork	(N)
Upward			
Middle managers	65%	35%	(54)
Specialists	58	42	(12)
Ambivalent			
Middle managers	40	60	(168)
Specialists	32	68	(41)
Downward			
Middle managers	39	61	(174)
Specialists	40	60	(40)

managers and specialists who are upwardly or ambivalently anchored. Current position does not discriminate with regard to central life interests for the downwardly anchored. The data suggest that the career anchorage options have a greater effect on central life interests than does present managerial position.

Data on initial level of labor force entry does little to alter the basic findings in the study. With the exception of lower white collar entrants, regardless of the level of entry, the upwardly anchored are substantially more work-oriented than are the am-

Table 3. Percentage of Central Life Interests by Career Anchorages and Initial Level of Labor Force Entry

Career Anchorages and Initial Position	Central Life Interests		
	Work	Nonwork	(N)
Upward			
Blue collar	58%	42%	(12)
Lower white collar	38	62	(13)
Management trainee	58	42	(12)
Specialists	76	24	(25)
Ambivalent			
Blue collar	37	63	(38)
Lower white collar	34	66	(35)
Management trainee	38	62	(53)
Specialists	36	64	(74)
Downward			
Blue collar	39	61	(90)
Lower white collar	27	73	(49)
Management trainee	19	81	(27)
Specialists	51	49	(49)

bivalently or downwardly anchored middle managers. For the ambivalently anchored managers, initial level of labor force entry has no effect on central life interest. Almost two out of every three ambivalently anchored, middle managers and specialists are non-work-oriented regardless of their initial level of entry into the labor force.

Among downwardly anchored managers, specialist career entrants exhibit a substantially high proportion of work orientation than do others. As the occupational entry ladder is ascended between blue collar entrants and management trainee entrants, the percent work-oriented decreases sharply.

The higher proportion of work-orientation among both upwardly anchored and downwardly anchored specialist entrants is of interest for several reasons. A rather large number of those classified as specialist entrants are professional personnel, such as chemists, engineers, metallurgists, and so on. These individuals along with those who entered the work force in other specialties, such as accountants, public relations representatives, quality control supervisors, and so on, indicated a greater commitment to work as a central life interest than did managers with other entry levels (Goldman, 1968). This outcome occurred regardless of the specialist entrants' current position in their work organizations when central life interests were analyzed separately. Sixty-six percent of the specialist entrants in the sample had moved to general management. The data in the original analysis of central life interests indicated that whether or not the specialist entrants were located in low or high middle management or remained in a specialty, the percent work oriented remained at approximately the same level (Goldman, 1968:119). In other words, little or no alteration occurred in the commitment to work as a central life interest when specialist entrants moved out of their specialties and into managerial positions.

On the other hand, those who moved into a specialist position from other entry points were substantially less work-oriented than current specialists with specialist entry points (Goldman, 1968:119). The data in Table 2 indicate that in each category of career anchorage, current specialists are less work-oriented than are specialist entrants

in Table 3. It appears that the lesser degree of commitment to work as a central life interest among current specialists is due to the filtering into the specialties of personnel of other career entry points rather than to a change in orientation of specialists of specialist career origin. Our data suggest that generally those with specialist career origins value work as a central life interest more than others regardless of their career orientation anchorage. But it is notable that upwardly anchored specialist entrants are more work-oriented than are downwardly anchored specialist entrants.

Table 4 indicates that regardless of age, the upwardly anchored managers in this study are more work-oriented than are ambivalently or downwardly anchored managers. For upwardly anchored middle managers, work orientation decreases slightly through age thirty-five and then increases substantially between ages thirty-six through forty-five. After age forty-five the percent work-oriented decreases to its initial level. Work orientation is greatest in the age period thirty-six through forty-five, suggesting that this may be a period of advancement into new positions. A majority or better of the upwardly anchored managers remain committed to work as a central life interest regardless of age.

Ambivalently and downwardly anchored

Table 4. Percentage of Central Life Interests by Career Anchorages and Age

Career Anchorages and Age	Central Life Interests		
	Work	Nonwork	(N)
Upward			
Through 30	59%	41%	(17)
31-35	50	50	(4)
36-45	78	22	(18)
46 & over	58	42	(26)
Ambivalent			
Through 30	48	52	(50)
31-35	50	50	(36)
36-45	25	75	(61)
46 & over	35	65	(60)
Downward			
Through 30	56	44	(25)
31-35	37	63	(19)
36-45	37	63	(75)
46 & over	36	64	(92)

younger managers are similar to upwardly anchored managers with respect to their central life interest, with about half expressing a work-oriented preference. Ambivalently anchored managers appear to sustain a commitment to work as a central life interest for a relatively longer period than do the downwardly anchored, but with increasing age both ambivalently and downwardly anchored managers become substantially non-work-oriented.

Age has a definite impact on central life interests, but only for the ambivalently and downwardly anchored managers. This fact suggests that central life interests are not static. Changing interests and changing career orientation anchorages may alter the significance of work as a central life interest. The general tendency regardless of age is for the ambivalently and downwardly anchored to be less work-oriented than their upwardly anchored counterparts.

Education as a variable proved to be less discriminating than career anchorage points with respect to central life interests. Table 5 presents the relationship between education, career anchorage points, and central life interests. Regardless of the level of education, the upwardly anchored are substantially more work-oriented than are the ambivalently and downwardly anchored. The differences between educational categories within each career anchorage are rather modest compared to the differences between career anchorage categories with educational level held constant. Not only are the upwardly anchored substantially more work-oriented, but the data indicate a marked similarity among the downwardly and ambivalently anchored in their orientation towards work as a central life interest, with education held constant. The vast majority of the ambivalently and downwardly anchored managers in each educational group are non-work-oriented.

CONCLUSIONS

The data indicate a consistent and relatively strong relationship between upward career anchorage and positive work-orientation. At the same time, a substantial proportion of non-work-orientation appears to exist among middle managers and special-

Table 5. Percentage of Central Life Interests by Career Anchorages and Education

Career Anchorages and Education	Central Life Interests		
	Work	Nonwork	(N)
Upward			
H.S. or less	69½	31½	(16)
College	58	42	(36)
Post-graduate	69	31	(13)
Ambivalent			
H.S. or less	34	66	(47)
College	40	60	(96)
Post-graduate	38	62	(63)
Downward			
H.S. or less	38	62	(104)
College	42	58	(85)
Post-graduate	30	70	(20)

ists with ambivalent and downward career anchorages.

It can be argued that a person on the way up may be in a situation that gives him cues that mobility is possible or likely, and thus his work becomes more central to him. We do not take issue with this position, since it is a possibility. Our data do not measure actual mobility, but rather orientations toward occupational mobility. The findings of this paper lend some credence to the above argument even though actual mobility has not been tapped. Such a position still fails to explain the meaning of the commitment to work as a central life interest for those who view upward mobility as possible or likely. Two alternative explanations appear to be feasible: (1) that the upwardly anchored are thoroughly committed to work as a central life interest. We would anticipate under this condition that any re-orientation of career perspectives would have little or no effect on the commitment to work as a central life interest. The commitment to work, then, is more or less total. Or (2) the upwardly anchored's commitment to work is largely instrumental. Its goal is to attain high office in the work organization.

Our data, we believe, suggest the second interpretation. The relatively high proportion of non-work-orientation among ambivalently and downwardly anchored managers observed in this study suggests that shifts

in career orientation anchorages, particularly from an upward anchorage, affect the commitment to work as a central life interest. In other words, our prediction, based on the data, is that shifts from an upward career orientation to an ambivalent or downward career orientation increase the proportion of non-work orientation among those previously upwardly anchored.⁵

Work and career for those who are and remain ambivalently or downwardly anchored may have a rather different meaning. The substantial proportion of non-work-orientation among these managers suggests that they view work and career as means to an end. But unlike upwardly anchored managers, they may view work and career as a means to success in other institutional settings.

Dubin and Goldman (1972:140) in a study of the central life interests of middle managers and specialists concluded that the majority of managers and specialists in American business organizations do not regard their work environment as a central life interest. Furthermore, in spite of a general preference for carrying out behaviors in nonwork settings, important attachments to work organizations did exist. These attachments focused on the characteristics of formal organizations and the technical features of work. They lent further support to the general conclusion reached in the original study that people may become attached to an environment without its being central to their life interests (Dubin, 1956). This conclusion is in keeping with the model that subdivides behavior in a setting into hygienic and motivating sources of attachment (Herzberg, Mausner, and Snyderman, 1959). The Dubin and Goldman study (1972:140) pointed out that attachment to work through the formal and technological behavior systems may be sufficient grounds for producing effective performances on behalf of the organization at the hygienic level alone. The motivating features of personality need not be located in the same behav-

ioral setting as the hygienic supports for the personality (Dubin and Goldman, 1972:141). Dubin and Goldman's conclusions complement other criticisms of Herzberg's two factor theory (e.g., Dunnette, Campbell, and Hakel, 1967; House and Wigdor, 1967).

This study calls for some modification in the stance of Dubin and Goldman. For under certain circumstances, Herzberg's two-factor theory may be quite viable, in the sense that his dynamics, the motivating features of the personality under certain conditions may be located in the same behavioral setting. If, as the data suggest, commitment to work as central life interest depends in part on career orientation anchorages, particularly upward career anchorage, then the maintenance of a central life interest in work may depend in part on these participants' abilities to self-actualize and achieve personal growth within the work environment. This is to say that both dynamics may be important to strivers in large-scale organization; and it may be that the motivating factors—recognition, achievement, advancement, responsibility and work itself—are the primary factors for sustaining the commitment to work as a central life interest among upwardly anchored managers and specialists in American industry and business.

REFERENCES

- Bowin, Robert Bruce
 - 1971 "Career anchorage points and central life interests of middle managers." Dissertation Abstracts 31, 10 (April):4958-A.
- Dubin, Robert
 - 1956 "Industrial workers' worlds: a study of the central life interests of industrial workers." *Social Problems* 3 (January):131-42.
 - 1958 "Industrial research and the discipline of sociology." *Proceedings of the Eleventh Annual Meeting, Industrial Relations Research Association, Madison, Wisconsin* XI:152-72.
- Dubin, Robert and Daniel R. Goldman
 - 1972 "Central life interests of American middle managers and specialists." *Journal of Vocational Behavior* 2 (April):133-41.
- Dunnette, M.D., J. P. Campbell, and M. D. Hakel.
 - 1967 "Factors contributing to job satisfaction and job dissatisfaction in six occupational groups." *Organizational Behavior and Human Performance* 2:143-74.
- Endo, Calvin Masao
 - 1971 Career anchorage points and central life

⁵ The alternative explanations and predictions are suggestive. Ours are not temporal data. In order to confirm our prediction we would need to follow the careers of the sixty-six upwardly anchored managers in this study to their conclusion or to the point where upward mobility ceases.

- interests of Japanese middle managers. Dissertation Abstracts 31, 7 (January): 3653-A.
- Goldman, Daniel Roger
1968 Career Anchorage Points and Central Life Interests of Middle Managers. Unpublished Ph.D. dissertation. Eugene, Oregon: Department of Sociology, University of Oregon.
- Herzberg, F., B. Mausner and B. B. Snyderman
1959 The Motivation to Work. New York: Wiley.
- House, R. J. and L. A. Wigdor
1967 "Herzberg's dual-factor theory of job satisfaction and motivation: a review of the evidence and a criticism." Personnel Psychology 20 (Winter):369-89.
- Lipset, Seymour M. and Hans L. Zetterberg
1956 "A theory of social mobility." Transactions of the Third World Congress of Sociology 2:155-77.
- Maurer, John G.
1968 "Work as a central life interest of industrial supervisors." Journal of the Academy of Management 11 (September):329-39.
- Mills, C. Wright
1956 White Collar. New York: Oxford University Press.
- Parsons, Talcott
1954 "An analytical approach to the theory of social stratification." Essays in Sociological Theory, Glencoe, Illinois: The Free Press.
- Riesman, David
1961 The Lonely Crowd. New Haven: Yale University Press.
- Tausky, Curt and Robert Dubin
1965 "Career anchorage: managerial mobility motivations." American Sociological Review 30 (October):725-35.
- Veblen, Thorstein
1953 The Theory of the Leisure Class. New York: Mentor Books.
- Whyte, William H., Jr.
1956 The Organization Man. Garden City, New York: Doubleday Anchor.

CHEMISTS IN BRITISH UNIVERSITIES: A STUDY OF THE REWARD SYSTEM IN SCIENCE *

S. S. BLUME

Civil Service College, London

RUTH SINCLAIR

Loughborough University of Technology

American Sociological Review 1973, Vol. 38 (February):126-138

This paper reports on a study of the scientific output of British University chemists. The relationships between three different aspects of scientists' work (quantity, quality and industrial orientation) were examined, and whilst quantity and quality were highly correlated, industrial orientation appeared to represent a different dimension of research performance. Quality of output was the best predictor of the recognition received by a chemist from the scientific community (in terms of medals, appointment to advisory committees, etc), his industrial involvement much the least good. It appears that a minority of British university chemists are oriented towards the industrial, rather than the basic research community in terms of their contributions and of their rewards. The influence of institutional affiliation upon recognition, for a given level of output, was examined. As in studies of American scientists, but in contrast to earlier work on British ones, institutional affiliation did appear to influence the recognition received.

THE current sociological notion of science emphasizes its communal nature. Only by considering scientists as forming a community can certain typical features of scientific behaviour be explained (Hagstrom, 1965: Chapter 1). These would include, for example, scientists' need to

publish the results of their work in one of a few "appropriate" journals, as well as the premium placed upon originality both by scientists in their choice of problems for study and by journals in accepting papers for publication. Added legitimacy has been given to such a view of science by the philosophical treatment of the scientific method developed by Thomas S. Kuhn (1962). Kuhn suggested that the complex of values, theories, techniques and problems characterizing any scientific speciality forms a communal tradition largely

* We would like to thank the Department of Education and Science and the Council for Scientific Policy, London, for financial support and encouragement; Mr. Murray Sinclair for his invaluable help with statistical matters, and Dr. Diana Crane for criticisms of an earlier draft of this paper.

insulated from outside influence. The basis of scientific organization may be regarded as the exchange of social recognition for information, (Hagstrom, 1965) or as the social validation of creative achievement (Storer, 1966). By controlling the allocation of rewards, the scientific community is at once able to maintain the values and effectiveness of science as well as the commitment of scientists, and to invoke sanctions against those who fail to conform. Recognition is allotted to an individual to the extent to which he has fulfilled his scientific role: to the extent, that is, to which he has furthered the scientific endeavour (Merton, 1957).

THE PRESENT STUDY

This paper is an attempt to describe aspects of the reward system of science as it functions within the chemical community of the United Kingdom. We should add that the data were collected as part of a larger study with somewhat different objectives (Blume and Sinclair, 1973). There are fifty-eight university departments of chemistry in the UK (including the semi-autonomous colleges of London University and of the University of Wales), ranging in size from eight to over sixty full-time members of staff. These departments contained (in December 1970) some 1,537 faculty members, 750 post-doctoral fellows, and three thousand research students. Our sample consisted of all faculty-level members of these fifty-eight departments.¹ A questionnaire based on a series of fifty preliminary interviews in six departments, was therefore sent postally to the one thousand five hundred and thirty-seven chemists. The response rate was 55%, and respondents were equivalent, in terms of their distribution by ranks at least, to the initial population surveyed.²

¹ Names were obtained by taking all those listed under the entry "chemistry" in the annual government publication "Research in British Universities and Colleges" for 1969-70, and obtaining recent corrections from departmental secretaries.

² The two distributions were as follows:

	Survey	
	Returns	Population
Professors	13.2%	12.4%
Readers/Senior lecturers	24.5%	23.4%
Lecturer/Other	62.2%	64.2%

PROBLEMS OF INDEX CONSTRUCTION

In order empirically to examine the working of the reward system it is necessary to construct operational indices of "performance" (the magnitude of an individual's contribution to the advancement of science), and of the recognition received in return. One may therefore discern in the literature a variety of attempts, of varying degrees of sophistication, at coping with these crucial problems. Consider the measurement of contributions to science. In a sense the only really valid assessment must come from within science: indeed it must come from within the specialty to which a given piece of work is pre-eminently directed, for only a specialist can judge the importance of a contribution to his speciality.³ In practice, a simple counting of published papers has often been used by sociologists, (e.g. Crane 1965; Gaston 1969), even though it is increasingly apparent that quantity of publication thus defined, cannot be identified with the quality of a man's contribution to science in any a priori fashion. Much better, and used quite frequently of late has been the "Citation Index." It has been suggested that the extent to which a paper is quoted by succeeding authors is a measure of its impact on the field, and that this may be equated with its importance as a contribution to science. The use of such a procedure has been facilitated by the development of the Science Citation Index, in which succeeding references to any given paper are listed.⁴

Each paper published by a given individual could thus be 'weighted' by its importance, and his total 'contribution' thus assessed. A number of attempts have been made to refine the index thus generated. The Coles, for example, attempted to correct for the fact that quantity of publication may attract as many citations as quality by making use not of the total citation count of each scientist, but the number of citations to his three most heavily cited

³ And the scientific system works in practice largely by delegating responsibility for such evaluations to members qualified by both specialist interest and by personal attainment. Amongst these performance-judge roles are those of journal referee, member of grant-awarding committee, etc. (Zuckerman and Merton, 1971).

⁴ For use of the S.C.I. in this way see Bayer and Folger (1966).

papers (Cole and Cole, 1967). They attempted in addition to correct for the contemporaneity of science⁵ by giving extra weight to publications which had remained significant over a prolonged period (i.e. work published some while ago which was still receiving citations). Use of the citation index in this way is clearly an excellent means of assessing the impact of an individual paper. Its use as an indicator of the overall contribution of a scientist to the advancement of his discipline is dependent upon the assumption that his contribution is wholly expressed by his published work. This may not be the case, and there are many eminent men who have guided the work of innumerable novitiates down exciting and profitable channels without ever seeking the formal credit of co-authorship. Nobel Laureates, for example, may omit their names deliberately, in order to ensure the due recognition of their co-workers' contributions (Zuckerman, 1967). Ultimately, then, it must be the direct and specific evaluation by peers of a colleague's work which is the ideal indicator of the quality of his overall research effort. This procedure was employed by Kenneth E. Clark in his study of the research-productivity of American psychologists (Clark, 1957:31-40). Clark showed that the use of citation indices was, in actual fact, a fairly accurate guide to peer group evaluation. The correlation which he obtained between citation counts and evaluation scores ($r=0.67$) was subsequently used by the Coles (1967:379) to justify their own use of the citation index. Perhaps partly in the attempt to vindicate a simple count of papers some interest has attached to the comparison of sheer quantity of research output with quality as assessed by one of the

procedures outlined above. Comparing quantity of output with quality (as given by the citation count), the Coles found that 70% of their sample of U.S. physicists fell into the high productivity/high quality or low productivity/low quality cells, and only 30% into the deviant cells (Cole and Cole, 1967). Clark, in the study of psychologists already referred to, found a (product-moment) correlation between total number of publications and total number of citations of 0.45, and between total publications and votes received (as being a significant contributor) of 0.44.

Similar problems of operationalization are involved in measuring recognition, for scientific achievement is rewarded in many ways, ranging from the most informal (such as requests for advice on matters of research strategy or the implications of an experiment); through more formal manifestations of esteem such as invitations to submit a paper or deliver a course of seminars at a foreign university; to the rarest and most formalized forms of reward (such as election to the National Academy of Sciences or the Royal Society) including, above all, the Nobel Prize, receipt of which may profoundly affect the whole of the scientist's career (Zuckerman, 1967). Also acknowledged by scientists as an appropriate reward for achievement is the request of the scientific community that the performance of others be evaluated: that is, that one serve on an editorial or grant-awarding committee, referee papers, and so on. As we have said, these tasks are delegated by the scientific community to an elite group of its members; and though they may be arduous and time-consuming in performance they are welcomed by scientists as valid tokens of achievement.⁶ In the U.S.A., where rank-orderings of departments in terms of quality have been

⁵ By the "contemporaneity" of science we refer to the fact that the majority of citations to previously published papers at any one time are to papers published within the recent past (say, within the past five years). Price has suggested that a paper published, say, in the 1930's will receive a constant number of citations year by year—but this is because each year there are many more papers published in which it may be cited than in previous years. Thus a constant number of citations is equivalent to a diminishing impact. (Price, 1963:81) For differences between fields of science in their relative citation of recent and archival papers, see Price (1970).

⁶ That such tasks are so regarded is nicely demonstrated by the reaction of the scientific community to their having been withheld from individuals on grounds other than those of scientific performance. The recent furor over the exclusion of certain individuals, on apparently political grounds, from service on grant-awarding and other panels and committees of the National Institutes of Health is indicative. In this public controversy, conducted in the pages of the periodical *Science*, many scientists referred to the importance of this kind of work from a career point of view, (Boffey, 1970:142-4).

constructed by Cartter (1966), holding a post in a top department may also be thought to indicate real contributions to science.

Sociologists have generally estimated the recognition received by an individual scientist in terms of the more formal indications referred to above: prizes, honorary fellowships, service on grant-giving committees, and so on. Empirical studies in the reward system of science have thus largely involved juxtaposing these two sets of indices: those measuring the contributions of individuals on the one hand, those measuring the recognition received in return on the other. Among such studies, the Coles have been concerned to assess the extent to which, when they do not occur together, the quality rather than quantity of publication is rewarded (Cole and Cole, 1967). Crane (1965), Gaston (1969, 1970) and Hargens and Hagstrom (1967), on the other hand, have sought to assess the salience of such non-universalistic factors as educational and social background, institutional affiliation, in the receipt of recognition for contributions to science.

INDICATORS EMPLOYED

Our data enabled us to assess both the quantity and quality of our respondents' work, and to give independent scores on each parameter. Since, for reasons primarily connected with other aspects of our study, we were particularly interested in the chemists' current and recent activities, all our indicators were designed to reflect their most recent research effectiveness and rewards. For this reason we took as our indicator of 'quantity of research production' the number of papers published over the past five years (and including papers accepted for publication but which had not yet appeared): it was assumed that such a procedure had the additional advantage of alleviating the need to stratify for professional age.⁷ Mirroring the procedure used

by Hagstrom (1967) and by Gaston (1969), numbers of papers were respondents' own count of their publications in the period. Review articles and books published were excluded for this purpose, since they are not used to report original research by chemists and fulfil a different function within the communication system of science. 'Quality of research production' was defined by peer group assessment. Respondents were asked the questions "In your research area which researchers do you feel are the 'pacemakers' (make the most valuable contributions to the body of knowledge)" firstly "in Britain," and secondly "in the World," and were given scores for quality on the basis of votes received.⁸ Each respondent was asked to make three nominations in each category.

In addition we introduced an indicator designed to reflect an aspect of the nature of research not hitherto referred to by

moment correlation of 0.52 between total research and production over the past four years for his sample of psychologists.

⁸ Total scores were calculated by allowing one point for a mention in the UK list, and three points for a mention in the world list. Clark's (1957) procedure was more complicated. His respondents were presented with a list of the names of the most highly productive psychologists, and asked to select (a) the twenty-five psychologists whom they thought had made the most significant contributions to the discipline as a whole, and (b) the ten psychologists whom they felt had made the most significant contributions to that of five pre-selected areas of psychology with which they were most familiar. From these lists a single list of 150 most eminent psychologists was prepared. We admit that our not having pre-defined the research areas poses difficulties, but we must point out that this *could not be done* in any way acceptable to our fifty initial interviewees. In addition it could be argued that since some areas of research are larger than others, scores should have been normalized with respect to size of research area. We did not do this principally because we believe that the availability of rewards is also in some way a function of the size of the research area. Moreover a similar problem would arise in using citations as measures of quality. Our index was not as discriminatory as might have been wished: only 25% of respondents received any votes, and they were grouped into low (1-3 scorers (15%)) and high (4 or more scorers (10%)). In part this lack of discriminating power was a consequence of the obtrusiveness of the measure: about half the respondents failed to complete the question, many making clear their specific objection to it.

⁷ Over 80% had been carrying out at least post-doctoral research for five or more years. It is probable that the relationship between this indicator, productivity, (a concept which entails a notion of production/time), and total quantity of production is inexact. Clark (1957) found a product

sociologists. We had hoped in some way to evaluate the external value, or relevance, of specific contributions to research, and thence to assess the extent to which the scientific community appears to be affected in its judgments by such considerations. In practice the best we could do was to give respondents scores for their industrial involvement since no operational means of assessing the value of their *work* to society-at-large could be found.⁹ The index, as we have defined it, may be said to reflect the conscious orientation of respondents to the problems of applied science. It is not an estimate of the potential applicability of their work in industrial development.

We may add at this point that the extent to which scientists are biased by such industrial considerations in their recognition of achievement, though it may represent a departure from normative behaviour, has important implications for the use by government agencies or industry of panels

of scientists in project assessment. Finally an index of recognition was constructed, based upon a wide range of formal and less formal honours and indications.¹⁰

QUANTITY AND QUALITY OF RESEARCH AND INDUSTRIAL INVOLVEMENT

Earlier work, such as that of the Coles (1967) to which we have referred, suggested some relationship between quantity and quality of research contributions. Our data confirm that a chemist who has produced a substantial number of papers in recent years is more likely than one who has not to be regarded as making a significant contribution to his field. Thus, we have divided our respondents into three groups on the basis of their productivity: 44% who have published ten or fewer papers within the past five years (low producers), 42% who have published eleven to thirty papers within the past five years

⁹ Scores of industrial involvement were based upon the number of patents held by respondents, the number of days they spent on industrial consultancy in the past year, and the number of Science Research Council 'CAPS' awards they had held. (This is a predoctoral fellowship scheme in which fellowship funds are allocated to projects submitted jointly by an academic and an industrial scientist. Projects are then evaluated in terms of both scientific and industrial value.) Total scores were calculated as follows. The 13% of respondents who held one or more patents were allotted a score of five points. 43% had acted as consultants to industrial or other user organizations. Scores allotted ranged from one point for one to two days consulting over the previous twelve months, to 5 points for twenty-one days or more. 20% of respondents held one or more CAPS awards, of whom 80% held only one. The scores given here ranged from 2 points (one award) to 6 points (three awards). The final distribution, in abridged form, was as follows:

DISTRIBUTION OF INDUSTRIAL INVOLVEMENT SCORES

Score	% Respondents
0	48.1
1	6.6
2	12.1
3	7.3
4	6.3
5	5.4
6	2.3
7	3.2
8 or more	8.7

Respondents were grouped into zero scorers (48%), those scoring 1-3 points (26%) and those scoring 4 or more points (26%).

¹⁰ The recognition index was constructed from the following information: number of expense-paid invitations abroad received within the past five years; number of senior visitors from abroad (excluding post-doctoral fellows) received in the individual's laboratory over the past five years; current service on Research Council or other grant-awarding committees; membership of the council of a learned society or editorial advisory committee; number of medals received; Fellowship of the Royal Society. Again following Hagstrom (1967) and Gaston (1969), data were 'self-reports.' The relative weightings given to each in the final index, which were approved by a panel of eminent chemists, were as follows. Expense-paid invitations abroad were scored from 1 (one invitation) to 5 (ten or more invitations); senior visitors from abroad from 1 (one visitor) to 3 (three or more visitors). Service on the editorial board of a journal received 2 points, service on government funding or advisory committees, or on the council of a learned Society, 4 points. Medals received (e.g. of the British or American Chemical Societies, of the Royal Society, etc.) were scored as 4 points (one medal), or 9 points (two or more medals). Fellowship of the Royal Society was scored as 16 points. None of the British Nobel Prize winners in chemistry (of whom about four are still active) replied. Respondents were grouped into zero scorers (37%), those scoring 1-4 points (37%) and those scoring 5 or more points (26%). One or two of the components of this index may be prone to exaggeration in reporting; for example, "number of visitors from abroad." However, the complexity of the index means that this component contributes only 15% to the average overall score, and so substantial bias is unlikely.

(medium producers),¹¹ and 14% who have published more than thirty papers (high producers). We find that 58% of the high producers, 27% of the medium producers, and only 9% of the low producers were recognised as making substantial contributions to any branch of chemistry. The relationship between quantity and quality of research is shown in Table 1, and may be well expressed by the Goodman-Kruskal correlation coefficient which has a value of 0.63.¹² (Values given are percentages of the column totals.)

We may now note that those chemists who work most closely with industry, and who are apparently most involved with problems of industrial science, are neither highly productive nor are they highly evaluated by their peers. Correlations between industrial involvement scores and indicators of quantity and quality of research production are both slight. Whether because

it has been unable to attract the interest of the most scientifically effective chemists, or for other reasons, British industry seems to have the attention of a minority of academic chemists whose commitment to the scientific community is at the same time reduced. Paradoxically, although university-industry relations in Britain have generally been regarded as less than ideal (O.E.C.D. 1972, Part IV), it is in the chemical field that they are likely to be at their best.

THE ALLOCATION OF RECOGNITION

We have now defined three indicators by means of which to characterize the recent work of individual scientists: quantity of research, quality of research, and industrial involvement. We have seen that the first two are relatively strongly associated, but that the latter is a substantially independent dimension of research effectiveness. Which is the best guarantee of recognition from the scientific community? Contrary to the common view that it is weight of publications that really counts, Cole and Cole (1967) found that the correlations between various measures of recognition and quantity of publication were very much lower than they were with quality of publication for their sample of U.S. physicists.¹³ Gaston (1969), who did not introduce an index of quality, found that British high energy physicists received recognition largely in proportion to their productivity.¹⁴ Does the reward system in chemistry appear to accord with these findings for physics? That is, are both quantity and quality of work rewarded, but the latter more than the former? Is any concession made to extrinsic value (as expressed by industrial involvement) in allocating rewards? The answers to these questions are given in Tables 2-6. (Values given are percentages of column totals.)

Firstly we may note that the third relationship is very much weaker than the

¹¹ This band was designed to include the arithmetic means of publication for the discipline as a whole (16.7 papers) and for each of the individual sub-disciplines which ranged from 13.4 to 26.4 (See Table 7).

¹² Perhaps the use of the Goodman-Kruskal requires some explanation. Its advantage over χ^2 derives from the fact that in the first place it is intended for use with ordinal level data such as we have here. It has a maximum value of 1.00. In addition it is a measure of one way association (Weiss, 1968: Chapter 11).

Table 1. Association between Productivity (number of papers published over five years) and Peer-Group Evaluation of Quality of Work

Quality Score	Productivity (number of papers published)		
	Percentages		
	Low (0-10)	Medium (11-30)	High (>30)
Zero	91	73	42
Low (1-3)	7	17	28
High (>3)	2	10	30
Total	100 (n = 348)	100 (n = 329)	100 (n = 114)
$\gamma = 0.63$			

¹³ They found correlations of 0.24-0.49 between quantity of publication and four (uncombined) indicators of recognition; correlations between quality and recognition ranged between 0.33 and 0.67.

¹⁴ He obtained a product-moment correlation of 0.65 between productivity and recognition.

Table 2. Association between Productivity (numbers of papers published over five years) and Recognition Score

Recognition Score	Productivity (number of papers published)		
	Percentages		
	Low (0-10)	Medium (11-30)	High (>30)
Zero	58	25	6
Low (1-4)	33	46	26
High (>4)	9	29	68
Total	100 (n = 348)	100 (n = 329)	100 (n = 114)
$\gamma = 0.66$			

other two. It was of course to have been expected that the external orientation of an individual's work should be of little significance in allocating rewards: that it should be would represent a departure from normative behaviour. Moreover, Table 6 shows that when productivity is held constant the association between recognition and industrial involvement falls substantially. An industrial orientation is not an important source of internal rewards. The relationships between both quantity of research on the one hand and quality of research on the other with recognition received are strong. And as found by other authors looking at

Table 3. Association between Peer-Group Evaluation of Quality of Work and Recognition Score

Recognition Score	Quality Score		
	Percentages		
	Zero	Low (1-3)	High (>3)
Zero	46	7	7
Low (1-4)	39	40	18
High (>4)	15	53	75
Total	100 (n = 610)	100 (n = 108)	100 (n = 73)
$\gamma = 0.75$			

Table 4. The Receipt of High Recognition Scores by Productivity and Quality of Output Simultaneously*

Quality	Productivity		
	Low (0-10)	Medium (11-30)	High (>30)
Zero	8	18	49
Low (1-3)	24	56	74
High (>3)	29**	68	87

* The table shows the percentages of respondents in each productivity-quality cell receiving high recognition scores.

** Inaccurate due to very small number of cases.

other groups of scientists, it is quality of research which seems to be the more important determinant of recognition. However the difference is slight. Not only does the coefficient of association between productivity and recognition fall only marginally when quality is held constant, but Table 4 confirms that quantity and quality of output may be, almost equally, sources of high scientific recognition in the UK.

A number of factors may be invoked to explain divergence from complete association. The first is the delay which frequently occurs between the completion of a piece of research and the formal recognition of the

Table 5. Association between Industrial Involvement and Recognition Received

Recognition Score	Industrial Involvement Scores		
	Percentages		
	Zero	Low (1-3)	High (>3)
Zero	51	32	21
Low (1-4)	33	33	24
High (>4)	16	35	55
Total	100 (n = 380)	100 (n = 204)	100 (n = 207)
$\gamma = 0.46$			

Table 6. Recognition Received by Productivity and Industrial Involvement Simultaneously

Industrial Involvement	Productivity								
	Percentages								
	Low			Medium			High		
	Zero	Low	High	Zero	Low	High	Zero	Low	High
Recognition Score:									
Zero	63	56	42	36	17	14	9	6	1
Low	31	32	41	50	47	42	39	24	25
High	6	12	17	14	36	44	52	70	74
Total	100 (n=211)	100 (n=75)	100 (n=64)	100 (n=143)	100 (n=101)	100 (n=80)	100 (n=23)	100 (n=29)	100 (n=65)
	$\gamma=0.27$			$\gamma=0.41$			$\gamma=0.30$		

scientist's achievement by the scientific community. As an extreme example, receipt of a Nobel Prize has often followed thirty or so years after the discoveries for which it is awarded. Thus it is that many of those in positions of honour and influence in the scientific community may no longer be among the most significant contributors to their fields of research. Secondly, the relationship between the measures of contribution and of reward may depend on the structure of the discipline: of the *homogeneity* of the disciplinary unit chosen for study. Just as Gaston had found¹⁵ that greater esteem attached to a contribution in theoretical high energy physics than to one in the experimental branch of the field, so it may well be that a similar differentiation obtains in chemistry. That is to say, in allocating such rewards as honorific medals, Fellowships of the Royal Society, in selecting government advisors and so on, greater weight may be attached to contributions in some areas of chemistry than in others. Moreover, because of the variations in the ease with which results may be obtained, and of differing standards in publication, some research areas in a discipline such as chemistry are characteristically more prolific than others. This is shown by Table 7.

¹⁵ Gaston (1969) uses such a distinction (between theory and practice, and their different statuses) to explain cases of unmerited recognition.

Other factors which may be relevant to the extent to which a given contribution to science is rewarded are the degree of differentiation of the discipline,¹⁶ and the in-

¹⁶ There may be an unequal availability of rewards between the research areas of a large discipline. In the first place, the less formal manifestations of esteem (invitations to give research seminars, or to advise on the performance of an experiment) may depend on the size of the speciality, since they are in the gift of all members. In the second place, it must be recognized that many of the sub-disciplines within such a well-

Table 7. Mean Publication Rates in the Sub-disciplines of Chemistry and Relative Sizes of Sub-disciplines

Sub-discipline	Arithmetic Mean of Papers Published: 5-year Totals	Size (percentage respondents working in the area)
Theoretical	13.4	6.0
Physical	14.6	28.5
Organic	16.9	24.6
Physical-organic	17.4	10.4
Inorganic	17.9	10.8
Physical-inorganic	18.5	8.3
Analytical	26.4	3.7

dividual characteristics of the scientist concerned. The former implies an individual's choice of research area may characteristically affect not only the availability of rewards to him, but the probability of his publishing large numbers of papers, as well as the importance which the discipline as a whole may attach to his contribution. The latter implies that the receipt of recognition may depend on not only the nature and magnitude of an individual's contributions to science, but on such non-universalistic (ascriptive) factors as his social or educational background, or his institutional affiliation.

ASCRIPTIVE FACTORS IN THE ALLOCATION OF REWARDS

Scientific norms prescribe that rewards are allocated to individuals solely on the basis of their achievement: in proportion, that is, to the magnitude of their individual contributions to the advancement of science. Rewards received not so much as a consequence of achievement, but rather because of the advantages of birth, or social or educational background, or political or religious background may truly be called unmerited. The same would be true of rewards marking ascribed status, rather than contributions to science directly: as the preferential citing of the work of the eminent (perhaps in the attempt to validate one's own contribution), or the appointment of eminent rather than productive scientists to official advisory committees. In addition, empirical research in the U.S.A. has indicated that other ascriptive factors may be relevant to the recognition accorded the like contributions of two unlike scientists. Diana Crane (1965), comparing the recognition received

by scientists for given levels of productivity, concluded that there were advantages attaching to affiliation to a major university. However, because she used no estimate of *quality* of output, her results may reflect only the higher qualitative standard of the work done in the top institutions. Indeed Hagstrom (1971) has shown that departmental prestige is highly correlated with the quality of faculty research output. It is therefore not clear whether institutional affiliation affects the receipt of recognition. However Hargens and Hagstrom (1967) found that having obtained one's Ph.D. at a major institution *was* a substantial advantage to the scientist. They invoked Turner's (1960) distinction between 'sponsored' and 'contest' mobility in explanation. The 'universalism' norm of science requires a contest mode of mobility: individualistic competition for rewards, strictly within the rules of the game. That advancement should follow upon the advantages of a favourable educational background may be regarded as sponsored mobility.¹⁷ Provided that one sets aside all those higher educational institutions which are termed non-university, British universities have generally been acknowledged (at least publicly) as similar to one another. It is true that they are much more alike than the degree-awarding institutions of the U.S.A., and since they are all largely financed by the state, in proportion principally to their sizes, they are not in competition for general operating funds as American institutions may be.¹⁸ It is implicitly assumed that each university either does, or could, excel in some field or other, and there has never been an attempt at arranging the institutions in a hierarchy of quality as has been done in the U.S.A. Examining the recognition of contributions to

developed field as chemistry have secured a measure of autonomy, such that they possess their own societies, journals, and so on. Consequently, many of the more formal symbols of recognition (medals, editorial board and Council service, etc.) must be regarded as within the autonomous gift of the group of scientists working within such specialties, even though these rewards may be less prestigious than those more widely available. One may therefore suggest that the greater the differentiation which has occurred within the discipline, the greater the relative delegation of power to the sub-discipline(s).

¹⁷ Sponsorship here may be seen as a consequence of the likelihood that in a major institution the acolyte may make the acquaintance of members of the scientific elite who control the allocation of many of the rewards of science.

¹⁸ To be sure both research funds and graduate studentships (fellowships) are in short supply, but Ben-David has pointed out that the academic ethos of European universities lacks the competitiveness characteristic of American institutions (Ben-David, 1968). They are not, for example, so competitive for staff. See also Gaston (1971).

science by high energy physicists in the UK, Gaston (1969: Chapter 7) found no evidence for unmerited recognition deriving either from being attached to, or having been educated at, one or another university. But Halsey and Trow's (1971) findings in their study of The British Academics belie this conclusion. Their work led them to suggest that a substantial prestige attached to the Universities of Oxford and Cambridge, in so far as these two ancient universities have not only provided the models for many newer foundations, but continue to exert a profound attraction for British academics. Oxford and Cambridge, they say, "serve as status and intellectual models" (Halsey and Trow, 1971:233). Moreover their faculties are distinguished from colleagues elsewhere by higher background statuses (Halsey and Trow 1971:216), and by their predominance in the Royal Society (33% of whose members are attached to these two universities) and in its equivalent in the humanities, the British Academy (63% of whose members are attached to Oxford and Cambridge). If Halsey and Trow have interpreted their findings aright, the generality of Gaston's conclusion—that affiliation had no effect on the allocation of rewards in science—seemed doubtful. We thought it worth look-

ing to see if rewards (which include election to the Royal Society) accrued preferentially to chemists attached to these two universities.

In Table 8 we show how the recognition received by chemists whose work is regarded as of equivalent quality is dependent upon their institutional affiliation. Among those whose work is of relatively low quality (the majority, on the basis of our rather demanding criterion of quality) 54% of the Oxford and Cambridge chemists have received substantial recognition, compared to 17% of London University chemists, and only 12% of others. Among those of medium quality a similar differential is apparent, although it is less strong. Moreover there is slight evidence that those attached to the Universities (or University Colleges) of Scotland, Wales and Northern Ireland may be at something of a disadvantage in the allocation of recognition. At the highest quality level it is not certain that affiliation significantly affects recognition, although it must be borne in mind that the numbers here are rather small.

The quality of a scientist's work is the most important determinant of the recognition he receives from the scientific community: a scientist doing really significant

Table 8. Recognition Received as a Function of Institutional Affiliation Holding Quality of Output Constant

Recognition Score	Quality Score											
	Zero				Low (1-3)				High (>3)			
	Affiliation*				Affiliation*				Affiliation*			
	Ox-bridge	London	S/W/I	Other	Ox-bridge	London	S/W/I	Other	Ox-bridge	London	S/W/I	Other
Zero	20	33	52	47	0	0	14	9	0	17	0	9
Low (1-4)	26	50	36	41	28	31	38	44	15	0	11	24
High (>4)	54	17	12	12	72	69	48	47	85	83	89	67
Total	100 (n=30)	100 (n=64)	100 (n=162)	100 (n=409)	100 (n=11)	100 (n=16)	100 (n=21)	100 (n=68)	100 (n=13)	100 (n=12)	100 (n=9)	100 (n=46)

* The division of affiliations is Oxford and Cambridge Universities; London University; Scottish, Welsh, and Northern Irish Universities; all other UK universities.

research at even the least prestigious university is more likely to be highly rewarded than one doing routine work even at Oxford or Cambridge. Nevertheless, affiliation does appear significantly to affect recognition: chemists attached to the Universities of Oxford or Cambridge are more likely to be highly rewarded than others, doing work of similar quality, working elsewhere. However the benefits of working at one or other of these ancient institutions appears to be rather slight for those whose research is of the highest quality. It is upon those whose work is not of this extremely high calibre that the prestige attaching to Oxford and Cambridge affiliation confers the greatest advantage. It might be hypothesized that, at least so far as foreign visitors are concerned, any Oxbridge bias reflects the attractiveness of the locale rather than unmerited recognition of the scientists' work. However, given that the maximum score for recognition obtainable on this basis was three, and that the factor makes an average contribution of 15% to recognition scores, the differences between Oxbridge and other universities (at zero quality score) cannot be explained away, even if the hypothesis is wholly correct. It could also be argued that chemists in these prestigious institutions are likely to be seen as pacemakers to an unmerited degree, and that our findings may for this reason be spurious. However, the effect of making a hypothetical downwards adjustment in the 'quality' scores of Oxford and Cambridge chemists would be to magnify the effect demonstrated in Table 8.

We ought finally to deal with the objection that our results are a consequence of systematic differences in research interests between 'Oxbridge' chemists and those of other universities, for we had, after all, suggested that recognition is more easily available in some areas of work. In fact this is not so: the distribution of the interests of Oxford and Cambridge chemists is only slightly different from that of all our respondents.¹⁹

¹⁹ The distribution between the major areas of chemistry of the interests of our Oxford and Cam-

CONCLUSIONS

This paper reports on some of the results of a study of British academic chemists, and deals in particular with the reward system operating within the chemical community in Britain. Three indicators for characterizing the work of individual chemists were introduced: quantity of output, quality of output, and industrial involvement, and their interrelationships were examined. As in comparable studies of physicists in the U.S.A., the most prolific chemists were also, by and large, the most significant (that is, they had produced the work of highest quality). However, we found that those who in their work were most significantly oriented towards industrial problems did not produce many scientific papers, nor did they tend to be highly rated by their peers for the quality of their work. We suggested that this may imply that a minority of the chemical community have a principal orientation to the industrial system—its needs, and the rewards which it can offer—rather than to the scientific community.²⁰ So far as the allocation of professional rewards was concerned, industrial orientation counted for little—as indeed was to have been expected assuming normative behaviour. However, both quantity and quality of research output are highly correlated with recognition received, quality being somewhat the more important. The differentiation which has occurred within chemistry has led to the co-existence of numerous distinct sub-disciplines differing not only in their size (popularity), and in the average numbers of papers pub-

bridge respondents (which may be compared with the overall figures given in Table 7) were

Sub-discipline	% Distribution of Interests
Theoretical chemistry	5.3
Physical chemistry	35.0
Organic chemistry	22.8
Physical-organic chemistry	7.0
Inorganic chemistry	8.8
Physical-inorganic chemistry	12.2
Analytical chemistry
Other	8.8

²⁰ We have it on good authority that there is at least one professorial head of a large department of chemistry who discourages foreign visitors because of the substantial industrial stake in his work.

lished by individuals working within them, but also in their autonomy. Since the perquisites of relative autonomy, such as a journal, a learned society, lead to a greater availability of rewards, the sub-disciplines may differ in the ease with which recognition may be obtained. Factors of this kind may obscure any relationship between research production and reward at the disciplinary level. We have demonstrated that this relationship is determined in part also by institutional affiliation, such that the rewards received by an individual chemist for a given contribution will depend on his place of work. In particular, chemists from the Universities of Oxford and Cambridge appear to be favoured in this. Our findings thus diverge from those of Gaston, who found no evidence for ascriptive allocation of rewards in high energy physics in Britain. They are, however, consistent with Crane's account of the institutional advantages to be had in the U.S.A., and with Halsey and Trow's discussion of the peculiar prestige attaching to Oxford and Cambridge within the British university system. They may to some extent account for the high concentration of Fellows of the Royal Society in those institutions!

REFERENCES

- Bayer, A. E. and J. Folger
1966 "Some correlates of a citation measure of productivity in science." *Sociology of Education* 39 (Fall):381-90.
- Ben-David, Joseph
1968 *Fundamental Research and the Universities: Some Comments on International Differences*. Paris: OECD.
- Blume, Stuart S. and Ruth Sinclair
Forth- *Research Environment and Performance coming in British University Chemistry*. Science Policy Study Number 6. London: HMSO.
- Boffey, Philip M.
1970 "HEW blacklists: new security procedures officially adopted." *Science* 170 (9 October):142-4.
- Cartter, Allan M.
1966 *An Assessment of Quality in Graduate Education*. Washington, D.C.: American Council on Education.
- Clark, Kenneth E.
1957 *America's Psychologists—A Survey of a Growing Profession*. Washington, D.C.: American Psychological Association.
- Cole, Stephen and Jonathan R. Cole
1967 "Scientific output and recognition: a study in the operation of the reward system of science." *American Sociological Review* 32 (June):377-90.
- Crane, Diana
1965 "Scientists at major and minor universities: a study of productivity and recognition." *American Sociological Review* 30 (October):699-714.
- Gaston, Jerry Collins
1969 *Big Science in Britain: A Sociological Study of the High Energy Physics Community*. New Haven: Ph.D. thesis, Yale University.
1970 "The reward system in British science." *American Sociological Review* 35 (August):718-32.
1971 "Secretiveness and competition for priority of discovery in physics." *Minerva* IX (October):472-92.
- Hagstrom, W. O.
1965 *The Scientific Community*. New York: Basic Books.
1967 *Competition and Teamwork in Science*. Madison, Wisconsin. Department of Sociology, University of Wisconsin, Mimeographed.
1971 "Inputs, outputs, and the prestige of university science departments." *Sociology of Education* 44 (Fall):375-97.
- Halsey, A. H. and M. Trow
1971 *The British Academics*. London: Faber and Faber.
- Hargens, L. L. and W. O. Hagstrom
1967 "Sponsored and contest mobility of American academic scientists." *Sociology of Education* 40 (Winter):24-38.
- Kuhn, T. S.
1962 *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.
- Merton, Robert K.
1957 "Priorities in scientific discovery: a chapter in the sociology of science." *American Sociological Review* 22 (December):635-59.
- Organization for Economic Co-operation and Development (OECD)
1972 *The Research System*. Paris: OECD.
- Price, D. J. de Solla
1963 *Little Science, Big Science*. New York: Columbia University Press.
1970 "Citation measures of hard science, soft science, technology, and non-science." Pp. 3-22 in Carnot E. Nelson and Donald K. Pollock (eds.), *Communication Among Scientists and Engineers*. Lexington, Massachusetts: D. C. Heath and Company.
- Storer, N. W.
1966 *The Social System of Science*. New York: Holt, Rinehart, and Winston.
- Turner, R. H.
1960 "Modes of ascent through education: sponsored and contest mobility." *American Sociological Review* 25 (October):855-67.
- Weiss, Robert S.
1968 *Statistics in Social Research*. New York: Wiley.

Zuckerman, Harriet

1967 "Nobel laureates in science: patterns of productivity, collaboration, and authorship." *American Sociological Review* 32 (June):391-403.

Zuckerman, Harriet and Robert K. Merton

1971 "Patterns of evaluation in science: institutionalization, structure and functions of the referee system." *Minerva* 9 (January): 66-100.

A RE-EXAMINATION OF HERNES' MODEL ON THE PROCESS OF ENTRY INTO FIRST MARRIAGE FOR UNITED STATES WOMEN, COHORTS 1891-1945 *

DONALD W. HASTINGS AND J. GREGORY ROBINSON

*University of Tennessee**American Sociological Review* 1973, Vol. 38 (February):138-142

Hernes' mathematical model for the process of entry into first marriage is applied to U.S. women, cohorts 1891-1945. Selected cohorts with incomplete data proportions on age-specific first marriage were estimated following a modification of a technique suggested by Ryder. Hernes' model was found applicable across successive cohorts and estimated parameters of his model depicted historical shifts in the nuptial tempo.

INTRODUCTION

HERNES (1972) presented a mathematical model for describing the process of entry into first marriage for members of a birth cohort over time. To further test the applicability of Hernes' model, we analyzed cumulative percentages on age-specific first marriages for all United States women for single birth cohorts from 1891-1945. Due to the variability in differences between observed and calculated values obtained in this analysis, we analyzed average cumulative percentages on age-specific first marriages for aggregated cohorts, i.e. 1891-1895, 1896-1900, etc. We were interested in two questions: (1) to what extent is Hernes' procedure generalizable across successive cohorts? and (2) does this procedure accurately depict historical shifts in the nuptial tempo? This paper reports the results of this procedure.

METHODS

Hernes limited his analysis to selected sub-groups and birth cohorts for which data were available. To go beyond his analysis we estimated age-specific first marriage numbers for those cohorts with in-

complete data, following a procedure outlined by Ryder (1970) but with some modification. The following steps were taken.

(1) Information was obtained on number of women ever married by age at first marriage for single years of age, i.e. fourteen to thirty-four, and for age groups, i.e. thirty-five to thirty-nine, forty to forty-four, and forty-five to fifty-four from the U.S. census of population (U.S. Bureau of Census, 1966). Numbers also were gathered on total female population by single years of age, for birth cohorts 1891-1945 (U.S. Bureau of Census, 1961). In 1960 these cohorts were between the ages of fifteen and sixty-nine.

(2) We departed from Ryder (1970, Appendix II, pp. 119-20) in extending data beyond 1960. For the years 1961 through 1968, data on numbers of women ever married by age of first marriage, for single years, were taken from the U.S. vital statistics reports for those years in which data were incomplete in step (1) (U.S. National Vital Statistics Division, 1961-1968). These data also were presented for single years of age and for age groups as noted in step (1) above. Since these numbers from the vital statistics reports were based on information from marriage registration states only, it was necessary to inflate them for comparability with figures reported in the

* We are indebted to Annie Aitken, Suzanne B. Kurth and Walter W. Robinson for comments and criticisms on this paper. As usual we bear sole responsibility for the content of the paper.

1960 Census. To accomplish this task we calculated a proportion of number of first marriages by age in marriage registration areas in 1960 (U.S. National Vital Statistics Division) to number of first marriages by age for the United States in 1960 Census and used this proportion to inflate the vital statistics annual figures 1961-1968. This procedure provided information complete through 1968.

(3) To convert these data for number of women ever married, by age at first marriage reported in age groups into single years in each birth cohort, we calculated the proportions first married at each age, for ages thirty through thirty-four, and applied these proportions to totals reported for the age groups thirty-five to thirty-nine and forty to forty-four. To break out data for the age group forty-five to fifty-four into single years in each birth cohort, we calculated the average proportion of women ever married by age of first marriage for each age group forty-five to forty-nine and fifty to fifty-four, as reported in vital statistics reports (U.S. National Vital Statistics Division, 1956-1959). The average proportion for the forty-five to forty-nine age group was applied to the forty-five to fifty-four group to obtain an estimate of the number of marriages occurring to women forty-four to forty-nine years of age in each birth cohort. To convert these estimates for ages forty-five to forty-nine into single years of experience, the same proportioning technique was used as described for age groups thirty-five to thirty-nine and forty to forty-four.

(4) To project marriage data beyond 1968 two calculations were made. First, we calculated the ratio of number of women first married by age for single years, to the total female population by single years of age in each of the last three cohorts for which data were available for a given age. Then we obtained the average ratio for these last three years for which data were available and applied the average ratio as a constant to total female population in those cohorts for which data were missing.

These procedures yield a set of data on number of women ever married, by age at first marriage for birth cohorts 1891-1945 in single years. Conversion of these numbers

to rates and percentages for subsequent analysis was straightforward. Our procedure varies from that of Ryder's due to the different assumptions used in projecting marital experiences for cohorts with incomplete data. Nonetheless, this set of assumptions provide a good approximation for depicting the tempo of nuptiality across cohorts.

RESULTS

An examination of column 3 in Table 1 reveals that in general this mathematical model fairly accurately depicts the overall process by which cohort members enter into first marriage. However, in each cohort differences between observed and calculated cumulative percentages for age-specific first marriages vary. Cohorts 1916-1920 and 1921-25 are the two which exhibit the smallest differences between observed and calculated values. Cohort 1921-1925 most closely coincides with Hernes' cohort 1920-24. The fact that Hernes selected the cohort with the smallest differences was perhaps fortuitous.

In cohorts 1926-30, 1931-35 and 1936-40 the model underestimates the proportionate entry among cohort members into first marriage between the ages nineteen and twenty-six. (Since data for the cohorts 1931-1945 are derived from estimation procedures, figures presented in Table 1, ages thirty-three and over for the cohort 1931-35, twenty-eight and over for 1936-40 and twenty-three and over for 1941-45, should be interpreted cautiously. They are reported here, however, since they are used in estimating the parameters V , A , and b .) In part, this underestimation may be an artifact of estimation procedures used to generate age-specific first marriage data for cohorts with incomplete data. More than likely, the underestimation can be attributed to the fact that mean age at marriage has declined steadily in each successive cohort until recently when mean age at marriage has started to rise. (See Table 2.) Further, in each successive cohort, increased proportions of cohort members who married tended to do so at earlier ages (see column 1, Table 1). Ryder, (1970, p. 109) after examining the skew and kurtosis of "the nuptiality-age function" makes the

Table 1. Cumulative First Marriages Total Women in the United States for Birth Cohorts 1891-1945: Observed and Calculated Percentages*

Year	1891-1895			1896-1900			1901-1905			1906-1910		
	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)
15	1.14	1.84	-0.70	1.27	1.98	-0.71	1.30	3.02	-1.72	1.57	3.39	-1.82
17	7.34	7.06	0.28	7.84	7.74	0.10	8.44	9.29	-0.85	9.10	9.86	-0.76
19	20.37	18.43	1.94	21.55	20.16	1.39	23.33	20.92	2.41	24.10	21.52	2.58
21	36.81	34.51	2.30	38.72	37.16	1.56	40.48	36.01	4.47	41.13	36.56	4.57
23	51.37	50.46	0.91	54.47	53.28	1.19	54.87	50.66	4.21	55.17	51.22	3.95
25	62.59	62.96	-0.37	66.50	65.45	1.05	65.47	62.36	3.11	65.16	63.04	2.12
27	71.23	71.69	-0.46	74.35	73.70	0.65	72.77	70.80	1.97	72.33	71.63	0.70
29	77.42	77.53	-0.11	79.59	79.14	0.45	77.52	76.66	0.86	77.82	77.61	0.21
31	81.59	81.45	0.14	82.95	82.73	0.22	80.71	80.69	0.02	81.78	81.74	0.04
33	84.22	84.12	0.10	85.10	85.16	-0.06	83.11	83.51	-0.40	84.50	84.63	-0.13
35	86.02	85.97	0.05	86.51	86.84	-0.33	85.03	85.51	-0.48	86.50	86.68	-0.18
37	87.31	87.28	0.03	87.74	88.02	-0.28	86.48	86.96	-0.48	88.06	88.17	-0.11
39	88.16	88.23	-0.07	88.52	88.87	-0.35	87.61	88.02	-0.41	89.20	89.26	-0.06
41	88.84	88.92	-0.08	89.24	89.49	-0.25	88.55	88.81	-0.26	90.06	90.08	-0.02
43	89.36	89.44	-0.08	89.83	89.95	-0.12	89.29	89.40	-0.11	90.70	90.70	0.00
45	89.71	89.82	-0.11	90.22	90.29	-0.07	89.89	89.85	0.04	91.17	91.17	0.00
47	90.19	90.11	0.08	90.73	90.54	0.19	90.37	90.20	0.17	91.57	91.54	0.03
49	90.50	90.33	0.17	91.06	90.74	0.32	90.75	90.47	0.28	91.86	91.83	0.03
Year	1911-1915			1916-1920			1921-1925			1926-1930		
	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)
15	1.43	2.43	-1.00	1.34	1.76	-0.42	1.21	1.48	-0.27	1.33	2.00	-0.67
17	8.78	8.26	0.52	8.05	7.52	0.53	8.05	7.57	0.48	8.83	9.64	-0.81
19	22.78	20.20	2.58	22.38	21.00	1.38	14.56	23.23	1.33	28.08	27.64	0.44
21	38.61	36.78	1.83	40.44	40.18	0.26	45.41	45.33	0.08	52.80	50.50	2.30
23	53.82	53.18	0.64	58.08	58.10	-0.02	63.80	64.34	-0.54	71.15	68.49	2.66
25	66.15	66.05	0.10	71.00	70.98	0.02	76.48	76.67	-0.19	81.18	79.56	1.62
27	75.05	75.00	0.05	78.95	79.22	-0.27	84.14	83.91	0.23	86.70	85.92	0.78
29	80.98	80.95	0.03	84.12	84.37	-0.25	88.27	88.17	0.10	90.00	89.61	0.39
31	84.79	84.90	-0.11	87.69	87.63	0.06	90.80	90.75	0.05	92.00	91.84	0.16
33	87.49	87.57	-0.08	89.85	89.75	0.10	92.36	92.38	-0.02	93.17	93.25	-0.08
35	89.46	89.41	0.05	91.22	91.19	0.03	93.42	93.46	-0.04	93.87	94.18	-0.31
37	90.77	90.71	0.06	92.24	92.18	0.06	94.12	94.19	-0.07	94.61	94.81	-0.20
39	91.72	91.66	0.06	91.84	92.88	-0.04	94.57	94.70	-0.13	95.05	95.25	-0.20
41	92.41	92.35	0.06	93.35	93.39	-0.04	94.96	9.506	-0.10	95.42	95.57	-0.15
43	92.89	92.87	0.02	93.74	93.77	-0.03	95.25	95.33	-0.08	95.72	95.80	-0.08
45	93.24	93.26	-0.02	93.99	94.05	-0.06	95.45	95.52	-0.07	95.90	95.97	-0.07
47	93.53	93.56	-0.03	94.30	94.26	0.04	95.75	95.67	0.08	96.21	96.10	0.11
49	93.74	93.80	-0.06	94.49	94.42	0.07	95.94	95.77	0.17	96.39	96.19	0.20
Year	1931-1935			1936-1940			1941-1945					
	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)	Obs. (1)	Cal. (2)	Diff. (3)
15	1.38	2.91	-1.53	1.37	3.62	-2.25	.98	1.77	-0.79			
17	10.76	12.15	-1.39	10.81	13.41	-2.60	8.30	9.23	-0.93			
19	33.01	31.38	1.63	34.63	32.44	2.19	28.94	27.36	1.58			
21	57.24	53.81	3.43	59.73	54.27	5.46	52.74	50.35	2.39			
23	74.54	70.77	3.77	76.80	70.97	5.83	71.82	68.21	3.61			
25	83.87	81.09	2.78	84.81	81.33	3.48	81.64	79.09	2.55			
27	88.65	87.03	1.62	88.94	87.37	1.57	86.70	85.31	1.39			
29	91.23	90.49	0.74	91.49	90.91	0.58	89.52	88.93	0.59			
31	92.63	92.60	0.03	93.12	93.07	0.05	91.16	91.12	0.04			
33	93.68	93.94	-0.26	94.24	94.45	-0.21	92.28	92.51	-0.23			
35	94.48	94.83	-0.35	95.05	95.35	-0.30	93.10	93.43	-0.33			
37	95.11	95.43	-0.32	95.71	95.09	-0.27	93.75	94.05	-0.30			
39	95.61	95.86	-0.25	96.20	96.42	-0.22	94.24	94.49	-0.25			
41	95.99	96.16	-0.17	96.58	96.74	-0.16	94.62	94.79	-0.17			
43	96.29	96.39	-0.10	96.88	96.97	-0.09	94.92	95.02	-0.10			
45	96.52	96.56	-0.04	97.10	97.14	-0.04	95.14	95.18	-0.04			
47	96.78	96.68	0.10	97.37	97.28	0.09	95.41	95.30	0.11			
49	96.99	96.77	0.22	97.57	97.38	0.19	95.61	95.39	0.22			

* For more detailed information contact the authors, i.e., even numbers.

Table 2. Mean Age of First Marriage for All Women United States for Birth Cohorts, 1891-1945

Cohorts	\bar{X}	Mean Age of Nuptiality ^(a)
1891-1895	23.42	23.33
1896-1900	23.11	23.10
1901-1905	23.25	23.21
1906-1910	23.22	23.18
1911-1915	23.19	23.16
1916-1920	22.76	22.73
1921-1925	22.20	22.10
1926-1930	21.63	21.56
1931-1935	21.27+	21.35
1936-1940	21.16+	21.32
1941-1945	21.58+	21.55

Source: (a) Norman B. Ryder, "The Emergence of a Modern Fertility Pattern: United States, 1917-66," in *Fertility and Family Planning: A World View*, ed. by S. J. Behrman, Leslie Corsa, Jr., and Ronald Freedman (Ann Arbor: University of Michigan Press, 1970), Table 6, p. 109.

same point. He found that over time for successive cohorts there has been a shift toward positive skew and increasing leptokurtic distribution in nuptial patterns.

An examination of Table 3 reveals the historical shifts in the nuptial tempo. The estimated asymptotic values (V) increase in each successive cohort from 1891-95 to 1936-40, then decline for the cohort 1941-45. These values parallel the actual increases over time in the percentages of females ever married. Although the more recent cohort estimates heavily depend on data derived from estimation procedures, it is reasonable to assume that a possible decline might occur in the near future in the percentages of females ever married. Such an assumption is warranted in light of the increased percentages of women completing a college education, and the inverse relationship between level of education attainment and marriageability.

The estimated values of A, initial capacity for marriage, tend to increase for

the cohorts 1901-05 to 1926-30. Increases in these values tend to parallel the shift toward a younger mean age at first marriage. Values of A for the cohort 1931-35 and after, suggest a decline in marriage capacity, yet are difficult to assess due to the nature of the data base.

The estimated values of b, the constant of deterioration, show only small differences across successive cohorts. Nonetheless, these values tend to reflect the historical shifts in the tempo with which females marry. For instance, the cohorts of 1901-05 and 1906-10 married more rapidly than the cohorts of 1911-15 through 1921-25. The earlier cohorts were reaching marriageable ages during the years of post-WWI prosperity and changing sexual norms, while the later cohorts were entering the marriageable ages during the depression years and WWII era. Note also that the cohorts of 1926-30 through 1936-40 married more rapidly than the cohorts 1911-15 and

Table 3. Estimated Parameters for Entry into First Marriage, Total Women in United States for Birth Cohorts, 1891-1945*

Cohort	A	b	V
1891-1895	.79	.882	.91
1896-1900	.81	.879	.91
1901-1905	.67	.892	.91
1906-1910	.63	.899	.93
1911-1915	.71	.897	.95
1916-1920	.84	.885	.95
1921-1925	.97	.878	.96
1926-1930	.95	.878	.96
1931-1935	.86)	.884)	.97)**
1936-1940	.79)	.894)	.98)
1941-1945	.99)	.870)	.96)

* A = marriageability; b = constant of deterioration; V = asymptotic value for percentage ever married.

** Values should be interpreted with caution as they are based on data derived mainly from estimation procedures.

1906-10. Again these females were reaching ages in which they were most likely to marry during the nuptial boom of the late forties and fifties and during the years of economic growth—the sixties.

CONCLUSION

These results suggest that Hernes' model is applicable across successive cohorts and that estimated parameters can be used to study historical trends in nuptial patterns. Further research might explore the possibility of using Hernes' model to generate estimates for women's nuptial experiences where only partial data are available on age-specific first marriages for selected ages. Such a procedure would benefit sociologists viewing marriage patterns in areas where records are incomplete and demographers attempting to account for the impact of tempo of nuptiality on other demographic variables.

REFERENCES

- Hernes, Gudmund
1972 "The process of entry into first marriage." *American Sociological Review* 37 (April): 173-82.
- Ryder, Norman B.
1970 "The emergence of a modern fertility pattern: United States, 1917-66." Pp. 99-123 in S. J. Behrman, Leslie Corsa, Jr., and Ronald Freedman (eds.) *Fertility and Family Planning*. Ann Arbor: University of Michigan.
- U.S. Bureau of the Census
1961 U.S. Census of Population: 1960 General Population Characteristics. Final Report PC(1)-1D. Table 156, p. 354.
1966 U.S. Census of Population: 1960 Subject Reports Age at First Marriage, Final Report PC(2)-4D. Table 2, Pp. 28-9.
- U.S. National Vital Statistics Division
1958 Vital Statistics of the U.S. 1956. Vol. I, Marriages & Divorces. Table 4, Pp. 52-3.
1959 Vital Statistics of the U.S. 1957. Vol. I, Marriages & Divorces. Table 6, Pp. 66-7.
1960 Vital Statistics of the U.S. 1958. Vol. I, Marriages & Divorces. Table 6, Pp. 78-9.
(n.d.) Vital Statistics of the U.S. 1959. Vol. I, Marriages & Divorces. Table 6, Pp. 1-60.
1964 Vital Statistics of the U.S. 1960. Vol. III, Marriages & Divorces. Table 2-15, p. 2-15.
1965 Vital Statistics of the U.S. 1961. Vol. III, Marriages & Divorces. Table 2-13, p. 2-11.
1965 Vital Statistics of the U.S. 1962. Vol. III, Marriages & Divorces. Table 1-24, p. 1-20.
1967 Vital Statistics of the U.S. 1963. Vol. III, Marriages & Divorces. Table 1-32, p. 1-36.
1968 Vital Statistics of the U.S. 1964. Vol. III, Marriages & Divorces. Table 1-33, p. 1-36.
1968 Vital Statistics of the U.S. 1965. Vol. III, Marriages & Divorces. Table 1-33, p. 1-35.
1969 Vital Statistics of the U.S. 1966. Vol. III, Marriages & Divorces. Table 1-31, p. 1-28.
1970 Vital Statistics of the U.S. 1967. Vol. III, Marriages & Divorces. Table 1-31, p. 1-29.
1971 Vital Statistics of the U.S. 1968. Vol. III, Marriages & Divorces. Table 1-31, p. 1-29.

POLITICAL OVERCONFORMITY BY UPWARDLY MOBILE AMERICAN MEN

ANDREW HOPKINS

University of Connecticut

American Sociological Review 1973, Vol. 38 (February):143-147

It is popularly believed that upwardly mobile American men are more conservative than those born into the middle class. This paper reviews the evidence for the belief and concludes that, if anything, upwardly mobile American men are less conservative than their stable middle class compatriots.

SURVEYS in many European countries and in the United States show that upward social mobility (specifically, intergenerational movement from manual to non-manual occupations) is universally accompanied by an increase in political conservatism (Butler and Stokes, 1969:98; Lipset and Zetterberg, 1964:457; Lipset and Zetterberg, 1959:67; Lopreato and Hazelrigg, 1972:451). At the same time the data indicate that in Europe the upwardly mobile remain, statistically speaking, significantly¹ less conservative than those born into the middle class, adopting a political stance somewhere between their classes of origin and destination.

By contrast, in the United States, upwardly mobile sons of workers seem little influenced by their class of origin and exhibit voting patterns very similar to those of the stable middle class. In fact, according to Lipset and Zetterberg, "the American data . . . indicate that successfully upwardly mobile sons of workers are even more conservative in party choice than those middle class individuals whose fathers held occupations comparable to their own" (1964:456). Tumin echoes this claim in a current textbook, asserting that "in America, persons who move up into the middle class are more conservative than those born into it" (1967:94).²

However this proposition is not beyond

¹ Throughout this study, statistical tests are based on the .05 level of significance with direction predicted.

² The context of this assertion indicates that it is intended to apply to males only. There is in fact convincing evidence that upwardly mobile American women adopt a political stance intermediate between stable middle and working class women (Thompson, 1971:229).

doubt. On the basis of a brief and somewhat incomplete review of the literature, Thompson (1971) has concluded recently that the evidence is "contradictory," and his own data cast further doubt on the thesis that upwardly mobile American men "overconform" to the political norms of the middle class.

Most recently though, discounting Thompson's misgivings, Lopreato and Hazelrigg have reiterated the established position in the following terms: "(upwardly mobile) Europeans remain more leftist than their class of destination while their North American counterparts become even more conservative than their new class peers" (1972:452).² Indeed Lopreato goes so far as to offer an explanation of this supposed political overconformity by upwardly mobile Americans. His theory runs as follows: in America success is the supreme goal to which all can and must aspire; as a result, upward mobility gives the achiever enormous satisfaction and a sense of relief at having "made it"; this is likely to give rise to a "cult of gratitude," an attitude of deep-seated appreciation towards the social order for making the new-found pleasures possible; "... such gratitude is then expressed through an 'over conformity' to the prescribed behavior of the middle class, specifically, by voting for (the Republican Party)" (1967:592).

But such theorizing is premature. The data on which the political overconformity thesis currently rests are not just contradictory as Thompson suggests but in fact require the rejection of the thesis. To demonstrate this, I shall devote the remainder of this paper to a systematic review of the evidence.³

³ Although, as will be shown here, upwardly

The Lipset-Zetterberg Data

The principal evidence cited by the proponents of the overconformity thesis was originally presented by Lipset and Zetterberg (1959:67). It is re-presented here in the first panel of the table. The trends in these data are certainly suggestive of political overconformity, but in fact the data provide us with no reason to reject the null hypothesis that in America the upwardly mobile are politically similar to the stable middle class. If chi-square is calculated for these figures we get values of .24 for the 1948 election and 1.18 for the 1952 election, indicating that in both cases the difference between the upwardly mobile and the stable middle class is far from being statistically significant.

The M.I.T. Study

A second piece of evidence cited by the proponents of the overconformity thesis is a study by the M.I.T. Center for International Studies in 1955, involving 1,000 randomly selected American business executives. According to Lipset and Zetterberg, "these data show that only 5 percent of the children of manual workers are Democrats as compared with 10 percent Democrat among the executive sons of middle and upper class fathers" (1964:460). Unfortunately, since these data are apparently unpublished, it has not been possible to check the details. However, Lipset and Zetterberg admit elsewhere that "the differences are too small to be significant" (1959:67).

West's Study of College Graduates

In their recent book, Lopreato and Hazelrigg cite the work of Patricia West (1954:465-80) as further evidence of the overconformity of upwardly mobile Americans. As part of her study, West looked at American

college graduates who, at the time of her survey in 1947, had made progress in their chosen careers and were financially well-off (earning more than \$7,500 a year). She singled out two groups of such men: those who had worked their way through college, earning more than half their college expenses, and those who had earned none of their college expenses, having been supported by their parents. These she termed the self-made and the privileged men respectively. Clearly the former, starting from a relatively impoverished situation, may be regarded as upwardly mobile; while the latter are, in some sense, the stable middle class.

In her article West was concerned to see whether, for college graduates, the economic status they attain in later life is a better indicator of their political attitudes than is their economic origin. She therefore compared her two groups of men on the politically relevant question of whether or not they favored *laissez-faire* government. Her results are presented in the second panel of the table. It should be noted that the difference suggested by the figures is statistically insignificant ($\chi^2 = .37$). Therefore, given West's concern, we might have expected her to conclude from these data that self-made men were "just as opposed to government planning as the originally privileged group." Instead she actually wrote that they were "if anything, *more* opposed to government planning than the originally privileged group" (1954:479, emphasis in original). Whether or not the former were really *more* opposed than the latter is irrelevant to her concern, and she can be forgiven the statistical license she takes in making this assertion.

However, Lopreato and Hazelrigg quote this observation by West as evidence of the political overconformity of upwardly mobile Americans (1972:452). In doing so they transfer the statement from a context in which the statistical insignificance is unimportant to a context in which it is critical. West's statement is simply unacceptable as evidence of political overconformity.

Interestingly enough, as Lopreato and Hazelrigg concede, there is in West's article some good evidence that upwardly mobile Americans are *not* as conservative as the stable middle class. She presents data on the party affiliations of self-made and privileged

mobile American men are not more conservative than their stable middle class compatriots, it is quite possible that they are more conservative than would be expected on the basis of an additive model of the effects of class of origin and class of destination on political preference. If analysis of the data revealed a pronounced departure from additivity; that is, if it revealed a significant interaction effect, Lopreato's theorizing might then properly be invoked to account for this effect. (For an example of the kind of analysis referred to, see Jackman, 1972.)

Table 1. Political Orientation by Mobility in the United States¹

Panel No.		Upwardly Mobile	Stable Middle Class	Chi-Square	Significance ²
1	Left Voting by Urban Middle Class Men (Lipset and Zetterberg)				
	% Democrat in 1948	35 (72)	39 (83)	.24	N.S.
	% Democrat in 1952	22 (67)	30 (79)	1.18	N.S.
2	Attitude of Self-made and Privileged Men towards Laissez-Faire Gmt. (West)				
	% Favoring L.F.	73 (484)	71 (338)	.37	N.S.
3	Party Affiliations of Self-made and Privileged Men (West)				
	% Democrat	18	22	2.13	N.S.
	% Independent	40	29		
	% Republican	42	49	3.89	Sig.
	Total (Sample Total)	100 (469)	100 (326)		
4	Political Stance of Middle Class Men (Centers)				
	% Radical	14	11		
	% Intermediate	23	22		
	% Conservative	63	67	.46	N.S.
	Total (Sample Total)	100 (103)	100 (209)		
5	Party Preference of Cambridge Youth (Maccoby)				
	% Republican definitely	18	21		
	% Republican leaning	5	-		
	% Independent	9	28		
	% Democrat leaning	12	8		
	% Democrat definitely	56	43		
	Total (Sample Total)	100 (82)	100 (73)		
6	Party Preference of Cambridge Youth--Collapsed Data (Maccoby)				
	% Republican	23	21	.16	N.S.
	% Independent	9	28		
	% Democratic	68	51	4.99	Sig.
	Total (Sample Total)	100 (82)	100 (72)		
7	Presidential Choice of Cambridge Youth (Maccoby)				
	% Stevenson ³	52 (82)	63 (72)	1.58	N.S.
8	Left Voting by American Middle Class Men (Thompson)				
	% Democrat in 1948	37 (70)	40 (73)	.10	N.S.
	% Democrat in 1952	29 (77)	28 (92)	.00	N.S.
	% Democrat in 1958	45 (77)	51 (100)	.54	N.S.
	% Democrat in 1960	38 (87)	40 (108)	.07	N.S.
	% Democrat in 1964	71 (79)	53 (91)	5.86	Sig.
	% Democrat in 1966	54 (50)	49 (53)	.25	N.S.
	Aggregate % Democrat	45 (440)	43 (517)	.42	N.S.

¹Unless otherwise indicated, percentages are based on a two-party choice. Figures in brackets are sample totals. Sources are quoted in the text.

²All tests are based on the .05 level of significance with direction predicted.

³Stevenson was the Democratic candidate; the Republican was Eisenhower.

men, analysed by age. In the context of the overconformity thesis, West's age analysis is irrelevant; and so we may amalgamate her age groupings. Doing so yields the information presented in the third panel of the table. These data enable us to conclude that the privileged men are significantly more Republican than the self-made men ($\chi^2 = 3.89$). Thus, far from supporting the political overconformity thesis, West's data provide significant evidence that the upwardly mobile in America are *less* conservative than the stable middle class.⁴

The Work of Richard Centers

A further piece of relevant evidence comes from the work of Richard Centers (1949: 180). On the basis of a battery of questions, he divided respondents in a 1945-47 survey into radical, intermediate and conservative categories. By relating this to mobility, he obtained the results presented in the fourth panel of the table. Although these data reveal a trend to radicalism by the upwardly mobile, it is not statistically significant. (For conservatives, $\chi^2 = .46$.) Nevertheless, Centers' results are inconsistent with the overconformity thesis.

Maccoby's Study of Cambridge Youth

More evidence bearing on the issue is to be found in E. E. Maccoby's study of the political attitudes of youth in Cambridge, Massachusetts in 1952 (1954:23-39). Since these data are for both males and females and since Lipset, Zetterberg, Lopreato and others formulate their proposition for upwardly mobile American males only, Maccoby's findings are not strictly relevant. Nevertheless, Lipset and Zetterberg cite this study as evidence for their thesis; and so it must be considered here. According to Lipset and Zetterberg, "Maccoby found that upward mobile youth in Cambridge were more Republican than non-mobiles in the class to which the upward mobile moved" (1964: 460). The evidence actually presented by

Maccoby is contained in the fifth and seventh panels of the table.

Let us first examine panel 5 for evidence of the Lipset-Zetterberg claim. To facilitate the discussion let us collapse panel 5 to form panel 6. The first line of panel 6 suggests that, in apparent conformity with the claim made by Lipset and Zetterberg for Maccoby's findings, the upwardly mobile are more inclined than the stable middle class to prefer the Republicans. However the value of chi-square for the difference is a mere .16. In other words, if the Lipset-Zetterberg claim is based on these data, it is based on a difference which, statistically speaking, is totally insignificant.

If, on the other hand, their claim is made on the basis of panel 7 it is again ill-founded, since chi-square for that difference is only 1.58. Whichever the case the Lipset-Zetterberg interpretation of Maccoby's findings must be rejected.

It is worth observing that panel 7 can be interpreted in quite another way. Scrutinizing the third line of the panel enables us to make the following claim: Maccoby found that upward mobile youth in Cambridge were more *Democratic* than non-mobiles in the class to which the upward mobile moved.⁵ Furthermore, this difference is statistically significant, being supported by a chi-square of 4.99.

Thus the overall conclusion from Maccoby's study is that amongst Cambridge youth the upwardly mobile are significantly more Democratic and not significantly more Republican than the stable middle class. It seems that Lipset and Zetterberg have misinterpreted her results. If, as they intend, this study is to be treated as relevant to the issue of political overconformity by upwardly mobile American males, then Maccoby's data provide evidence against rather than for the thesis.

⁴ An interesting feature of these data is the disproportionate support which the upwardly mobile give to independent candidates at the expense of both Democrats and Republicans. This suggests that independent candidates offer the upwardly mobile a political 'halfway house.'

⁵ This claim is in striking contrast with that made by Lipset and Zetterberg. But because of the disproportionate tendency of the stable middle class to prefer independents, the two are not actually inconsistent. In principle both claims could be simultaneously true. It may be observed that the tendency for members of the stable middle class to prefer independents is at odds with the situation commented on in footnote 4. This however is not the place to dwell on the discrepancy.

Greenstein and Wolfinger

A further finding relating to the voting habits of upwardly mobile Americans comes from a study of suburbanites conducted by Greenstein and Wolfinger. They found in their sample that "suburbanites who were 'objectively' upwardly mobile (those with professional, executive, or white collar jobs whose fathers were manual workers) were more Democratic than were 'stable' suburbanites at the same occupational levels" (1958:479). Unfortunately the authors do not present their data, and so it has not been possible to check statistical significance. Nevertheless their conclusion is inconsistent with the claim that the upwardly mobile are political overconformists.

Thompson's National Survey Data

Kenneth Thompson has provided the most important data yet on the political stance of the upwardly mobile in America (1971: 223-35). Using a sample drawn by the Michigan Survey Research Center, he analysed national voting patterns for the six elections from 1948 to 1966. His results are presented in the eighth panel of the table. As can be seen, for three of the six elections upwardly mobile men were more conservative than stable middle class individuals; while for the other three they were less conservative. Five of these six differences are statistically insignificant, but in the case of the 1964 election the upwardly mobile were *significantly* more likely to vote Democratic than was the stable middle class. If we aggregate the data for the six elections, we find that the upwardly mobile were on the whole more likely to vote Democratic than were those born into the middle class, although the difference is not significant. Overall, therefore, Thompson's data constitute strong evidence against the overconformity thesis.

Political Overconformity: The Verdict

This review of the evidence indicates that upwardly mobile American men vote conservatively in approximately the same proportions as do stable members of the middle class. *Not one piece of statistically significant evidence supports the proposition that*

upwardly mobile sons of workers are more likely than old-time members of the middle class to vote Republican. On the other hand, not counting Maccoby's study, two items of statistically significant evidence (West and Thompson) have been found to support the proposition that upwardly mobile American men are *less* conservative than the stable middle class. If anything, therefore, the data support the view that the upwardly mobile "underconform" to the political norms of the middle class. However, that most of the findings yield no statistically significant differences one way or the other suggests rather persuasively that upwardly mobile American men are politically indistinguishable from their stable middle class compatriots.

REFERENCES

- Butler, D. and D. Stokes
1969 *Political Change in Britain*. London: Macmillan.
- Centers, R.
1949 *The Psychology of Social Classes*. Princeton: University Press.
- Greenstein, F. I. and R. E. Wolfinger
1958 "The suburbs and shifting party loyalties." *Public Opinion Quarterly* 22 (Winter): 473-82.
- Jackman, M. R.
1972 "The political orientation of the socially mobile in Italy: a re-examination." *American Sociological Review* 37 (April):213-22.
- Lipset, S. M. and H. Zetterberg
1959 "Social mobility in industrial societies." Pp. 11-75 in S. M. Lipset and R. Bendix, *Social Mobility in Industrial Society*. Berkeley: University Press.
- 1964 "A theory of social mobility." Pp. 437-62 in L. Coser and B. Rosenberg (eds.), *Sociological Theory*. New York: Macmillan.
- Lopreato, J.
1967 "Upward social mobility and political orientation." *American Sociological Review* 32 (August):586-92.
- Lopreato, J. and L. Hazelrigg
1972 *Class, Conflict and Mobility*. San Francisco: Chandler.
- Maccoby, E.
1954 "Youth and political change." *Public Opinion Quarterly*, 18 (Spring):23-39.
- Thompson, K. H.
1971 "Upward social mobility and political orientation: a re-evaluation of the evidence." *American Sociological Review* 36 (April):223-35.
- West, P. S.
1954 "Social mobility among college graduates." Pp. 465-80 in R. Bendix and S. M. Lipset (eds.), *Class, Status and Power* (1st ed.). London: Routledge and Kegan Paul.

ITEMS (*Continued*)

ter, the University of Texas at Austin. His present research interests include ecological analyses of ethnic and sexual differences in income and occupation, and the factorial ecologies of Southwestern cities and their ghettos.

■ **Robert E. Kennedy, Jr.** is Associate Professor of Sociology at the University of Minnesota, Minneapolis. His article continues his research interests in Irish society. A book on Irish social demography will be published by the University of California Press in early 1973. He is also the principal technical advisor for a Venezuelan project on the utilization of demographic materials for policy formulation.

■ **Melvin L. Kohn** is Chief of the Laboratory of Socio-environmental Studies of NIMH and **Carmi Schooler** is Chief of the Laboratory's Section on Personality and Environment. Kohn's research is primarily focused on interpreting the effects of social structure (in particular, class and occupational position) on values, orientation, and psychological functioning. He continues to do what he can to keep social science alive at NIMH and to secure a greater role for scientists in the formulation of Institute policy. Schooler's research interests include survey-based analyses of the psychological effects of occupations and the childhood antecedents of adult functioning, the experimental study of schizophrenia and cross-cultural studies of psychiatric symptomatology. He is also working on a cross-cultural study of women's attitudes towards their various roles.

■ **Daniel R. Goldman** is Assistant Professor of Sociology at Wayne State University. His current

research is focused on emerging professions, including a study of optometrists in the state of Michigan. **Stuart S. Blume** has worked principally in the fields of scientific and higher educational policy at the University of Sussex, for the British Government, for the OECD. He has recently completed a book on the political sociology of science, to be published by The Free Press. Current projects include an examination of the management of research by government, and the effects of higher education expansion upon university research in various countries. **Ruth Sinclair**, until recently on the staff of the Centre for the Utilization of Social Science Research at Loughborough University of Technology, is a co-editor of the recent volume *Social Science and Government: Policies and Problems* (Tavistock 1972).

■ **Donald W. Hastings** and **J. Gregory Robinson** are, respectively, Assistant Professor of Sociology and Graduate Research Assistant with the Center for Business and Economic Research at the University of Tennessee, Knoxville. Hastings is interested in fertility and birth planning. His current research focuses on social and psychological factors associated with voluntary sterilization among selected social and economic sub-groups in the Southeast. Robinson is interested in demographic factors associated with labor force participation.

■ **Andrew Hopkins**, a graduate student in sociology at the University of Connecticut, holds a B.Sc. in pure mathematics and an M.A. in sociology, both from the Australian National University in Canberra, Australia. His current research interests are in criminology and methods of data analysis.

J.F.S.



AMERICAN SOCIOLOGICAL REVIEW
Volume 38 **Number 2**

Whyte		Bureaucracy and Modernization	
atz et al	The Use of Mass Media	Williams	Community Power Concentration
andwerker	Technology and Household in Africa	Mason et al	Cohort Analysis of Archival Data
osen	Change, Migration and Family	Webster	Reductionism
humann ppi	The Study of Elites	Huber	The Bias of Emergent Theory

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit three copies, and retain a copy for your own files. Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—double-spaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated with reference. ["... have claimed that this is so (but see Jones, 1952:99 for a conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. ["... as many have noted (Johnson, 1942; Perry, 1947; Lindquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–66.

1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

BUREAUCRACY AND MODERNIZATION IN CHINA:
THE MAOIST CRITIQUE *

MARTIN KING WHYTE

The University of Michigan

American Sociological Review 1973, Vol. 38 (April):149-163

The Chinese critique of bureaucratic forms of organization is delineated, and the alternative Maoist organizational ideal is sketched. The adequacy of this Maoist alternative as part of a modernizing strategy is considered, both on logical and on (limited) empirical grounds. The Maoist conception seems to be neither a general solution to the organizational problems of developing societies nor totally inappropriate or utopian.

IN the West the term bureaucracy has an ambivalent heritage. Max Weber stressed that bureaucracy, in particular the rational bureaucracy of capitalist societies, could more efficiently coordinate the diverse activities of large numbers of individuals than organizational forms based on such things as kinship and personal loyalty. He foresaw that with growing trends toward rationality, mass democracy, and complex divisions of labor, the increasing dominance of the bureaucratic form of organization could not be avoided. At the same time Weber was very aware of the features of real bureaucracies (impersonality, red tape, etc.) which have given the term its negative connotation in common speech. Weber also predicted that future socialist societies would require an even higher degree of formal bureaucratization than capitalist societies (Weber, 1968:225). The evolution of Soviet society has given us no grounds for rejecting this prediction, and has in fact reinforced the conviction of Western social scientists that bureaucratization is an inevitable concomitant of economic development, whether socialist or capitalist.

Recent events in China, however, direct renewed attention to the relationship be-

tween bureaucratization and modernization. The Cultural Revolution (1966-1969) witnessed a broad attack on the growing bureaucratization of Chinese society in terms which sometimes parallel, but in other cases differ from, Western critiques of bureaucracy. The Cultural Revolution was portrayed in Chinese media as a struggle between two roads, one toward Soviet-style bureaucratic development, and one toward a new Maoist style of non-bureaucratic development. The Cultural Revolution debates have produced reverberations in the West, with some saying that the "Maoist model" is more suited to the needs and problems of today's developing societies than Western or Soviet-style development (Gurley, 1971; Andors, 1971); while others view Maoism as a romantic and irrational approach, incompatible with the demands of modernization (Lewis, 1968; Loewenthal, 1970).¹

The present article is an attempt to deal with some of the issues raised in these debates, while relating them to theoretical concerns of those studying organizations outside of the Chinese context. First it will be necessary to specify what the nature of the Maoist critique of bureaucracy is, and

* Michael Flynn, Gordon Bennett, Janet Salaff, and Gayl Ness all provided useful criticism of an earlier draft.

¹ Here the Maoist ideal for organizations is seen as part of a more general Maoist model for economic development, which is discussed most specifically by Gurley (1971).

what the features of the alternative Maoist organizational ideal are. How thorough is the Maoist critique of bureaucracy? How non- or anti-bureaucratic is the Maoist alternative? Our tactic will be to compare various features of the emerging Maoist organizational conception with traits from Western organizational models—many, but not all of them, stemming from the Weberian tradition. After we have clarified somewhat the nature of the Maoist organizational ideal, we will try to deal with the more difficult question of its appropriateness to the needs and problems of China and other developing societies. Throughout this article we will concentrate on bureaucracy and Maoism within organizations, rather than in national political and economic institutions.

The term bureaucracy has connotations to Mao and his followers beyond those, good and bad, which are attached to the term in the West. While to some Westerners the traditional Chinese bureaucracy, with its emphasis on philosophical and literary training for service, seemed close to the Platonic ideal of rule by philosopher kings, the Chinese Communists have always held a very different view. They argue that the traditional bureaucratic system allowed the educated to set themselves apart from the rest of the population and to advance themselves and their relatives at the expense of the common man. Thus to the Chinese Communists bureaucracy entails not only ritualism and red tape, but a selfish quest for power and office obtained through education and perpetuated by emphasizing the distinctive status of the office-holders. While Weber stressed the ways in which the traditional Chinese bureaucracy differed from the rational bureaucracy emerging in capitalist societies, the Chinese Communists tend to emphasize the similarities, and the evils, of both.

Although our knowledge of internal Chinese politics prevents us from being very precise about just what constitutes a "Maoist," and whether Mao Tse-tung would give full blessing to all who use his name, it is clear that throughout his political career Mao has held a sharply critical view of bureaucracy and bureaucrats. As early as 1933, when the Chinese Communists con-

trolled a relatively isolated area of China with a population of only three million, Mao Tse-tung was already railing, "This great evil, bureaucracy, must be thrown into the cesspool" (quoted in Meisner, 1971:29). After assuming power in 1949 the Chinese Communists increasingly followed the Soviet model of economic development; but in the late 1950's they became dissatisfied with the results and, in 1958, launched the Great Leap Forward. The Great Leap was a frenetic attempt to apply the Maoist model of economic development, and it was a dismal failure. While some other Chinese leaders took this experience as evidence of the superiority of rational bureaucratic development, Mao Tse-tung did not. In the Cultural Revolution he launched another attempt to get his anti-bureaucratic model adopted as the ideal for China's future development. Mao's 1967 "Twenty Manifestations of Bureaucracy" is surely one of the most damning critiques ever written on the subject (translated in Joint Publications Research Service, 1970: 40-3). From Mao's writings, from the policies of the Great Leap Forward and the Cultural Revolution, and from the descriptions in the Chinese press of model organizations, we can piece together the Maoist alternative to rational bureaucracy.

One basic aspect of the Weberian ideal type that is modified by the Chinese Communists is the notion that bureaucracies contain a hierarchy of specialized posts to which people are appointed and promoted according to criteria of technical competence. The Chinese do not argue for organizations without hierarchy or without specialized offices, but they do object to the emphasis on technical competence. They do not ignore questions of education and skills, but they also place strong weight on political purity. In practice this means considering social class origins, Party membership, level of political enthusiasm, and performance in past political campaigns. The Chinese Communists also resist the tendency toward a high degree of specialization and the development of professionalism. During the Great Leap and the Cultural Revolution periods in particular, the press was full of examples of highly trained specialists who could not solve the simplest work problems,

and of unskilled personnel who, using common sense and political inspiration, were able to come up with vital work innovations. "Specialists in command," a slogan not out of keeping with the Weberian bureaucratic model, has come to signify an ideological deviation in China. Thus personnel allocation schemes in China are supposed to favor the politically pure generalist more than the apolitical technical specialist.

One can argue that this emphasis on the generalist and the politically pure represents an effort by those who made the revolution to hold on to power and control the technocrats, even if modernization be impeded. But the Maoists feel that there are good economic, rather than simply political, reasons for this emphasis. An exclusive stress on technical competence would not only promote elitism among China's limited number of specialists, but would also, so the argument goes, dull the spirit and initiative of the ordinary members of organizations, the "masses" upon whose efforts the Maoist model depends so heavily. The masses would feel incapable of contributing to the decisions affecting their lives due to their ignorance, and pessimistic about their abilities to compete for higher places and positions. The result, according to the argument, would be passivity and lack of dedication on the part of subordinates, phenomena which would undermine whatever decisions their superiors would make. On the other hand if generalists and the politically pure hold sway, they should be less concerned about establishing their professional prerogatives and more concerned about forging close ties with, and mobilizing the full energy and initiative of, their subordinates.

Perhaps on re-examination, this personnel policy is not such a departure from the bureaucratic ideal type. Hiring in Chinese organizations is supposed to be based on universalistic standards, and vigorous measures are taken to prevent personal ties and favoritism from interfering. But the specialized offices in Chinese organizations are somewhat broader in conception than in the rational-bureaucratic type, in each case including the obligation to work with and mobilize the ideologically-based enthusiasm of subordinates. So political criteria for hiring are seen as required in order to get per-

sonnel with the suitable technical *and* political skills. If all officers in the organization (cadres in the Chinese terminology) are to have political as well as technical duties, then from the point of the view of the Chinese it would be irrational to rely solely on technical criteria in allocating personnel. From this perspective the departure from the bureaucratic ideal type comes more in the conception of the duties of the offices than in the hiring criteria *per se*.

The Chinese Communists also challenge the Weberian emphasis on the autonomy of bureaucratic organizations. It is generally assumed in most Western writings that organizations need a certain autonomy from, or control over, their environment if they are to operate well (cf. Udy, 1970, Chap. 3). Outside demands and interference are seen as diverting the organization from the most rational and efficient pursuit of its goals. The Chinese Communists fundamentally reject this notion of the need for organizational autonomy, and a large number of colorful epithets—"departmentalism," "localism," "mountaintop stronghold mentality"—are used for organizations which try to assert such autonomy. The proposed alternative emphasis is signified by the slogan "politics takes command." What this means is that all organizational decisions and actions are seen as having political implications which extend beyond organizational boundaries. Ideally this means that every action is supposed to be based not only on the desire to maximize internal efficiency, but on its effect on the pursuit of revolutionary social goals. When the two are in conflict, the latter should take precedence. Since the Chinese Communist Party and the authorities in Peking are the arbiters of revolutionary social goals, this means that all organizations throughout society are supposed to be open to continuous direction from these sources. The use of political criteria in personnel allocation is supposed to ensure this openness, by promoting people who are especially responsive to outside political authority. The Chinese Communists claim to have good economic as well as political justification for arguing for both lack of autonomy and the use of political criteria in personnel matters. Political authorities within and outside the organiza-

tion are not seen as meddlers in affairs better left to experts, but as both the people who make sure that decisions do not have undesirable social consequences, and as those who play central roles in getting organizational participants to keep on their toes and avoid mistakes and inefficiency. By relating every organizational activity to national social goals, they may deem some actions and programs unacceptable. But the argument is that those activities which are approved will be pursued with greater dedication because of the politicization involved. Organizational participants should become convinced that their most mundane daily activities have some ultimate impact on the future of socialism and communism, and this realization is supposed to promote high quality work and diligence in avoiding waste and inefficiency. The method used is somewhat different, but the rationale is similar to that of Western advocates of job enlargement and enrichment: if a man finds more meaning in his work, he will work better.

It should be clear by now that the Chinese Communists do not subscribe to the notions of authority contained in the bureaucratic ideal type. They are fundamentally ambivalent toward rational-legal justifications of authority and toward the hierarchy and obedience entailed in large, complex organizations. Individuals are not supposed to obey because they are subordinates in a legitimate organization, or because they have less technical knowledge than their superiors. As mentioned previously, this sort of obedience would be classified as "dulling the initiative of the masses." In Weberian terms, the kind of authority relations desired would probably be classified as charismatic; but this pigeon-holing misses the spirit of the Maoist ideal. Unquestioned obedience to superiors because they are portrayed as carrying out the wishes of Chairman Mao and the Party is also labelled dulling the initiative of the masses. The Maoists idealize a more participatory style of leadership, and it is this emphasis that has occasioned the most interest in the West.

The term "mass line" stands for a large number of procedures organizations are supposed to use to ameliorate the effects of

hierarchy. Cadres are required to establish regular schedules for spending part of their time out of their offices and down working with their hands alongside their subordinates. While to the Western mind this may seem like a poor use of time for skilled personnel, again the Maoists see economic benefits. Sending administrators and technical personnel down to the basic levels is seen as fostering organizational cohesion, while increasing the information superiors have about concrete problems and how subordinates are reacting to work situations. The emphasis on getting cadres to go down and participate in labor is only one side of the "two participations." The other is that subordinates are supposed to be systematically organized to participate in decision making. In industry one of the most common forms advocated is the "triple combination" in which technical problems are attacked by ad hoc groups composed of cadres, technicians, and ordinary workers. The "revolutionary committees" which now administer post-Cultural Revolution Chinese organizations are similarly supposed to include representatives not only of administrators, but of subordinates and even janitorial personnel.

Even when decisions do originate from organizational superiors (or authorities outside the organization), they are not supposed to be simply announced and obeyed. Rather there are elaborate procedures for mobilizing support for decisions made at higher levels. This, too, reflects the mass line approach. A new policy is announced and explained, and then subordinates break into regular discussion groups to go over each point in detail. In these groups efforts are made to convince everyone of the need for a change in routine, to elicit suggestions and ideas, and to get "activists" to encourage their co-workers to support the change. This kind of communication process is supposed to take place not only in technical matters, but in basic political decisions as well. All things affecting the lives of members of an organization are supposed to include those members in some way in their resolution. The extent of actual influence of subordinates over policy decisions may not be great, but they should not be left out of the process completely. The aim

here is similar to the goals of "participative management" in the West: subordinates, by taking an active although perhaps secondary part in decisions affecting them, will identify more with the organization and contribute more to it.

The thoroughness with which subordinate participation is supposed to be pursued in China, however, goes much further than the Western thought (and also Soviet thought) along these lines. In Western organizations subordinates may or may not respond to higher requests for ideas and suggestions, depending in part on the influence of informal social groupings and norms. In Chinese organizations subordinate participation is not to be just solicited, but guaranteed; and to this end active efforts are made to formalize and mobilize the informal social groupings of subordinates. The basic vehicle is the discussion group already mentioned. The factory work group, school row, office section, and military squad all constitute groups which are to maintain their form outside the activities and hours of formal organizational life (cf. also Schurmann 1959, Whyte, 1970). These groups, numbering generally eight to fifteen members, elect officers, arrange joint recreation, hold outside political study meetings, and engage in group criticism and self-criticism rituals. Notes are taken by a designated "recorder" at discussion and criticism meetings, and these are used to report group ideas and morale to superiors. The group leader will cultivate within the group certain "activists" whom he can count on to help him steer discussion and criticism meetings along proper channels. It is in these groups that subordinates not only discuss new pronouncements, but criticize their own ideological shortcomings and poor work performance, and promise improvement to their peers. Just as the organization itself is supposed to be permeated by outside political demands, so primary groups within that organization are to be formalized and politicized, and made unable to pursue autonomous objectives. Obviously there are problems in implementing this ideal, since participation which is mandatory and organized from above may not be enthusiastic, and views expressed in the group may not come from the heart. But the procedure

does illustrate the ideal conception of the leader-led relationship within Chinese organizations in which leaders will be in close contact with and solicitous of the views of the led, but the led will be unable to resist or be uninvolved.

By a number of other devices the Chinese Communists strive to minimize the importance and effects of organizational hierarchy. The similarity of dress of people high and low tends to downplay status distinctions, and this egalitarianism was given its most noted application in the elimination of ranks and insignia of rank in the Chinese armed forces in 1965. Here it should be noted that absolute equality is not being pursued, and is in fact denounced as a deviation. The commanders of companies and regiments are still the commanders of companies and regiments, with much more authority than the ordinary soldier; but they now have less in the way of visible symbols of their distinctiveness. Similarly, the Chinese do not reject the basic bureaucratic feature of higher remuneration for those holding higher posts. In fact both wages and perquisites such as apartments are distributed according to rank. But at the same time efforts are made to downplay the differences. Wage differentials are kept within limits narrower than are common in the United States, Russia, or developing societies; and the relatively more spacious apartment of an administrator may be in the same apartment complex where his unit's janitor lives (cf. Richman, 1969:804-8). In general it is felt that the higher performance that might result from those favored by wider differentials and status distinctions would be more than offset by lowered organizational cohesion and poorer morale among those at the lower end of the scale. In a variety of ways, then, the hierarchy that exists in Chinese organizations is supposed to be de-emphasized, with efforts made to form close ties and communications across hierarchical divisions.

At the same time alternative forms of rewards and recognition are provided which are not dependent on rank or productivity. In industry, piece rates have been largely discarded since the Cultural Revolution; and the differentiated wage levels which remain are supplemented by selections of

honorary workers based not only on work performance, but on criteria of political enthusiasm and willingness to help other workers. In rural communes the pegging of rewards to individual work tasks has been replaced by wages set in public discussion meetings, where the criteria include not only work performance, but labor attitudes and political enthusiasm. While in the West we should expect a weakening of the link between performance and remuneration to lead to poorer performance, Chinese authorities argue the reverse: weakening this link makes it more possible to avoid the calculative involvement and lack of initiative that comes from a strong emphasis on material incentives. Or, to put the matter in the light of our earlier discussion, if organizational roles are defined more broadly to include not only immediate work tasks but political contributions, then perhaps rewards should be based on such broader criteria. But keep in mind that the Maoists argue that by such a strategy organizational performance will be enhanced even if we consider only work output, and the Chinese media are full of examples of organizations which improved their production after they scaled down their systems of material incentives and replaced them with increased political mobilization.

If the drive to blur hierarchical divisions and eliminate the autonomy of informal primary groups could be successful, then a number of assumptions of Western organizational thought would have to be modified. One basic empirical generalization is that varying means of securing compliance are required in different organizations, depending in part on the orientation of subordinates (Etzioni, 1961).² If you expect sullen and antagonistic subordinates, as in a prison, then you had better be willing to use coercion. If you expect relative indifference or calculative involvement, then material incentives are more appropriate; while if you expect enthusiasm and commitment you can rely more on persuasion and the manipula-

tion of symbols. In China, while coercion and material incentives remain important (cf. Skinner and Winckler, 1969), there is a constant effort to get people to respond more to what Etzioni calls normative and social power. The organizational elements we have already described (the mass line, politics in command, the small group) are supposed to make this possible. If these techniques can be successfully applied, subordinates should respond more and more to social pressure and patriotic appeals; and there should be less need for coercion and material incentives, no matter what the type or goal of the organization involved. In other words, the Maoist ideal is supposed to be appropriate and beneficial not in special sorts of organizations, but in everything from the Party itself to the forced labor camps which form the core of the Chinese penal system. In the latter case, an inmate should feel surrounded not by an inmate subculture exerting pressure to subvert labor camp rules, but by other inmates urging him to reform and confess any thoughts he may have of escaping.

The emphases on close organizational ties and on relating every activity to national goals alert us to another Maoist deviation from the rational bureaucratic ideal type. The Maoist ideal does not entail formalistic impersonality. The Chinese Communists do want their organizations to be impersonal in the sense that individual friendships and rivalries are not supposed to influence activities and decisions. The ideal is comradeship rather than formalistic impersonality. This means that everyone is supposed to show a high degree of personalistic concern and solicitude for everyone else. People are expected to treat each other not just as holders of narrow roles, but as whole individuals with problems and private lives which may affect organizational performance. Within a small group it is supposed to work like this: Members are not simply to criticize each others' failings constantly. Rather they should engage in a variety of joint activities, helping each other to solve personal problems, while at the same time criticizing those who depart from the official line. The same kind of personal concern matched by criticism is supposed to extend across hierarchical divisions within the organization.

² Etzioni specifically states in his introduction that his empirical generalizations may not hold for non-Western, non-democratic societies. Skinner and Winckler (1969) have found, however, that Etzioni's scheme helps them to interpret patterns of change in rural organizations in China.

This ethic of comradeship is seen as contributing to organizational loyalties in a way an ethic of impersonality could not, and we have already seen that the effort to build strong organizational loyalties occupies a central place in the Maoist ideal.

Emotions are also regarded somewhat differently in Chinese organizations. Individuals are not to let personal emotions interfere with their performance, but the ideal is not unemotionality. Rather a high degree of passion and zeal should be generated for even the most mundane tasks. Much of the effort of Party cadres and propagandists goes into proclaiming slogans, organizing group competitions and mass meetings, and so forth, in the effort to mobilize the proper spirit. Individuals are advised to regard taking school quizzes, selling pork, or spreading manure as objects of revolutionary struggle which must be overcome with the same zeal that characterized the guerrilla struggle against Japan of an earlier era.

These basic notions of coopting primary groups, comradeship, and zeal are all related to the fact that Chinese organizations make greater claims on their participants than do their Western (or Soviet) counterparts. The "partial inclusion" and limited contractual obligations of officeholders which in the bureaucratic ideal type protect participants from undue exploitation are not basic features of the Maoist ideal. No aspect of the life of an individual is regarded as completely irrelevant to his organizational performance. Informal contacts within the organization, outside recreation with friends, marital relationships, and many other factors are seen as affecting the performance of individuals. The leaders of a Maoist organization are to try to make sure that all these influences support, rather than undermine, organizational goals. Internal activities are highly organized, spare time recreation is arranged, evening political study sessions are run, and at times efforts are even made to organize families and outside friends for organizational purposes. During the political campaigns which periodically sweep across Chinese society, work days may be extended; and in some cases individuals will have to remain within the organization for days or even weeks without returning home. These efforts tend

to make Chinese organizations more total in scope and more pervasive than their Western counterparts (i.e., their members engage in more joint activities and there are more activities inside and outside the organization for which the organization sets norms—cf. Etzioni, 1961:160–3). While partial inclusion is seen in Western organizational thought as freeing officials from nonrational obligations and interference so that they can apply their expertise to their job, in Maoist organizations the more nearly total inclusion is seen as promoting organizational involvement and commitment, which is supposed to lead to more diligent and efficient work.

A few further contrasts round out our picture. The Chinese Communists also do not accept the bureaucratic notions of contractually based job security and office-holding as a career. In the Weberian ideal type these features are seen as encouraging the acquisition of high level skills and their optimal use, and eliminating the need to bend to pressures and toady to superiors. In Chinese organizations many people do serve in one post for long periods, and do ascend the ladder of ranks; but the official ideal is that individuals serve at the will of the state. "Careerism," the desire to acquire skills and use them to rise in a bureaucratic hierarchy, is seen as a traditional political evil. Instead, skilled personnel are supposed to be willing to leave comfortable and familiar posts for terms in manual labor or work for which they have not been trained. There are a number of reasons for this emphasis, including the desire to undermine bureaucratic elitism; but probably the major justification is the effort to prevent rigidities in the national personnel allocation system. (State control over employment and job changes is more nearly total in China than in, say, the Soviet Union.) Officeholders who have outlived their usefulness do not have to be "kicked upstairs," but can be "sent down" or shifted to some other work to meet the rapidly changing demands of the developing economy.

Maoists are also suspicious of the bureaucratic assumption that organizations should be rule-bound and possess stable routines so that participants will have the security and calculability needed for rational action.

In China the notion of stable routines conflicts with the desire to maintain a spirit of revolutionary change. Rules and work procedures are looked on with suspicion as things bureaucrats use to control subordinates so that they can maximize their own successes, without necessarily maximizing the successes of their organizations. Routines and rules fall again into the familiar category of "dampening the enthusiasm of the masses." According to the Maoists, organizations should strive to minimize their rules and procedures so that members (particularly subordinates) who have new ideas and innovations that will improve work will feel free to carry them out. Periodically special campaigns are launched to "shake up" organizations and "break through" unnecessary rules and procedures.

The Maoists also reject the notion of unity of command, prominent in classical organizational theory and in Weber's monocratic bureaucracy and Soviet one-man-management. With unity of command everyone has only one immediate superior to deal with, and this is usually seen as avoiding confusion and conflict while making it easy to specify responsibility and accountability for decisions. The Chinese, who followed Soviet one-man-management ideas in the early 1950's, have since shifted more to the diffusion of decision-making both vertically and horizontally within organizations. Horizontally this means collective decision-making by Party committees in consultation with administrators, technicians, and workers. Vertically this means referring many kinds of decisions up and down various levels of the administrative hierarchy, often several times, for ideas, reactions, and approval. This procedure may lead to delays in decision-making, and may make it difficult to individualize responsibility; but the Chinese feel that it will contribute to the desired general involvement in organizational goals and activities.

We have now reviewed a large number of ways in which the Chinese Communists (or at least the Maoists among them) either specifically reject or modify basic features of Western organizational models. These contrasts should not blind us to the similarities between Maoist and Western organizational conceptions. Chinese organizations

do have specific goals, and they do employ a division of labor entailing a hierarchy of specialized offices in pursuit of these goals. Those at the top of organizations have authority over those lower down; and in general they have more training and experience, and receive greater rewards, than do their subordinates. Universalistic criteria are to be used in allocating personnel; and files, rules and written communications are basic facts of life in Chinese organizations. Offices are separated from office-holders, who can be replaced. Perhaps a brief list at this point will highlight the similarities and the contrasts.

Admittedly this listing is something of a hodge-podge. We have compared features of the Maoist ideal with characteristics drawn from various Western sources: the Weberian ideal type of rational bureaucracy, basic assumptions of other schools of thought, and empirical generalizations from Western organizational research. It has also been mentioned that the prescriptions of the Maoist Ideal have some parallels not listed here with the thinking of some other Western organizational theorists, most particularly with what is known as participative management (cf. McGregor, 1960; Likert, 1961; Tannenbaum, 1968). Weber himself was, of course, not unaware of the economic potential, but perhaps limited applicability, of organizations departing from the rational bureaucratic ideal type in the direction of value infusion and collective commitment. His writings on monastic and congregational forms of organization are particularly relevant here (cf. Weber, 1968: 1168-70, 1204-10).

Clearly underlying all the contrasts listed is a general disagreement over the ways organizations are conceived. In the rational bureaucratic conception, the central concern is with achieving internal efficiency through the maximum use of technical knowledge. In the Maoist ideal the predominant emphasis is instead on finding ways to maximize the involvement and commitment of organizational participants, particularly the "masses" at the bottom of organizations. While Weber focussed most of his attention on the administrators within bureaucracies, rather than on the entire personnel, the Maoists focus most of their

CONTRASTS

Western conceptions

1. Use criteria of technical competence in personnel allocation
2. Promote organizational autonomy
3. Legal-rational authority
4. Informal social groups unavoidably occur
5. Differentiated rewards to office and performance encouraged
6. Varied compliance strategies needed, depending on the organization
7. Formalistic impersonality
8. Unemotionality
9. Partial inclusion and limited contractual obligations of office-holders
10. Job security encouraged
11. Calculability through rules and established procedures
12. Unity of command and strict hierarchy of communications

Maoist conceptions

1. Use both political purity and technical competence
2. Politics takes command, and openness to outside political demands
3. Mass line participative-charismatic authority
4. Informal groups can and should be fully coopted
5. Differentiated rewards to office and performance deemphasized
6. Normative and social compliance should play the main role everywhere
7. Comradeship
8. Political zeal encouraged
9. Near total inclusion and theoretically unlimited obligations
10. Job security not valued, and career orientations not encouraged
11. Flexibility and rapid change valued, rules and procedures looked on with suspicion
12. Collective leadership and flexible consultation

SIMILARITIES

- | | |
|--|--|
| 1. Organizations have specific goals | 1. Same |
| 2. Organizations utilize a hierarchy of specialized offices | 2. Same |
| 3. Authority and rewards greater at the top of an organization | 3. Same, although efforts to deemphasize |
| 4. Universalistic hiring and promotion criteria | 4. Same, although criteria differ |
| 5. Files, rules, and written communications regulate organizational life | 5. Same, although not always viewed positively |
| 6. Offices separated from office-holders | 6. Same |

attention on how subordinates are tied into the organization. To oversimplify, the primary concern of Maoists is with maximizing (human) inputs rather than with getting the most return from limited inputs. It can be argued that there is some sense to this approach in China's case given her relatively low level of economic development and abundance of unskilled labor. But the Maoists make a more general case for the value of their organizational ideal, one that says it should be used in organizations of all types, no matter what the mix of back-

grounds and skills of participants. Again the parallels with participative management are suggestive. Advocates of the Maoist ideal claim that its implementation will produce greater involvement in organizations among participants, thus producing more diligent, careful, and creative work. Thus much of the poor performance which occurs when the ideal type of rational bureaucracy is translated into practice³ (ritualism, restriction

³ Rational bureaucracy as an ideal type has no direct normative implications. However, because of the efficiency which is claimed for rational

of output, etc.) is seen as avoidable if the Maoist ideal is followed. In other words the major emphasis on involvement rather than on internal efficiency is seen as producing, as a byproduct, greater actual efficiency. This line of thinking is the basis for the argument that the Maoist ideal, far from being incompatible with economic development, can actually make a contribution. The Maoists do not feel that they have to sacrifice economic progress to remain true to their ideology; they feel they can be Maoists and also modernize Chinese society.

At this point we might return to the question of how non or anti-bureaucratic the Maoist ideal is. The answer depends on what we take as the defining attributes of bureaucracy. The Maoists have not dismantled large-scale organizations and introduced participatory democracy. As we have seen, the critique of bureaucracy is broad but not total; and organizations with multiple levels of specialized offices continue to exist. Individual organizations are controlled and coordinated by national state administrative, army, and party hierarchies.⁴ If we take size, hierarchy, and division of labor as our criteria, then, rather than other traits on our list, we can say that the Maoist, like the advocates of participative management, are not rejecting bureaucracy, but are trying to build more responsive and efficient bureaucracies.

However bureaucratic or non-bureaucratic we regard the Maoist ideal, it is clear that what is advocated is different in many ways from Western organizational experience. How do we evaluate the claims made for this organizational ideal? There seem to be two alternatives. We can examine the available evidence on whether the Maoist ideal, when implemented in real organizations, has the favorable results (both political and economic) which are claimed for it. Or we can consider the logic of the Maoist ideal—in other words consider it as an ideal

type comparable to the Weberian rational-bureaucratic type, and deal with its adequacy at that level. We will discuss both approaches briefly in the pages to follow.

The problem in using the available empirical evidence is, of course, that there isn't much; and what there is doesn't bear directly on our question. To illustrate: The Chinese press regularly carries stories about how, during the Cultural Revolution, organizations scaled down material incentives, increased participation from below, etc.—with the result that everyone felt much more ideologically correct, and the organization in question broke all previous production records. But how representative are these articles of the experience of all organizations? May there not be organizations which achieved negative results from the same procedures? Are the production increases mentioned the result of improved involvement and commitment, or perhaps of other changes, such as technological improvements, improved supply of raw materials, and so forth?

We have similar problems with the negative evidence. The Maoist organizational ideal was espoused earlier, during the Great Leap Forward (1958–1960); and the Great Leap was a disaster, leading to economic depression and famine. But what role did following the Maoist organizational ideal play in this failure? How much was due instead to bad weather, poor national economic planning and coordination, the withdrawal of Soviet economic aid, the overly hasty introduction of new structural reforms, perhaps even an insufficient explanation of what the Maoist organizational ideal was? The economic recovery which occurred after the Great Leap Forward was abandoned has indicated to many China scholars the economic superiority of rational bureaucratic as opposed to Maoist tactics. But does the current record of post-Cultural Revolution economic progress indicate that the Maoist ideal, if properly implemented, does work? Or does it mean that the proper Maoist slogans and external forms are being observed, while underneath rational bureaucratic tactics are responsible for any favorable results? Nothing much of a definitive sort can be said using this sort of crude evidence.

bureaucracy, the ideal type easily becomes an ideal for those wishing to construct more efficient organization.

⁴During the Cultural Revolution the Chinese Communist Party was attacked and immobilized. The more recent reconstruction of the Party seems to have brought in new members, but the structure is largely as before.

There are some data from visitors to Chinese enterprises and from refugee interview studies of Chinese organizations, but even these do not get us much closer to understanding the consequences of pursuing the Maoist ideal. Barry Richman (1969) spent two months in China in 1966, just as the Cultural Revolution was getting under way, and collected a wealth of data on thirty-eight industrial enterprises. His report on these enterprises contains detailed comparisons with similar enterprises in India, Japan, the Soviet Union, and America. His conclusion is that enterprises in China operate more efficiently than their counterparts in India, although not so well as those in the other countries. But much of the credit for this superiority over India Richman assigns to China's coming closer to the rational bureaucratic ideal, rather than to being more Maoist. In general Richman feels, more on the basis of the Great Leap experience than on his own survey, that Maoism in small doses makes a positive contribution to work motivation, but in large doses it produces serious irrationalities in management. The actual data he collected in the thirty-eight enterprises doesn't tell us much about how Maoist or non-Maoist these organizations were. Below we reproduce selected data Richmond collected on two types of enterprises, machinery producing and chemical and pharmaceutical enterprises, ranked by Richman's rating (out of thirty-eight) of their managerial know-how and general efficiency given the physical technology available.

There seems to be a slight tendency among the machinery enterprises for some standard indicators of bureaucratization, such as the number of employees, the percentage of administrative and technical personnel, and the ratio of maximum pay to average pay, to correlate positively with Richman's subjective rating of enterprise efficiency; but among the chemical and pharmaceutical enterprises no such relationships are visible. But Richman's figures tell us relatively little about whether trying to implement the Maoist ideal helps or hurts organizational efficiency. There are no good indicators of closeness to the Maoist ideal in Richman's data, and we can't assume that where there is no bonus fund or where dif-

ferentials are low there is a compensating use of political mobilization and participatory approaches. About such things as the proportion of Party membership, the frequency of organized political study and group recreation, and so forth, Richman provides no information. And since his ranking of efficiency is a subjective one based on his own conceptions of managerial effectiveness, we cannot be sure that it is independent of the other data it is ranged against, such as the proportion of personnel with specialized education.

Studies of Chinese organizations based on refugee interviews, provide some additional information (George, 1967; Barnett and Vogel, 1967; Whyte, 1970, 1973). These studies yield several generalizations. First, vigorous efforts have been made in organizations of widely varying types to implement the Maoist ideal. Second, when efforts are made to implement this ideal, various problems tend to produce an operating reality which is often rather distant from the ideal. The Maoist ideal does not seem to grow naturally, but has to be fostered and pushed through campaigns launched from Peking (cf. here also Skinner and Winckler, 1969). When this is not done, what seems to develop naturally is bureaucracy in the Western sense, which does not need central encouragement. But in those organizations which do approximate the Maoist ideal, the predicted effects of a heightened sense of community, willingness to work longer and harder, and strong identification with organizational and national goals do seem to result. And even when an organization does not closely approach the Maoist ideal, the communication and participation procedures may have beneficial effects from the point of view of administrators, which keep them from being abandoned. Let us elaborate briefly on these points.

Visitors to China bring back information about officials laboring with their hands, subordinates participating in decision making, regular group political study routines in organizations of all types, and so forth. Yet the fact remains that periodically campaigns are launched to revive these Maoist procedures; and officials are charged with not encouraging political rituals in their

Table 1. Richman's Subjective Rating of Know-how and Efficiency Given the Available Technology by Selected Other Indicators, for Machinery and Chemical and Pharmaceutical Enterprises

Richman's Rank (of 38)	Name	No. of Employees	Age of Firm ^c	Average Pay (yüan/month)	Max. Pay ^d Ave. Pay	Max. Bonus Fund (% of Wage Fund)	Admin. & Technical Personnel (% of Employed)	Percentage of Employees ^h with Higher or Specialized Secondary Education
Machinery enterprises:								
5	Shanghai Machine Tool	6,000	20+	70	3.0	10	20	8.3
6	Wuhan Heavy Machinery	7,000	8	66	2.27	Ceased 1966	36.5	25.5
13	Peking 1st Mach. Tool	4,000	16+	52	3.46	8-9	27.5	17.5
14	Shanghai Machine Tool #3	1,000	8+	75	1.68	15	20	7
19	Canton Mach. Tool	3,100	8 (app.)	67	2.09	Ceased 1966	n.a.	8.5
20	Wusih Machinery	300	8	48	1.81	7	13	2
22	Shanghai Forg. & Press.	405	6+	75	1.53	10	12	2
31	Nanking Machinery	1,300	12 (app.)	60	2.0	Ceased 1965	n.a.	15
32	Tientsin N. Lake Instr.	165	8	47.5	2.02	4	6	0
37	Hangchow Mach. Tool	1,000	14 (app.)	61	1.77	8	20	11
Chemical and pharmaceutical enterprises:								
1	Peking Coke & Chem.	2,100	7	61	2.46	6	20	11
16	Peking Pharmaceutical	3,000	11	60	2.3	7	18	10
17	Nanking Chem. Fertil.	10,000	30 (app.)	62	2.74	Ceased	27	6.9
18	Canton Chem. Fertil.	2,400	3	65	3.08	7	23	11
25	Shanghai Pharmaceutical	1,200	13	66	2.64	5	13	13

Key: n.a. = not available; app. = approximate

^aRichman, 1969, 792-794.

^bRichman, 1969, 792-794.

^cRichman, 1969, 726-737.

^dRichman, 1969, 800-802 (pay scales follow a national scale, but with regional variations to accommodate varying cost of living standards).

^eComputed from Richman, 1969, 800-802 (these figures are for total enterprise personnel, not just for the workers).

^fRichman, 1969, 800-802.

^gRichman, 1969, 754-756.

^hRichman, 1969, 154-156.

organizations, and with arrogantly ignoring ideas and criticisms from below.

This failure to approximate the Maoist ideal is not due solely to the bureaucratic mentality of Chinese administrators, but to a variety of problems which arise in real organizations when the ideal is applied. If organizational participants are not very committed or cohesive to start with (as in, say, a forced labor camp), they are unlikely to apply the kind of group social pressure necessary to achieve general involvement. Even when organizations try to maximize the inclusion of participants, individuals who have relatives who have suffered in past campaigns, who are worried about the illness of a spouse, or who are hoping the clock will speed up so that they can resume yesterday's basketball game, are unlikely to respond to political appeals with the desired enthusiasm. In spite of vigorous efforts there are parts of the organizational environment over which administrators in China, as in other societies, have little control; and these may interfere with the creation of the desired atmosphere. Also in the Maoist ideal, as in participative management schemes, there is the problem of what happens when subordinate initiative is stimulated; and the result is ideas and demands that superiors do not wish to follow. In other words the participation in Chinese organizations is stimulated from above rather than below, and can also be managed and turned off from above; but in doing so the chances of maintaining valued participation are jeopardized. On the other hand the emphasis on subordinate participation can sometimes interfere with using the ideas and experience of subordinates. If superiors concentrate on mobilizing enthusiasm for, and eliminating anxieties about some new activity (such as building backyard steel furnaces during the Great Leap Forward), they may suppress valuable and rational objections. So much effort is given in the Maoist ideal to overcoming organizational inertia, which is seen as stemming from bad political views, individualism, and bureaucratic ritualism, that inertia stemming from rational sources may be overlooked.

Further problems could be cited, but this brief discussion should make clear a not very surprising fact: When trying to imple-

ment the Maoist ideal, as when trying to implement other ideals, reality intrudes to produce unanticipated non-ideal results. Given the great effort expended on political and participatory rituals in Chinese organizations, the question remains whether there is a commensurate pay-off. Or perhaps do administrators follow Maoist procedures because Peking demands them, although they create more problems than they solve? This may sometimes be the case, but the available evidence points to the value of the Maoist procedures in some circumstances even when they are not fully implemented.

Alexander George (1967), after extensive interviews with captured Chinese soldiers during the Korean War, concluded that much of the impressive performance of the Chinese Army against the more heavily armed U.N. forces could be attributed to the skillful use of the Maoist ideal: the encapsulation of soldiers in small groups with regular political rituals and group criticism; the maintenance of comradely relations and mutual consultation between officers and men, the penetration of political cadres to the lowest levels, and the politicizing of every military activity. U.S. military observers had been sufficiently impressed by the cohesiveness and fighting spirit of the Chinese Communist Army of an earlier period, during the 1930's, to adapt the cohesive small group concept into what became the U.S. Marine Corps fire teams (George, 1967:52-3).

To pick another organizational example, the Maoist model would seem to be singularly inappropriate for penal institutions, where we could hardly expect the desired kind of enthusiasm and involvement to develop among incarcerated inmates. Interview materials (Whyte, 1973) suggest that this is true, but that even in forced labor camps the Maoist procedures have contributions to make. Even though inmates do not identify with their guards or sense devotion to the camp that confines them, their encapsulation in small groups led by activists, groups which engage in political study and mutual criticism rituals of inmate failings, does hinder the development of the kind of deviant inmate subculture which is a familiar feature of penal institutions in other societies. As a result the inmates are not

enthusiastically reforming themselves, but they are easier to control; and this is reason enough for Chinese penal officials to continue to stress the coopting of informal groups even when they are not following other "mass line" procedures.

The admittedly sketchy evidence available suggests that it is difficult for organizations to approach the Maoist ideal, that when they try they often deviate in unexpected ways, but that in at least some circumstances organizations benefit from applying Maoist procedures. Where does this leave us in our analysis of the Maoist ideal? Let us conclude by considering the Maoist ideal not in practice, but as an ideal type, and compare its adequacy relative to the Weberian ideal type of rational bureaucracy. Weber's ideal types were constructs of abstracted elements forming a unified pattern which were to serve as conceptual tools to highlight certain features of organizations, even though no concrete organization would be a perfect fit on all the given characteristics. (Weber, 1968:20-1; Goode, 1947:473-5; Mouzelis, 1967, Chapter II). While it is not valid to criticize an ideal type because no real organization fits the type, or because it leaves out characteristics real organizations possess, it is legitimate to consider whether the traits specified are objectively possible, and whether they are coherent and adequate at the level of meaning, i.e. whether, taken together, they have the logical implications claimed for them. In the case of rational bureaucracy, this means whether Weber's list of traits contradicts known laws of nature, and if, taken together, the traits suggest a coherent model which, if realizable, would lead to maximum rationality and internal efficiency. Even on these grounds Weber's ideal type of rational bureaucracy is not immune from criticism (cf. Mouzelis, 1967:47-8), since, for example, there is an inherent contradiction between offices bound by rules and procedures and offices with incumbents free to make maximal use of their knowledge. And more recent theorizing has suggested that the maximum efficiency predicted for rational bureaucracy would only result in limited organizational situations, generally those of stable and routine technology (Perrow, 1970, 1972).

Considered in comparable terms the Maoist organizational ideal type does not fare badly. In spite of the criticism of some that Maoism represents utopianism and unrealizable objectives, it is not clear that the Maoist organizational traits discussed earlier violate known laws of nature. And the traits do seem to form a relatively coherent pattern organized around the concern for maximizing organizational involvement, even though internal contradictions (e.g. between getting perfect information on subordinate opinions and getting perfect subordinate consensus) are detectable here as well. What is less clear is that the Maoist ideal type should logically lead not only to a maximization of involvement, but also to an equal or higher degree of internal efficiency than the Weberian ideal type. Efficiency would seem to depend on both maximizing the application of knowledge and maximizing the motivation of participants, and a high degree of zeal in wasteful directions is possible within the framework of the Maoist ideal type.

The reasoning suggests a conclusion somewhere between the supporters and the critics of the Maoist ideal. The Maoist ideal does not seem to be totally irrational or contrary to human nature or the demands of industrialization. Activities which seem irrational to Western eyes may have a rational justification. For example, weekly sessions for the study of Mao's thought for factory workers may not simply interfere with production by tiring people out. Insofar as this activity strengthens a sense of organizational cohesion and identification, it may contribute to production. At the same time, it is not clear that the Maoist ideal is a panacea for all organizational problems, or that it can even be very easily applied. In real Chinese organizations its application may result in some cases in both political involvement and internal efficiency, in others in political involvement without greater efficiency, or perhaps in failure in both areas. It would take much better information than we have available now to specify the conditions required for successful application of the Maoist ideal. The suggestion that this ideal is appropriate for all circumstances may be just as dubious as the suggestion that organizations modeled

after Weber's ideal type will be the most efficient in all circumstances. Thus claims that China has found a route to modernization without bureaucratization, or that the Maoist ideal solves the problem of how to modernize without sacrificing revolutionary social goals, must continue to be treated skeptically. But this skepticism should not blind us to the opportunity to broaden our understanding of organizational dynamics by a closer scrutiny of Chinese organizational innovations.

REFERENCES

- Andors, Stephen
1971 "Revolution and modernization: man and machine in industrializing societies, the Chinese case." Pp. 393-444 in Edward Friedman and Mark Selden (eds.), *America's Asia*. New York: Vintage.
- Barnett, A. Doak and Ezra F. Vogel
1967 *Cadres, Bureaucracy and Political Power in Communist China*. New York: Columbia University Press.
- Etzioni, Amitai
1961 *A Comparative Analysis of Complex Organizations*. New York: Free Press.
- George, Alexander L.
1967 *The Chinese Communist Army in Action*. New York: Columbia University Press.
- Goode, William J.
1947 "A note on the ideal type." *American Sociological Review* 12 (August):473-5.
- Gurley, John G.
1971 "Capitalist and Maoist economic development." Pp. 324-56, in Edward Friedman and Mark Selden (eds.), *America's Asia*. New York: Vintage.
- Joint Publications Research Service
1970 "Chairman Mao discusses twenty manifestations of bureaucracy." Pp. 40-3 in *Translations on Communist China* No. 90, Washington, D.C., February 12.
- Lewis, John W.
1968 "Leader, commissar and bureaucrat: The Chinese political system in the last days of the revolution." Pp. 449-81, in Ping-ti Ho and Tang Tsou, (eds.), *China in Crisis*. Chicago: University of Chicago Press, Vol. I, Book 2.
- Likert, Rensis
1961 *New Patterns of Management*, New York: McGraw-Hill.
- Loewenthal, Richard
1970 "Development vs. utopia in communist policy." Pp. 33-116, in Chalmers Johnson (ed.), *Change in Communist Systems*. Stanford: Stanford University Press.
- McGregor, Douglas
1960 *The Human Side of Enterprise*, New York: McGraw-Hill.
- Meisner, Maurice
1971 "Leninism and Maoism: Some populist perspectives on Marxism-Leninism in China." *China Quarterly* 45 (January/March):2-36.
- Mouzelis, Nicos P.
1968 *Organisation and Bureaucracy*, Chicago: Aldine.
- Perrow, Charles
1970 *Organizational Analysis: A Sociological View*. Belmont, Calif.: Wadsworth Publishing.
- Perrow, Charles
1972 "A framework for the comparative analysis of organizations." Pp. 48-67, in Merlin B. Brinkerhoff and Phillip R. Kunz, (eds.), *Complex Organizations and Their Environments*. Dubuque, Iowa: Wm. Brown.
- Richman, Barry M.
1969 *Industrial Society in Communist China*. New York: Random House.
- Schurmann, H. F.
1959 "Organization and response in Communist China." *Annals of the American Academy of Political and Social Science* 321 (January):51-61.
- Skinner, G. William and Edwin A. Winckler
1969 "Compliance succession in rural Communist China: A cyclical theory." Pp. 410-38, in Amitai Etzioni (ed.), *A Sociological Reader on Complex Organizations*, 2nd edition. New York: Holt, Rinehart and Winston.
- Tannenbaum, Arnold S.
1968 *Control in Organizations*, New York: McGraw-Hill.
- Udy, Jr., Stanley M.
1970 *Work in Traditional and Modern Society*. Englewood Cliffs, N.J.: Prentice-Hall.
- Weber, Max
1968 *Economy and Society*. Guenther Roth and Claus Wittich (eds.). New York: Bedminster Press.
- Whyte, Martin K.
1970 *Small Groups and Political Rituals in Communist China*. Unpublished Ph.D. thesis, Harvard University.
- Whyte, Martin K.
1973 "Corrective labor camps in the People's Republic of China." *Asian Survey* (March).

ON THE USE OF THE MASS MEDIA FOR IMPORTANT THINGS *

ELIHU KATZ

MICHAEL GUREVITCH

HADASSAH HAAS

The Hebrew University of Jerusalem

and

The Israel Institute of Applied Social Research

American Sociological Review 1973, Vol. 38 (April):164-181

The mass media are ranked with respect to their perceived helpfulness in satisfying clusters of needs arising from social roles and individual dispositions. For example, integration into the sociopolitical order is best served by newspapers; while "knowing oneself" is best served by books. Cinema and books are more helpful as means of "escape" than is television. Primary relations, holidays and other cultural activities are often more important than the mass media in satisfying needs.

Television is the least specialized medium, serving many different personal and political needs. The "interchangeability" of the media over a variety of functions orders televisions, radio, newspapers, books, and cinema in a circumplex. We speculate about which attributes of the media explain the social and psychological needs they serve best. The data, drawn from an Israeli survey, are presented as a basis for cross-cultural comparison.

EARLY in the history of communications research (Cantril, 1942), an approach was developed to studying the "gratifications" which attract and hold audiences to the kinds of media and the types of content which satisfy their social and psychological needs. Although its career has been chequered, and it has been much overshadowed by the study of "campaigns" to change opinions and attitudes, this approach has persisted, grown in sophistication, and even undergone something of a revival lately.¹ What deserves emphasis, however,

is that these studies of media "uses and gratifications" are not only interesting in themselves; but they are, ultimately, an effort to understand "effects."²

This approach to mass communications is essentially functional (Wright, 1959). It argues that people bend the media to their

of functions suggested or confirmed by respondents. The best known examples are Cantril's (1942) analysis of quiz programs; Herzog's (1954) and Warner and Henry's (1948) studies of the functions of the daytime serial; Waples' *et al.* (1940) study of the functions of reading and Berelson's (1954) study of what the newspaper means to its readers. More recent studies compare the media use of different groups. See Riley and Riley (1951), and Johnstone (1961). Current studies are centered in a number of European countries as well as the U.S. See Blumler and McQuail (1968); McQuail *et al.* (1972); Emmett (1968); BBC Audience Research (1972); Rosengren and Windahl (1972); Lundberg and Hulten (1968); Mendelsohn (1966); Stephenson (1966); Escarpit (1966); Robinson (1972); and Nordenstreng (1969).

² Contrary to what some people think, the recent studies of the immediate effects on children of seeing violence on the screen still find, by and large, that aggressive responses are confined to children who are previously disposed. Imitative effects, even here, are confined to the predisposed, to smaller children, and particularly to the laboratory situation. See Comstock, Rubinstein and Murray, eds. (1972). In an ingenious field experiment of the imitation of anti-social behavior seen on television, Milgram (1972) finds no effect among adolescents or adults.

* This paper was prepared for the Symposium on the Effects of the Mass Media of Communication at the XXth International Congress of Psychology, held in Tokyo in August 1972. The data are from a national survey on the use of leisure in Israel under the direction of Elihu Katz and Michael Gurevitch, commissioned by the Ministry of Education and Culture. Additional analysis in the area of mass communication was supported by a grant from the Israel Foundation Trustees, a subsidiary of the Ford Foundation. The authors acknowledge with thanks the assistance and advice offered by Hanna Adoni, Gila Brand, Leah Isaac and Oved Cohen, fellow members of the project team, and by Professor Louis Guttman and Mrs. Tsiyona Peled. Helpful comments were offered by Dr. Akira Fujitake, of the NHK Theoretical Research Center, who served as discussant at the Tokyo Symposium, and by Professors Jay G. Blumler and Herbert Menzel.

¹ The early studies are essentially based on lists

needs more readily than the media overpower them; that the media are at least as much agents of diversion and entertainment as of information and influence. It argues, moreover, that the selection of media and content, and the uses to which they are put, are considerably influenced by social role and psychological predisposition.

Viewing the media in this way permits one to ask not only how the media gratify and influence individuals but how and why they are differentially integrated into social institutions. Thus, if individuals select certain media, or certain types of content, in their roles as citizens, or consumers, or church members, we gain insight into the relationship between the attributes of the media (real or perceived) and the social and psychological functions which they serve. When Richard Crossman (1968) suggests, for example, that print is the medium most appropriate to democracy—referring to the preference of print for issues over personalities, and the relative ease with which a reader can detach himself emotionally from what he is reading—he is suggesting an hypothesis concerning the compatibility between the attributes of a medium and the social institution of politics. The same thing holds true in the realm of the family when Donald Bogue (1962) suggests that print is the medium appropriate to the dissemination of family planning information—referring to the durability of print and the privacy in which it is consumed. Jean Cazeneuve (1972) suggests that the attributes of television provide modern man with the means to satisfy his primitive needs for taboo, magic and religion. These are hypotheses, of course, and do not mean, necessarily, that people act in these ways; they are questions for empirical research.

These are the kinds of questions to which this paper is addressed. It reports, first, on the "needs" which different people consider important. It then clusters these needs both a priori and empirically. Next it assesses the relative contribution of the several mass media to gratifying each group of needs and asks whether persons differently "located" in society satisfy the same needs in different ways. Finally, it evaluates the contribution of the mass media to each type of need

relative to the gratifications obtained from other, non-media sources.

The "needs" selected come from a variety of institutional areas—politics, family, religion and education—and from the areas of self-identity, self-growth and self-gratification. The media examined are radio, television, newspapers, books and film; and some effort is made to distinguish among the gratifications provided by the *attributes*, the characteristic *content*, and the social and physical *contexts* with which each medium is typically associated.³ The population studied is a representative sample of 1,500 Israeli adults; but the intent of the study, substantively and methodologically, is to serve as a basis for extrapolation and cross-cultural comparison, hence its explicit self-consciousness about the assumptions and possible pitfalls in the procedures used.

Research Procedure

We began by assembling as comprehensive a list as possible of social and psychological needs said to be satisfied by exposure to the mass media. This list, taken from the literature, was supplemented by additional items, based on our own insights into the specific functions of the media in Israel. The list was then pilot-tested and subsequently reduced to thirty-five "need statements" of the form: "How important is it for you to spend time with your family?" or, "How important is it for you to understand the true quality of our leaders?" The list was

³ For example, Berelson's (1954) reference to the newspaper as a "tool for daily living" relates to content; while the "ritualistic and near-compulsive character" of newspaper reading appears to be an attribute. Similarly, radio provides "vicarious and identificatory participation in newsworthy happenings" on the one hand, and "companionship" on the other (Mendelsohn, 1964). While these functions are analytically distinguishable, it is not clear to what extent members of the audience conceive of the media separately from their characteristic content. One wonders whether different content would affect popular images of television as "escapist," newspapers as "informative" and books as providers of "culture"—functions which will be discussed below. We do not claim to have succeeded in separating these different aspects of the media in the questions which were addressed to respondents. The analysis, at a number of points, attempts to sort out the three types of functions, however.

put in an interview to the 1,500 respondents, and a three-pronged investigation was conducted for each of the "need statements."

An example will make the procedure clear: We asked, "How important is it for you to keep up with the way the government performs its functions?" Respondents who answered that this need was "very important" or "somewhat important" for them, were asked "How much does listening to the radio help you to keep up with the way the government performs its functions?" The same question was repeated five times—for radio listening, TV viewing, newspaper reading, book reading and movie going. (If a respondent said in the course of the interview that he was never exposed to a particular medium, he was not asked about the functions of that medium.) If the respondent's reply indicated that a given medium "does not help," interviewers were instructed to discover whether the medium simply does not help or whether it "hinders" satisfaction of the particular need. In this way we tried to take cognizance of the possible dysfunctions of each of the media as well. Finally, we asked, "Is there something else besides these media which helps you to keep up with the way the government performs its functions?" This last was an open-ended question.

In sum, our object was first to identify the felt needs of the population and subgroups within it. Then, for those respondents who answered that a given need was at least "somewhat important," we sought to identify the extent to which each of the five media functions to fulfill these needs.

Finally, we sought to compare the relative importance of the media with other means of fulfilling each need.

Facets for the Classification of Needs

As has been noted, the list of needs came from the (largely speculative) literature on the social and psychological functions of the mass media. After compiling the list, we attempted to classify it. The scheme, consisting of three facets, is presented in Table 1; while Table 2 codes each item in terms of the scheme.⁴

Classifying the thirty-five needs according to their resource and mode (Facets 1 and 2), we form what seem to us five meaningful groupings.

1. Needs related to strengthening information, knowledge, and understanding—these can be called cognitive needs;
2. Needs related to strengthening aesthetic, pleasurable and emotional experience—or affective needs;
3. Needs related to strengthening credibility, confidence, stability, and status—these combine both cognitive and affective ele-

⁴ The three facets taken together yield eighty-four possible combinations. The coding of the thirty-five needs by these three facets filled nineteen of the eighty-four three-dimensional boxes, with eight boxes containing two or more items. Thus, for example, the need "to understand what is going on in Israel and in the world" (item 1) is coded A1, B1, C4; the need to "learn how to behave among others" (item 23) is coded A3, B1, C3; the need to "escape from the reality of everyday life" (item 9) is coded A2, B4, C1. This a priori coding of all items is reported in the left-hand column of Table 2 with minor amendments, as given in footnote 7 below.

Table 1. Classification of Media-related Needs

A. Mode	B. Connection	C. Referent
1. To strengthen	1. Information, knowledge, understanding	1. Self
2. To weaken		2. Family
3. To acquire	2. Gratification, emotional experience	3. Friends
	3. Credibility, confidence, stability, status	4. State, society
	4. Contact	5. Tradition, culture
		6. World
		7. Others, negative reference groups

- ments and can be labeled integrative needs;
4. Needs related to strengthening contact with family, friends, and the world. These can also be seen as performing an integrative function;
 5. Needs related to escape or tension-release which we define in terms of the weakening of contact with self and one's social roles.

The third facet, the frame of reference, when added, skews the list heavily toward two "frames of reference"—the self and the socio-political collectivity. This, however, may not be accidental, since most functions served by the media for the audience member are related either to the self or his relations with his social environment and society. Nonetheless, it is unfortunate that the scheme was distilled only after the list was completed and the larger study, of which the present report is a part, had gone into the field. Otherwise, we might have attempted to experiment with the missing elements.

The Needs' Hierarchy of Importance

We turn now to the question of how important these needs are. Listing them by their relative importance to respondents yields roughly three groups based on percentage cutting points. The list is headed by the need "to feel pride that we have a state" (item 8), which was deemed "very important" by 90 percent of the sample. Indeed, the first group of eight items (8, 1, 14, 6, 35, 33, 26, 18) which were endorsed by 70 percent or more of the respondents is dominated by what might be described as "collectivity-oriented" needs, pertaining to the state, the nation (or the national tradition) and the family. Only two of the eight needs in this group—"to raise my morale" and "to feel that I am utilizing my time well"—can be described as personal.

Lowest on the list is the need to "escape everyday reality" (item 9), which was rated as "very important" by only 16 percent of the respondents. Those who know Israel will not be surprised to learn that Israelis are high on national pride, familialism and reality orientation; while the counterpart of these attitudes is a somewhat restrained—some would even say puritanical—attitude toward self-indulgence.

Background factors are, of course, related to the varying importance attributed to needs by respondents. Thus, the higher the level of education, the larger the number of needs rated as "very important." Of the thirty-five items on the list, twenty-seven won higher endorsements from respondents of middle and high education as compared with the lesser educated. Only two needs were found to be negatively correlated with education, i.e. a higher percentage of the least educated than of the others considered them "very important": these are the need to "get closer to Jewish tradition" (item 21) and to "strive for a higher standard of living" (item 32). Six other needs were related to level of education in a curvilinear way—respondents with medium (elementary) education rated them as "very important" more often than either the lower or the higher educated.

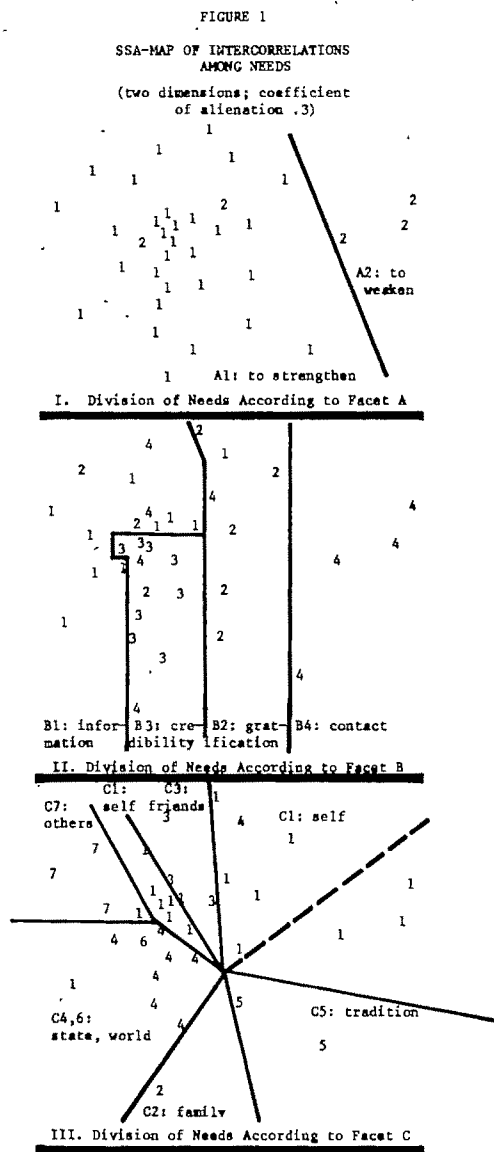
There are similar connections with age. For example, the need to "learn how to behave among others," or the need to "get to know the true qualities of our leaders" are negatively related to age; that is to say, younger people attribute greater importance to these and other socializing needs than do older people. The same thing holds true for the group of needs associated with aesthetic and emotional experience.

The Interrelationships among the Needs

Our next step beyond merely viewing their relative importance, was to examine the items' interrelationships, as they emerge from the data. Specifically, we wanted to see the extent to which items coded a priori as similar actually cluster empirically.

To this end we performed a factor analysis and also mapped the matrix of intercorrelations, using Guttman's method for smallest space analysis (SSA).⁵ The two-

⁵ Briefly, this method graphs the distance between all items in the matrix of intercorrelations as an inverse function of the size of the correlations; the higher the correlation between any two items, the smaller the distance between them in the map. A number of sources of distortion are possible; e.g. two items may fall close to one another, though they are not highly correlated, due to the similarity of their relationships with all other items. More basic is the problem of whether the matrix of correlation is well described in a two (or three) dimensional space. For details see Guttman, (1968). The facto-



dimensional mapping is barely adequate according to Guttman's criteria, and the results appear as Figure 1.

Close study of Figure 1 yields three observations:

1. Facet 1, the "mode," divides the map vertically, in that category A2, which denotes the "weakening" mode of media use and which includes items 4, 5, 7 and 9 (see Table 2), appears on the right hand side. Of this group of "escapist"

analysis—the starting point for which is the same matrix of intercorrelations used in the Smallest Space Analysis (SSA)—is not presented here. The results obtained correspond rather closely. Throughout, the correlations are monotonic.

items, only item 7 ("release tension"), is located at some distance and appears to differ from the others.⁶

2. Facet 2 also divides the map in roughly vertical chunks in that the left hand side of the map deals with cognitive matters (B1), and proceeds via the integrative concerns of B3 (credibility, confidence, stability, status) to affectivity (B2) and contact (B4).
3. But probably Facet 3, the frame of reference, constitutes the most important division of the map of intercorrelations. The spokes of a wheel divide the clustered frames of reference. Two of the clusters refer to self (C1). Viewed in conjunction with the other facets, the right-hand cluster appears to be a more affective self (B2-C1) and the left-hand cluster an integrative-cognitive one (B3-B1, C1). Going one step further, we can think of the "escapist" items (4, 5, 9) as representing the "id." In terms of the coding scheme, indeed, they might have been recorded as strengthening (A1), contact (B4) with one's *other* self rather than A2, B4, C1; and thus perhaps we could abandon the notion of "weakening" (and the first facet) altogether.

Viewing the map as a whole, and considering that the items dealing with self (especially "cognitive self") cluster near the center, we attempt in Figure 2 an idealized representation of the overall structure of interrelations among the thirty-five items.

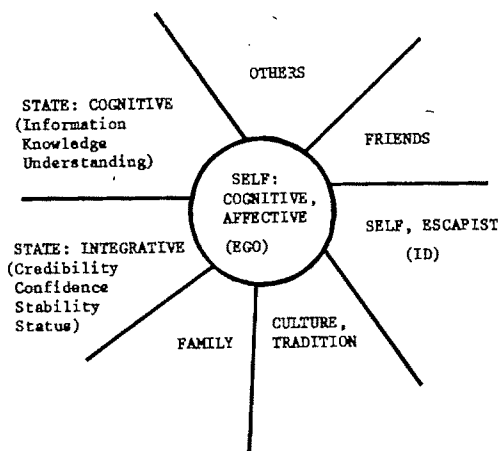
The Functions of the Media

Altogether, these representations of the empirical intercorrelations among the needs appear to support the a priori classification. The next step is to see whether the different clusters of needs are differently served by the mass media. Table 2 gives the rank order of "helpfulness" attributed to the five media for each of the thirty-five needs, now divided into fourteen clusters.⁷

⁶ It has therefore been reclassified for subsequent analysis; see the following footnote for details. This, taken together with the findings for the different media which serve these "escapist" needs (see Table 2 and discussion in text), raise further questions concerning the dimensionality of the concept of escape. Note, too, the large proportion of the population which affirms tension release as an important need, compared to the minorities agreeing to the other "escapist" needs (items 4, 5, 9).

⁷ Some minor revisions of typology are introduced here. First, we group category A3 ("to acquire") with category A1 ("to strengthen") to reduce the total number of different groupings. Second, we

FIGURE 2



Consider first the items concerned with self (C1). Here the media perform different functions depending on Facets A and B. Thus, for strengthening one's knowledge or understanding of self (A1, B1, C1) the key medium is the book, and the least important is the film. But the film is the key medium to enjoying oneself (A1, B2, C1) followed by television and the book. In this respect, the newspaper functions least well as a perceived source of enjoyment. As for the need for self-confidence, stability and self-esteem (A1, B3, C1), the newspaper is the most important medium followed by radio and television, books and films.⁸

have introduced "corrections" in our coding of three items based on the findings of the SSA and factor analyses. The first of these is in item 7, "to release tension." Originally coded as an "escapist" item (together with items 4, 5, 9) the empirical analysis places it as A1, B2, C1 (to strengthen gratification of self). Item 29, "to feel that I am not always right" has been displaced from A2, B3, C1 (to weaken stability of self) to the present A1, B1, C7 (to strengthen understanding of others). Similarly, item 24, "to participate in discussions with friends," was formerly A1, B4, C3 (to strengthen contact with friends) and is now A1, B1, C3 (to strengthen knowledge of friends).

⁸It will be recalled that questions about the media were put only to those who said that a given need was at least "somewhat important" to them, and that questions about each medium were put only to persons who had minimal accessibility to the particular medium. Thus, persons who did not view television (about 60 percent of the population owned sets at the time of the study; another 25 percent had regular access) were not asked to rate television's helpfulness in achieving any of the needs, just as questions about the helpfulness of

The striking thing about these findings is their consistency. The theoretical groupings of needs yield almost perfect regularities in the rank order of media helpfulness: Books cultivate the inner self; films and television give pleasure; and newspapers, more than any other medium, give self-confidence and stability. The latter finding seems best interpreted in association with the idea of stability and reinforcement: it gives the day its framework (item 19); it tells me that others think as I do (item 11); it helps me feel influential (item 30).

This consistency in mass media use is not evident in the "escapist" items (A2, B4, C1). Television is judged most helpful for killing time (item 4) followed closely by newspapers and books; films and radio are considered somewhat less helpful for killing time, though, on the whole the media seem to most almost equally useful for this end. The need "to overcome loneliness when I am alone at home" (item 5) obviously puts film at a disadvantage and may do the same for TV, because broadcasting is limited to a few evening hours when in most homes loneliness least prevails. But these two items are less frankly escapist than the need to "escape from reality" (item 9) which is best accomplished through books and films. Notice the similarity of this latter pattern to that of the media which serve the need "to release tension" (item 7), an item which seemed, a priori, "escapist" but found its way, empirically, to the group of items whose subject is self-gratification (A1, B2, C1). The dimensions of the concept of "escape" obviously require further clarification (Katz and Foulkes, 1962): These data suggest, at least, that managing time on one's hands is rather different from getting away from it all.

These data are also regular in the area of the individual's relationship with state and society. Regardless of whether it is one of understanding or pride, confidence or connectedness, the rank-order of media

books were put only to book readers (22 percent reported that they do not read books at all). In other words, the percentages in Table 2 are based on the total number exposed to each medium; and the base varies, therefore, from medium to medium. The media are then ranked by the percent which considered each medium "very helpful" in achieving the need in question.

helpfulness is uniform. Newspapers come first, followed by radio, then television. Books and film are far behind.

This pattern also holds for understanding others, perhaps particularly negative reference groups—Arabs, those who dis-

agree with government policy, and those who disagree with me (items 31, 28, 29, respectively). It will be recalled that strengthening one's own self-confidence and stability also fit this pattern in the sense that newspapers, radio and television came

Table 2. The Helpfulness of the Media in Satisfying "Important" Needs

Code	Needs	Percentage Claiming Need is "Very Important"	Ranking of Media by Percentage Claiming Medium Is "Very Helpful" for Satisfying Need*
A1,B1,C1	<u>Strengthen knowledge, understanding with self</u>		
	(34) To know myself	66	B (31) N (18) R (15) TV(14) C(13)
	(27) To develop good taste	61	B (49) TV(37) R (35) N (34) C(33)
	(22) To want to study	56	B (63) N (39) R (28) TV(27) C(10)
A1,B2,C1	<u>Strengthen gratification, experience with self</u>		
	(35) To raise my morale	77	C (59) TV(52) B (49) R (47) N(34)
	(13) To experience beauty	64	C (55) B (52) TV(48) R (30) N(23)
	(7) To release tension	61	C (51) B (47) TV(43) N (36) R(35)
	(3) To be entertained	54	C (61) TV(58) R (48) B (39) N(32)
	(16) To re-experience events in which I was involved	32	B (36) TV(29) R (27) C (26) N(26)
A1,B3,C1	<u>Strengthen credibility, stability, status with self</u>		
	(33) To feel that I am utilizing my time well	72	N (47) B (46) TV(30) R (30) C(24)
	(19) To order my day	63	N (19) R (17) TV(13) B (12) C(7)
	(32) To strive for a higher standard of living	54	N (33) TV(32) C (27) R (26) B(26)
	(30) To feel that I am influential	47	N (20) R (16) TV(15) B (15) C(9)
	(11) To feel that others think as I do	36	N (44) R (29) TV(27) B (24) C(14)
A2,B4,C1	<u>Weaken contact with self</u>		
	(5) To overcome loneliness when I am alone at home	37	N (63) B (62) R (62) TV(56) C(29)
	(4) To kill time	16	TV(53) N (52) B (50) C (47) R(44)
	(9) To escape from the reality of everyday life	16	B (45) C (44) TV(31) R (25) N(25)
A1,B4,C2	<u>Strengthen contact with family</u>		
	(14) To spend time with the family	85	TV(36) C (23) R (17) N (13) B (8)
A1,B1,C3	<u>Strengthen knowledge, information with friends</u>		
	(24) To participate in discussions with my friends	63	N (56) B (42) TV(37) R (37) C(24)
	(23) To learn how to behave among others	52	B (40) R (35) N (30) TV(29) C(25)
A1,B4,C3	<u>Strengthen contact with friends</u>		
	(10) To spend time with friends	55	C (31) TV(21) N (13) R (11) B(8)

A1,B1,C4	<u>Strengthen knowledge, information, understanding with society, State, world</u>																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																				</
----------	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	----

*B = Books; TV = Television; R = Radio; N = Newspaper; C = Cinema.

first. But note that for self-confidence, books play a more important role than they do in the political arena.

Connections with the family (A1, B4, C2) are best fulfilled by television, as expected, just as connections with friends

(A1, B4, C3) are best fulfilled by movies. Note that the need "to participate in discussions with my friends" (item 24) is served, first of all, by newspapers and then by books, with television and radio in third place. Here is a difference between medium

and message. It is the medium of film or television which contributes to friendship and familial solidarity (cf. Johnstone 1962), but the content of conversation is contributed by newspapers and books. It is more than a little surprising, one should add, to find that people do not give television as much credit as it intuitively seems to deserve as a topic of conversation.

Division of Labor Among Media

Summarizing from a different point of view, we note that the newspaper comes first in satisfying nineteen of the thirty-five needs. It is the most helpful in fulfilling the needs to strengthen both information about and confidence in society. At the same time it satisfies such personal needs as overcoming loneliness or strengthening stability and confidence in the state. It should be noted, however, that the newspaper's primacy may at least in part be a function of the specific socio-political bias of our list. On the other hand, as we have argued, perhaps it truly reflects the range of functions which the newspaper performs (cf. Edelstein, 1972), especially in a country like Israel.

The importance of the printed media for Israelis is demonstrated by the fact that the book is the medium second best able to satisfy needs. Its main uses are to satisfy such cognitive and affective personal needs as to "develop good taste," to "know myself," to "want to study," and to "re-experience events in which I was involved."⁹ At the same time books were deemed helpful in escaping "from the reality of everyday life" and getting "closer to Jewish tradition."

Despite its rapid diffusion and popularity during the time of the study, television turned out to be "most helpful" for three needs only; "to kill time," "to spend time with the family" and "to be in a festive mood" (which, as we have seen, has also been perceived as a family-oriented need). The discrepancy between the attention lavished on this medium and its rather limited "uses" may again be the result of the struc-

ture of our list, or the fact that exposure to television has not yet assumed a functional importance commensurate with its central position in the leisure time of the Israeli population.

A similar phenomenon occurs with radio. Despite its almost total penetration, not a single need on the list was best served by radio. It came second in serving self-integrative needs, and cognitive and integrative needs related to state and society.

Finally, and not surprisingly, going to the movies was found to serve such personal affective needs as "to be entertained," "to raise my morale," and "to release tension," and the social need "to spend time with friends."

The Effect of Education on Uses of the Media

Since we know that the various needs differ in importance to persons of different levels of education, it is imperative to ask whether educational level is also associated with different patterns of media use. Thus, even though our analysis of media use relates, in each case, only to those persons for whom a given need is important, it may well be that persons of different levels of education rank the helpfulness of the media differently with respect to the same need.

Analysis of media preferences by educational level indicates that the printed media—the book and the newspaper—assume increased importance with increased education, and the electronic media decline in importance. The reverse is true for the lesser educated, who find the electronic and visual media relatively more helpful for satisfying needs. Television, especially, satisfies not only personal-affective needs, as we have seen, but such cognitive needs as the need for information about society and the world, and the wish to study. Television, in that sense, is an "easy way of reading."

More important for our purpose, however, is whether the *ranking* of the relative helpfulness of the media with respect to a given need varies by educational level. Here we find far less difference among the educational groups. In six of the fourteen clusters of needs, virtually identical rankings were given by the three educational groups; for

⁹ This function was probably reinforced by the fact that during the period in which the field work was carried out the Israeli book market was flooded with books reporting the Six Day War.

example, in needs having to do with understanding, or feeling confident in matters of state, the ranking of the media for all groups was newspaper, radio, television, books and films. For five additional need clusters, the three educational groups ranked the media similarly. The major difference among groups with respect to these latter clusters and the three remaining for which there was a low level of concordance, reflects the differing importance assigned to television and books, as has already been noted. Television is decreasingly helpful with increased education, while books are increasingly helpful. A clear example of this can be seen in the cluster of needs defined as "strengthen[ing] gratification, [and] experience of self," as Table 3 reveals.

The table shows clearly how television moves down and the book moves up the educational ladder. But while this represents an important trend, the rule, as we have noted, is for similarities of rankings to outnumber the differences. In other words, the individuals who indicate that a given need is important to them tend to evaluate the relative helpfulness of the several media in much the same way, regardless of their educational level.

SSA Again

Having established which media are good for what purposes, we wish to look again at an SSA mapping, this time to examine the extent to which persons who name a medium in response to a given need tend to name the same medium in response to another, or conversely, whether people tend to be so highly selective in associating media and needs that low intercorrelations occur with respect to the usefulness of the same medium for different needs. We may find needs equally well served by different media and

needs for which one medium but not another are deemed useful.

To answer these questions, we selected eight items from the list of needs—four each from the two polar extremes of the matrix of intercorrelations which formed the basis for Figure 1. Four of these items relate to strengthening information and contact with state and society (items 1, 15, 25, 26: see Table 2), and four relate to self indulgence (items 3, 4, 5, 7). We intentionally chose these two groups since they represent the two major functions performed by the media—connection with society, and gratification of self—and because they give equal weight to the two sets of functions. Had we taken all thirty-five needs—which, as has been noted, may unduly weight the socio-political area—the analysis we are about to undertake would have been biased by the roles ascribed to the media in the socio-political area.

Accordingly, we constructed a matrix of intercorrelations based on the evaluations of the usefulness of each of the five media for each of the eight items. Plotting these intercorrelations by means of the SSA technique (Figure 3) we can now turn to see how the map is organized: If media preferences predominate—that is, if a person who says books help him "to get to know the true qualities of our leaders" also tends to say that books help him "to escape from the reality of everyday life"—then we shall find clusters of correlations organized by media. If, on the other hand, we find that a given need, when it is considered important, is served equally well by all media, we shall find needs at the center of correlational clusters.

The map shows both patterns, although one is somewhat stronger than the other. The first pattern shows each medium with a distinct area of its own in which evalua-

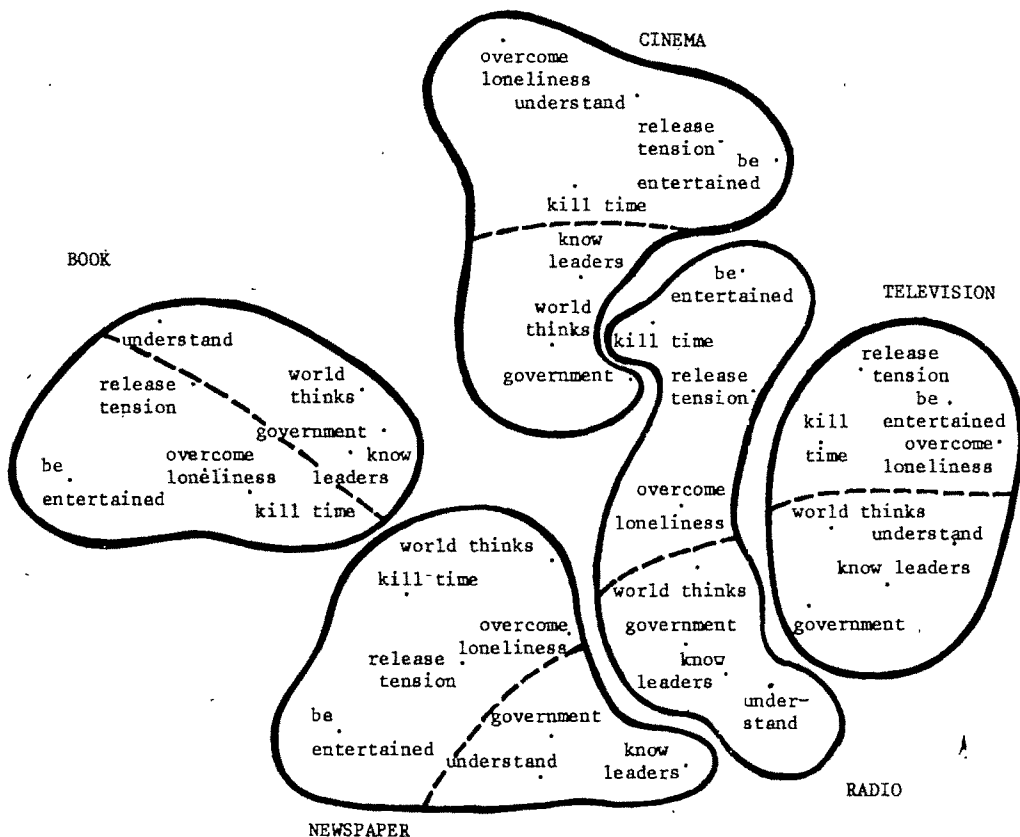
Table 3. Ranking of the Helpfulness of the Media in "Strengthening Gratification, Experience with Self" According to Education

Low Education	Television	Films	Radio	Newspaper	Book
Medium Education	Films	Television	Book	Radio	Newspaper
High Education	Book	Films	Television	Radio	Newspaper

FIGURE 3

SSA-I MAP OF THE MEDIA'S HELPFULNESS FOR SATISFYING 8 NEEDS

(two dimensions; coefficient of alienation .3)



tions of the usefulness of that medium for all eight needs clearly cluster. Consider books, for example. Respondents who say that books are very helpful (or not very helpful) for killing time also say that books help (or don't help) in understanding leaders. Moreover, the correlations among the declared usefulness of books over the eight different needs tend to be higher than the correlation among books and the other media fulfilling the same need. This is what makes for the relative isolation of each medium from the others. Some are more isolated: books and television, are the best examples; the others tend to spill over into each other rather more.

But there is a second pattern in this map, too, albeit less visible. Each of the spaces

which enclose a medium can be subdivided into two. Half the space, approximately, groups the political items; the other half groups the personal items. In other words, within each cluster which forms about a given medium, the content areas form sub-clusters, showing that those who say that television is useful for getting to know one's leaders, for example, are more likely to say that it is also useful for understanding events at home and abroad than that it is useful, say, for reducing tension.

As soon as one notices this fact, another becomes apparent. The political uses of radio, television, and newspapers border on each other. The political uses of films and books are contiguous with each other, but not with those of radio, television, and news-

Table 4. Average Correlation of Helpfulness of Each Medium for All Pairs of Eight Needs: How "Specialized" Are the Media?

Television	.66
Books	.53
Radio	.52
Cinema	.43
Newspaper	.42

papers. The personal uses of radio and television are adjacent. These patterns suggest that (1) both political and personal uses of radio and television are closely allied; (2) the uses of newspapers for political needs are related to the political uses of radio and television, but the personal uses of the newspaper are rather different; (3) books and films are akin in their political uses (or nonuses), but function differently for the self.

Further information can be obtained from the map (or, more exactly, from the matrix of intercorrelations on which the map is based) by averaging the correlations of the helpfulness of each medium for all pairs of needs (twenty-eight in all) and comparing the averages thus obtained.¹⁰ A high average indicates that the medium is more diffuse—that is, that it is considered useful for satisfying both socio-political and personal needs. A low average indicates the

¹⁰ In this and the next table, means were preferred to other measures of central tendency precisely because they permit extreme cases to exert disproportionate influence.

"specificity" of the medium, that is, its relevance to a limited and rather homogeneous set of functions. The averaging process yields the figures in Table 4.

These data suggest that television is the most diffuse medium, that is, its users apply it to a wide range of functions; whereas movies and newspapers are the most "specific" media. Books and radio fall in between. These findings also seem to confirm some aspects of popular images of these media, such as the almost universal attractiveness of television for television fans, and the more specific informational function of the newspaper.

Next, we looked at the interchangeability among the media. In other words, we examined the degree to which pairs of media perform similar functions, and therefore function as alternatives. This "index of interchangeability" was computed by averaging the correlations which signify the extent to which media pairs perform similarly for all eight needs. Table 5 presents these averages in matrix form.

The data of Table 5 reveal that television and radio are highly interchangeable, while television and books are least so. In other words, people who say that television is helpful (or not) for each need, are very likely to say that radio is helpful (or not) for the same needs. No such shadowing appears for television and books.

Next to radio, the best substitute for television is the cinema (.57). The visual entertainment function which both media serve apparently leads to this moderate degree of interchangeability. The documentary function of television also makes it somewhat interchangeable with newspapers.

Table 5. Matrix of Average Correlations of Helpfulness of Each Pair of Media for All Eight Needs: Which Media Are Interchangeable with Which?

Media	Television	Radio	Newspapers	Books	Cinema
Television	--	.71	.53	.26	.57
Radio		--	.69	.38	.49
Newspapers			--	.53	.37
Books				--	.51
Cinema					--

This analysis of the interchangeability of television and the other four media provides a key to reading the matrix as a circumplex which shows each medium to be most interchangeable with its two closest neighbors.¹¹ Thus, radio's best substitutes are television, on the one hand, and newspapers on the other. The newspaper's best substitutes are radio and books. Books are most interchangeable with newspapers and cinema, while the best substitutes for cinema are books and television—although it should be noted that books and cinema are more insulated than the other three media.

These relationships among the media appear to reflect the complex ways the media overlap. Thus, the media are classifiable first by their mode of transmission—print versus electronics. Second, they divide according to their mode of reception: television and cinema are received by watching and listening, radio by listening only, newspapers and books by reading. Third, as we have seen, the media differ in the range of their content (documentary, entertainment or both) and consequently in the needs they serve. Thus, for example, books share the element of print with newspapers and the element of entertainment with the cinema. Radio shares both range of content and mode of transmission with television and the documentary coverage of the newspaper.

That television and newspapers are radio's best companions is of interest in the light of the findings reported in Table 6 which showed that people name television as helpful (or not) for various needs but that their use of the newspaper is more restricted. It is remarkable that radio is the best substitute for both the most specialized (television) and the least specialized (newspapers) media. Respondents apparently see it both as a substitute for the versatility of television and as the medium that comes closest to fulfilling the newspaper's documentary role.

What Else Is Helpful?

Although the needs we began with were chosen for their presumed relationship to

the mass media, it is still an empirical question whether they are also served by other means, and whether the mass media or these other means are more helpful. In other words, even for what we have described as media-related needs, functional alternatives to the media surely exist. To find out what these might be we asked, what, if anything, the respondent found more helpful than the mass media for satisfying each need. Our question was open-ended, and the list of "functional alternatives" was based on the respondents' answers. It totalled seventeen items, ranging from family and friends to sleep and drugs.

Table 6 presents the findings. Perhaps the first thing to notice is that though the list includes only media-related needs, the media served no single need exclusively. Indeed, only two items mobilized as many as 40 percent of respondents to claim that "nothing else besides the mass media" helps. In other words, even here other sources of gratification taken together equal the mass media in importance. Thus, even media-related needs must be viewed in the larger context of human needs of which they form a small segment, and against the variety of means by which these needs can be and are, satisfied.

Viewing the table again we note that, relatively speaking, the highest percent of endorsements for the mass media as "most helpful" for satisfying a need goes to the group of socio-politically-related needs, both on the cognitive level (strengthening knowledge, information, understanding) and on the integrative level (strengthening confidence and stability). Thus, for example, of those who described as important the need to "know what the world thinks about us," 43 percent claimed that nothing else besides the media helps satisfy that need. A slightly lower, though still quite high vote for the media was given for such socio-political needs as to "have confidence in our leaders," "feel satisfied with the way of life in Israel as compared with other countries," "feel that I am participating in current events" and "get to know the true qualities of our leaders." The dominant role of the media in overcoming distance is obvious too. It helps people to understand op-

¹¹ The circumplex is also discernible in Figure 3 where the five media fall into the same circular order.

Table 6. Primary Sources of Need Satisfaction

Code	Needs	Is There Anything that Helps More Than the Mass Media? (Open-ended Question)			
		No, Percentage Saying Nothing Helps More than Media	Yes, Percentage Saying Something Helps More (most frequent mentions)*	Yes, Percentage Saying Something Helps More (other mentions)**	Percentage Don't Know or "Nothing Helps"
A1,B1,C1	<u>Strengthen knowledge, understanding with self</u>				
	(34) To know myself	18	Friends 26 Family 15	20	21
	(27) To develop good taste	26	Activi- 20 ties Friends 12	22	20
	(22) To want to study	23	Lecture 31 Work 8	19	19
A1,B2,C1	<u>Strengthen gratification, experience with self</u>				
	(35) To raise my morale	15	Friends 23 Family 18	34	8
	(13) To experience beauty	20	Activi- 26 ties Hobbies 9	30	15
	(7) To release tension	15	Friends 18 Sport 11	39	10
	(3) To be entertained	16	Friends 27 Activi- 17 ties	33	7
	(16) To re-experience events in which I was involved	21	Friends 36 Family 11	14	18
A1,B3,C1	<u>Strengthen credibility, stability, status with self</u>				
	(33) To feel that I am utilizing my time well	23	Work 15 Family 10	34	18
	(19) To order my day	24	Work 20	26	30
	(32) To strive for a higher standard of living	25	Work 15 Friends 15	19	26
	(30) To feel that I am influential	15	Friends 32 Family 18	17	18
	(11) To feel that others think as I do	23	Friends 44 Family 8	8	17
A2,B4,C1	<u>Weaken contact with self</u>				
	(5) To overcome loneliness when I am alone at home	25	Hobbies 15 Friends 12	39	16
	(4) To kill time	17	Friends 16 Hobbies 10	33	16
	(9) To escape from the reality of everyday life	25	Friends 15 Sport 7	28	18

continuation of Table 6

A1,B4,C2 <u>Strengthen contact with family</u>					
(14) To spend time with the family	16	Family 42 Holiday 11	17	14	
A1,B1,C3 <u>Strengthen knowledge, information with friends</u>					
(24) To participate in discussions with my friends	29	Friends 27 Activities 6	17	21	
(23) To learn how to behave among others	25	Friends 34 Family 5	16	20	
A1,B4,C3 <u>Strengthen contact with friends</u>					
(10) To spend time with friends	15	Friends 46 Activities 7	18	14	
A1,B1,C4 <u>Strengthen knowledge, information, understanding with society, State, world</u>					
(1) To understand what goes on in Israel and in the world	36	Friends 27 Lecture 11	12	14	
(26) To know what the world thinks about us	43	Friends 18 Lecture 15	6	18	
(25) To keep up with the way the government performs its functions	41	Lecture 20 Friends 18	7	14	
(15) To get to know the true qualities of our leaders	35	Lecture 26 Friends 14	7	18	
(2) To obtain useful information for daily life	33	Friends 31 Family 7	10	19	
A1,B3,C4 <u>Strengthen credibility, stability, status with society, State</u>					
(8) To feel pride that we have a State	25	Holiday 33 Friends 7	19	16	
(6) To have confidence in our leaders	36	Lecture 20 Friends 16	11	17	
(12) To feel satisfied with the way of life in Israel as compared with other countries	30	Friends 16 Lectures 6	22	26	
A1,B4,C4 <u>Strengthen contact with society, State</u>					
(17) To feel that I am participating in current events	35	Friends 19 Holidays 7	19	20	
A1,B2,C5 <u>Strengthen experience with culture, tradition</u>					
(18) To be in a festive mood	12	Holiday 46	31	11	
A1,B4,C5 <u>Strengthen contact with culture, tradition</u>					
(21) To get closer to Jewish tradition	15	Holiday 49 Prayer 11	12	13	

continuation of Table 6

A1,B1,C7 Strengthen knowledge, in-
formation, understanding
of others

(28) To understand those who disagree with government policy	35	Friends 27 Lecture 18	4	16
(29) To feel that I am not always right	19	Friends 42 Family 13	10	16
(31) To understand how the Arabs feel	33	Lecture 23 Friends 17	9	18

A1,B2,C7 Strengthen gratification,
experience with others

(20) To participate in the experiences of other people	22	Friends 43 Family 10	11	14
--	----	-------------------------	----	----

* "Activities" refers to cultural activities of all kinds (theater, cinema, etc.); "Holidays" includes religious and/or national holidays. Item 18 (A1,B4,C5), "to be in a festive mood," is served equally by religious holidays (24%) and national holidays (22%). Religious holidays exclusively are named for item 21 (A1,B4,C5), "to get closer to Jewish tradition."

** Detailed percentages are given for the two most predominant "functional alternatives." The rest, with lower percentage, have been lumped together as "other."

position groups (that is, "understand how the Arabs feel" and "understand those who disagree with government policy"). What is surprising, perhaps, is how often interpersonal communication (friends and lectures) competes with the mass media even here.

For personal needs, the role of the media declines; and face to face contacts, primarily with friends, become more salient. This is especially true of such needs as "to know myself," "to be entertained," "to raise my morale," "to feel that others think as I do" and "to feel that I am influential." Friends are thus the main support for personal confidence and security, while they are also the best outlet for tension and source for "learn[ing] how to behave among others." It is interesting to note that for most of these needs the family, though still mentioned often, lags far behind "friends."

Public lectures are very important in satisfying socio-political needs. They seem a fairly popular substitute for functions commonly attributed to the mass media. Likewise, national and religious holidays serve such integrative functions as the need "to feel pride that we have a state," "to be in a festive mood" or "to get closer to Jewish tradition."

Conclusions

The mass communication media are thought to satisfy a variety of needs arising from social roles and psychological dispositions. These needs, typically, take the form of (1) strengthening or weakening, (2) a connection—cognitive, affective, integrative (3) with some referent—self, friends, family and tradition, social and political institutions, others. A group of needs selected for study was found to cluster empirically in these terms.

The object of the study was to identify the uses made of the media in gratifying these clusters of social and psychological needs. The aim, ultimately, is to explore the relationships between the attributes of the media and the functions they serve.

Methodologically, the study rests on the assumption that people are aware of their needs and able to identify their sources of satisfaction. Respondents were asked, in an interview, to indicate the extent to which each of five media helped in gratifying each of the needs in question, and to assess the helpfulness of the media relative to other sources of need-satisfaction.

Substantively, the principal findings are as follows:

1. For all needs examined, the non-media sources (combined) were deemed more gratifying than the mass media. Friends, holidays, lectures and work were often said to be more important sources of gratification.
2. The greater the "distance" from a referent—social, physical or psychological—the more important the role of the media. Yet, interpersonal communication—formal and informal—competes even in areas relating to political leadership and negative reference groups.
3. Certain comparative processes—such as striving for a higher standard of living, or satisfying oneself that one's time is well spent or that one's country is a good place to live in—seem well served by the media. So are "escapist" needs. On the whole, however, friends are more important than the mass media for needs having to do with self-gratification, even the need "to be entertained!"
4. For individuals who say that matters of state and society are important to them, the rank-order of media usefulness in serving these needs is entirely consistent, regardless of the respondent's educational level. Newspapers are the most important medium, followed by radio, then television. Books and films fall far behind. Altogether, the centrality of the newspaper for knowledge and integration in the socio-political arena cannot be overstated.
5. Needs having to do with self are associated with different kinds of media, depending on the specific functions involved. Knowing oneself is best served by books; enjoying oneself is associated with films, television and books; while the newspaper contributes to self-regulation and self-confidence. The individualized character of book-reading, the social character of film and television viewing, and the simultaneity of exposure to the newspaper apparently link these media with the needs they serve best.
6. In satisfying needs associated with self, books are more helpful for the better educated; while television is more helpful for the lesser educated. Particularly in the area of self-gratification, books and television exchange places as educational level increases.
7. Television is useful for killing time, but not as a medium of "escape." Its presence at the hearth apparently prevents it from becoming an insulating agent against the demands of the ego and of others.
8. Film and television, respectively, help maintain friendship and family solidarity. However, the topics of conversation

which these relationships engender are provided by newspapers and books.

9. Television is the least specialized of the media: persons who say that it is helpful for one set of needs tend to say that it is helpful for the other. The cinema and newspaper are most specialized in this sense: the one serves self-gratification and sociability, the other, participation in the socio-political order.
10. An examination of the media for their "interchangeability"—that is, for the extent to which they serve similar functions—reveals a circular relationship whereby each medium (as a point on the circle) is most similar to its two nearest neighbors. The circle goes from television to radio, newspapers, books, and cinema, back to television. These overlapping functions may be explained in terms of shared technical attributes, overlapping content, and the social contexts in which the media are consumed.

Finally, it should be noted that media-related needs are not, by and large, generated by the media. Most predate the emergence of the media and, properly, ought to be viewed within the wider range of human needs. As such, they have always been, and remain, satisfied in a variety of ways, most quite unrelated to the mass media. The surprising thing is to realize the extent and range of the media's encroachment on the "older" ways of satisfying social and psychological needs.

REFERENCES

- Berelson, Bernard
 1954 "What 'missing the newspaper' means." Pp. 36-47 in Wilbur Schramm (ed.), *The Process and Effects of Mass Communication*. Urbana: University of Illinois Press.
- Blumler, Jay G. and Denis McQuail
 1968 *Television in Politics: Its Uses and Influence*. London: Faber and Faber.
- Bogue, Donald
 1962 "Some tentative recommendations for a 'sociologically correct' family planning communication and motivation program in India." Pp. 503-38 in C. Kiser (ed.), *Research in Family Planning*. Princeton University Press.
- British Broadcasting Corporation, Audience Research Department
 1972 *Violence in Television. Part II: Studies of the Functions Served for Viewers by Selected Programmes Containing Violent Sequences*. London: British Broadcasting Corporation.

- Cantril, Hadley
1942 "Professor quiz: a gratification study." In P. F. Lazarsfeld and Frank Stanton (eds.), *Radio Research* 1941. New York: Duell, Sloan and Pearce.
- Cazeneuve, Jean
1972 *La Société de l'Ubiquité*. Paris: Denoel.
- Clarke, Peter and Lee Ruggles
1970 "Preferences among news media for coverage of public affairs." *Journalism Quarterly* 47:464-71.
- Comstock, G. A., E. A. Rubinstein and J. P. Murray (eds.)
1972 *Television and Social Behavior* (5 vols.). Washington: U.S. Government Printing Office.
- Crossman, R. S.
1968 "The politics of viewing." *New Statesman* 76 (July-December):525-30.
- Edelstein, Alex
1972 "An alternative approach to the study of source effects as mass communication." Paper presented at the XX International Congress of Psychology, Tokyo (mimeo).
- Emmet, Brian P.
1968 "A new role for research in broadcasting." *Public Opinion Quarterly* 32:654-65.
- Escarpié, R.
1966 *Le Livre et le Conscrit*. Bordeaux: Université de Bordeaux.
- Guttman, Louis
1968 "A general nonmetric technique for finding the smallest coordinate space for a configuration of points." *Psychometrika* 33:469-506.
- Herzog, Herta
1954 "Motivations and gratifications of daily serial listeners." Pp. 50-5 in Wilbur Schramm (ed.), *The Process and Effects of Mass Communication*. Urbana: University of Illinois Press.
- Johnstone, John W. C.
1961 *Social Structure and Patterns of Mass Media Consumption*. Unpublished Ph.D. Dissertation, Department of Sociology, University of Chicago.
- Katz, Elihu and David Foulkes
1962 "On the use of mass media as escape: Clarification of a concept." *Public Opinion Quarterly* 26:377-88.
- Lundberg, D. and O. Hulten
1968 *Individen och mass media*. Stockholm: Norstedt & Soner.
- McQuail, Denis, Jay G. Blumler and J. R. Brown
1972 "The television audience: a revised perspective." Pp. 135-65 in D. McQuail (ed.), *Sociology of Mass Communications*. London: Penguin Books.
- Mendelsohn, Harold
1964 "Listening to radio." Pp. 240-8 in Lewis A. Dexter and David M. White (eds.), *People Society and Mass Communication*. New York: The Free Press.
- Mendelsohn, Harold
1966 *Mass Entertainment*. New Haven: College and University Press.
- Milgram, Stanley and R. Lance Shotland
1972 *Television and Anti-Social Behavior: Field Experiments*. New York: The City University (mimeo).
- Nordenstreng, Kaarle
1969 "Consumption of mass media in Finland." *Gazette* 15:249-60.
- Riley, Matilda W. and John W. Riley, Jr.
1951 "A sociological approach to communications research." *Public Opinion Quarterly* 15:440-60.
- Robinson, John
1972 "Toward defining the functions of television." Pp. 568-603 in Rubinstein, Comstock and Murray (eds.), *Television and Social Behavior*, Vol. 4. Washington: U.S. Government Printing Office.
- Rosengren, Karl Eric and Swen Windahl
1972 "Mass media as a functional alternative." Pp. 166-94 in D. McQuail (ed.), *Sociology of Mass Communications*. London: Penguin Books.
- Stephenson, William
1966 *The Play Theory of Mass Communication*. Chicago: University of Chicago Press.
- Waples, Douglas, Bernard Berelson and Franklyn R. Bradshaw
1940 *What Reading Does to People*. Chicago: University of Chicago Press.
- Warner, Lloyd and William Henry
1948 "The radio daytime serial: A symbolic analysis." *Genetic Psychology Monographs* 37:7-69.
- Wright, Charles
1959 *Mass Communication: A Sociological Perspective*. New York: Random House.

TECHNOLOGY AND HOUSEHOLD CONFIGURATION IN URBAN AFRICA: THE BASSA OF MONROVIA¹

W. PENN HANDWERKER

California State University, Humboldt

American Sociological Review 1973, Vol. 38 (April):182-197

The interrelatedness of technology and social structure has intrigued investigators for more than a century. Of particular interest has been the apparent sufficiency of different technologies for different residential and kinship structures. A distinct line of research on this issue may be traced from the early 1900's. The thread drawing together such studies is the long-suspected curvilinear relation between the complexity of the technological system and the complexity of the familial system, a proposition recently receiving empirical support. However, a trend toward nuclear family organization has not become apparent as African populations moving to urban centers have become enmeshed with industrial technologies and money economies. Rather, we are confronted with the simultaneous existence of an often bewildering array of household and kinship configurations. This paper discusses intertwining changes in technology and social structure among migrant Bassa in Monrovia, Liberia, to illustrate adjustments to industrial technologies which produce varied patterns of household and kinship in urban Africa. Analysis of these adjustments suggests why discrepancies exist between cross-cultural generalizations and phenomena of the contemporary world. It further suggests the primacy of one link between technology and household and kinship structure, and a set of associated variables systematic consideration of which should help clarify the intertwining of these phenomena.

INTRODUCTION

THIS paper discusses intertwining changes in technology and social structure among migrant Bassa in Monrovia, Liberia. The interrelatedness of many aspects of technology and social structure has intrigued investigators for more than a century. Of particular interest has been the apparent sufficiency of different technologies for different residential and kinship structures, and a distinct line of research on this issue may be traced from the early 1900's. Anthropologists from Lowie (1920) through Driver (1956) and Aberle (1961) have been concerned with the relation between technology and residential and kinship structures among non-industrialized societies. Among sociologists,

Goode (1963), notably, has been concerned with what happens to these structures with industrialization. The thread drawing together what often are non-comparable studies, focusing on different variables and defining ostensibly equivalent variables differently, is the long-suspected curvilinear relation between the complexity of technological system and the complexity of familial systems, a proposition for which Blumberg and Winch (1972) recently have provided empirical support. One would expect to find independent nuclear family households where people have become enmeshed with an industrial system. However, changes in the direction of nuclear family organization predicted from past cross-cultural research have not become obvious trends as African populations moving to urban centers have become enmeshed with industrial technologies and money economies. Lineality—including strengthened matrilineal ties (e.g., Colson, 1962; Baker and Bird, 1959)—descent groups, and extended family households have remained important features of urban social structure (Aldous, 1962; Comhaire, 1956). Indeed, a salient characteristic of data on household and kinship structures throughout the contemporary world is the absence of

¹ The fieldwork on which this paper is based was sponsored by funds from the Ford Foundation and by a Pre-Doctoral Fellowship (#1-FOI-MH44672-01) and Research Grant (#MH 12095-01) from the National Institute of Mental Health. This paper and Handwerker (1972b) are offshoots of a paper entitled "Causality and Household Organization in Urban Africa" (1972a) delivered to the Northwest Anthropological Conference in Portland, March, 1972. The position taken in this paper was foreshadowed by Munsell (1967). A special acknowledgement is due Professor Vernon R. Dorjahn for patiently reading and criticizing many earlier drafts.

these predicted trends. Reviewing the conventional view that industrialization has destructive effects on extended kinship organization, for instance, Hendrickson (1970: 32-48) concluded that

... the studies we have looked at make two major points: nuclear family patterns existed prior to industrialization and extended family patterns continue to exist after industrialization, i.e., there is no one family organization that is the most optimal or compatible with machine technology and densely populated urban areas (1970:48).

Although at first sight some data appear to support predicted trends (e.g., Blumberg and Winch, 1972:914-17), the fact that in African urban areas we are confronted with the simultaneous existence of an often bewildering array of household and kinship configurations—including "traditional," informal, and civil and church marriage procedures, each with distinct sets of attitudes and obligations, and households consisting of nuclear families, a variety of kinds of extended families, single individuals, parent-child structures and other groups of consanguines (Handwerker, 1972b)²—seriously questions the viability of interpretations thus far given correlations derived from cross-cultural research.

These and other anomalous data raise four questions. Do differences in technology tend to be sufficient or necessary for differences in residential and kinship structure? If there are meaningful relations between these phenomena, what aspects of each are involved and in what way? To what extent is it possible to generalize these relations for African urban areas? Finally, to what extent do the data from urban Africa represent changes, and precisely what elements have changed and in what ways? The issues implied by these questions are too broad to be dealt with adequately in this paper. The case-study discussed here addresses itself explicitly only to the first two questions and only hints at answers to the second two. Suffice it

to note that the accuracy of the observation that differences in technology imply differences in residential and kinship structure is only partly at issue. It has been possible to formulate a set of empirical and theoretical generalizations in which much of the observable complexity of these structures in African urban areas may be inferred from properties of industrial systems (Handwerker, 1972b). More directly at issue is the conceptualization both of these phenomena and of their interrelatedness. The data from Monrovia illustrate adjustments to industrial technologies producing varied configurations of household and kinship in African urban areas. Analysis of these adjustments suggests the primacy of one link between technology and household and kinship, and a set of associated variables systematic consideration of which should help clarify the intertwining of these phenomena.

METHODOLOGY

Data used in this paper were gathered during fifteen months field research in Liberia between September 1968 and September 1970, in widely varying situations and through a variety of procedures. Figures cited in the text and tables derive from a sample of 140 Bassa households in Monrovia. Data collected through participant-observation and informal and intensive interviews were used to formulate question schedules and interpret the results. Informal interviews in the form of conversations were conducted regularly in the course of living in Liberia. Intensive interviews with selected informants guided by question schedules probing specific phenomena (e.g., kinship structure, political organization, economic activities and attitudes) were undertaken as appropriate during the course of research. A survey of Bassa households in Monrovia was undertaken separately to make possible an objective assessment of this particular phenomenon. Survey interviews were made by Liberian research assistants trained during pretests of the several different interview schedules used in my research (see Handwerker, 1971:12-29 for a fuller statement of the work undertaken in Liberia and the procedures followed.) In the absence of aerial photographs, detailed maps of the city, or

² Although these points are apparent in the good reviews of social organization in African urban areas (e.g., Mair, 1969), Handwerker (1972b) systematically tabulates reports of these phenomena from monographs and papers written from the 1930's through the 1960's and suggests a series of theoretical and empirical generalizations explaining selected aspects of African urban household and kinship structure.

house numbers, selection of household units for the survey was determined by randomly initiated systematic sampling of every third household in three of the areas within Monrovia in which there were strong concentrations of Bassa. Lack of funds and time precluded extending the survey to a number of equally appropriate areas. In all cases the respondent to survey questions was the acknowledged household head. Because most people in Monrovia live in rooming houses, residential units themselves were determined both by common residence and acknowledged clusters of interdependent people. Interviews in houses containing more than one Bassa household were conducted according to the spatial sequence of residential units; the unit closest to the front of the house was counted as number one. Interviews were initiated by an explanation to each potential respondent of the general purpose of the survey. To assure anonymity, the names of respondents were not recorded. When approached for an interview the respondent tended either to cooperate fully or actively (sometimes insisting that his name be recorded) or—not wishing to be bothered—to refuse all cooperation. Where an initial explanation did not suffice, a potential respondent was skipped. Substitutions were made by selecting the next household. The sampled population consisted of approximately five hundred households. Interviews were conducted in Bassa and English, whichever was appropriate for a particular respondent. Re-interviews checking the reliability of the data were not undertaken. A respondent's remarks do vary, sometimes widely on matters pertaining to age, dates, frequency, amounts, and so forth. However, the consistency which emerged with repeated questioning and informal conversation with individuals was borne out well in the patterns exhibited by the full set of data.

HISTORICAL AND ETHNOGRAPHIC BACKGROUND

The Bassa are a Kru-speaking, patrilineal group of about 166,000 living on the central coast of Liberia. Prior to 1600, the Bassa were hunters and gatherers. In 1970 they were swidden cultivators of rice and cassava. Although the Bassa are the second largest tribe

in Liberia, population density always has been very light; and in 1970 rural dwellers were sparsely distributed in villages averaging only fifty to a hundred people. Prior to their incorporation into the state in the mid 1800's Bassa were distributed along the coast among a series of village groups organized largely around hunting territories (the only indigenously-derived system of land tenure). In 1970, rural dwellers were administered through a hierarchy of chiefs (Paramount, Clan, and Town) instituted by the Republic of Liberia.

Kinship is not tightly structured among the Bassa. Participation in cooperative activities follows principally from residential alignment; and traditional kinship groupings, although ideologically consisting of both sibs and lineages, effectively include lineage co-members and a variety of affines and matrilineal kin.

Putative kinship ties assigned on the basis of common food taboos define an ideologically exogamous grouping of kin. However, although one is supposed to acquire food taboos through one's father, it is not uncommon to find either that food taboos are unknown or were inherited from both mother and father. The importance of sibs in marriage is negligible. As one man cogently noted, sib "business does not carry much weight in Bassa." Demonstrable kinship ties themselves rarely can be traced for more than four generations, and patrilineal groupings of such kin are organized only for a few purposes: birth, death, marriage, and—with land not among the items except in recent years—the redistribution of what little property a person may have. Income-earning property passes through the patrilineage; but at least in 1970 men passed on property to both men and women, brothers and sons, and eldest and youngest sons.

Although a number of different marriage procedures have been used, characteristically marriages have been contracted by the families of the couple involved through one or both of the following: (1) payment of brideprice (locally, "dowry") and (2) a period (7–10 years) of brideservice. Potential economic independence determined marriage eligibility for both men and women. Selection of spouses appears to have been largely a matter of personal choice. But a

young man looked to his father, or in their absence, to his father's or mother's brother for the wealth and mediator through which to contract his first marriage. At marriage a couple could choose to live either adjacent to the parents of the wife, the parents of the husband, or—albeit rarely except in recent years—in a location in which they had no kin. Even if an initial period of brideservice was part of the initial marriage contract men might choose to remain with their wife's parents after this service had been completed. Rights of sexual access to women were acquired with the initial contracting of marriage, but rights in children were not acquired until men had completed periods of brideservice or until the full brideprice was paid. Children themselves could be taken by, and were expected to be sent to, parents of either husband or wife; and children might be raised largely by kinsmen other than parents. Aged adults could expect support from their sons or the husbands of their daughters. A man's brother or independent adult son assumed responsibility for the support of his remaining wives and children on his death. However, a woman could choose whether or not to remain with her husband's kin group, return to her own parents, or seek support from her brothers, sisters, or adult children. These practices often brought affines together, and affines might be quite as important as consanguines in such tasks as house-construction or rice cultivation.

As with other Kru-speaking people (McEvoy, 1971; Davis, 1968), the Bassa long have engaged in migration to the coast for maritime work. Bassa participation in such labor migration—for which the Pepper Coast was noted in the nineteenth century—never was as intense as that of the Kra- and Grebo-speaking groups to the southeast, however. Rather than forming distinct communities, Bassa have been incorporated into migrant Kra (Kru) communities. The principal migrations of Bassa have been restricted largely to within Liberia. Although forming part of the flow of labor to Fernando Po in the early twentieth century, the principal directions of migration have been, in the nineteenth century to the towns along the coast, in the early twentieth century to the rubber plantations established at Harbel by Firestone, and since 1950, along the newly-constructed

road network to all centers of employment and education established in Liberia within the last two decades. In 1970, there were reported to be communities of Bassa in all major administrative centers in Liberia, from Voinjama in the northeast, to Harper at Cape Palmas in the southeast; and Bassa migrants often made up large segments of the labor force in the iron mines and plantations in Liberia.

Migration to Monrovia was one of the earliest Bassa movements. The Bassa Community in Monrovia was established as a corporate landowning and religious community as early as 1925. As elsewhere in Liberia, however, migration to Monrovia has come about largely since 1950, and has centered on obtaining money incomes—to purchase goods, pay brideprice and taxes—and the formal schooling or technical training needed for higher incomes. Initially a movement primarily of young adult males, Bassa migration in 1970 included large numbers of children being sent to be educated and large numbers of adult females looking for money, adventure, and husbands.

HOUSEHOLD CONFIGURATION IN MONROVIA

Four times the size of the next largest city and twice the size of the largest concession, in 1970 Monrovia was the political, commercial, and industrial center of the Republic of Liberia. Although the Bassa have long constituted a large segment of Monrovia's population (approximately 25 percent in 1962 [Battelle-Institute, 1963]), since its inception Monrovia has been ethnically heterogeneous. Euro-American diplomatic missions and managers of commercial and industrial firms combine with a large Lebanese community in addition to the descendants of the early Liberian settlers, representatives from all the Liberian tribes, and many non-Liberian African ethnic groups. The 1962 census revealed that 89 percent of Monrovia's population (then about 80,000) were migrants. The sex ratio was 1.40, and reflecting the absence of children, the dependency ratio was 52. Migration changes since 1960 appear to have evened out the sex ratio; and since migrants have borne children in the city and other children have been sent for schooling, the 1970 population of about 100–125,000

appeared to be considerably younger than it was a decade ago.

The growth and context of African urban areas is conventionally considered in terms of a series of variables: plurality, migration patterns, political structure and administrative policy, economic structure, and so forth (Southall, 1961; Mitchell, 1966; Epstein, 1967). However, the cost structure of Monrovia sets the context of urban life considered in this paper. As Caldwell (1968) notes generally, urban cost structure

... incessantly demands cash for necessities, and questions whether more is not available for tempting luxuries and diversions. Certainly the townsman has to have a very good income if he is not to be plagued by financial worries on a scale that even the more poverty-stricken villagers rarely know (1968:183).

Urban patterns of family, household, and kinship follow from the fact that some activity yielding a money income is necessary to subsist in Monrovia. Garden production of cassava, eddos, greens, or peppers contributes little to urban subsistence. Although nearly all available space—an unfinished house, an unused lot, the narrow spaces between dwellings—is used for garden crops, little unused land is available on which crops may be grown. Large garden plots are found on the outskirts of Monrovia (there are even a couple wet rice plots on low-lying ground on Bushrod Island), but it is mainly market sellers producing for sale, not consumption, who have contracted to use this land. Although a variety of petty trade activities, shops, tailoring, carpentry, clothes-washing and so forth offer self employment, most people are wage or salaried workers. Indeed, characteristically shop management itself is a secondary source of income for such workers. Moreover, many independent electricians, masons, and carpenters appear to earn their income working both independently and for large firms. Room rentals also may serve as a secondary source of income. Dwellings often incorporate shops, and are more likely than shops to be a primary source of income. Further, by eliminating rent, dwelling ownership reduces the amount of money needed to subsist in Monrovia. Relatives serve as a tertiary source of income, either as they occasionally send money, or most commonly food, to the household, or as, together with

friends and voluntary associations, they provide backing in times of crisis: hospitalization, trouble with the police, burial expenses and funeral preparations, and so forth.

Three distinct classes of income are available to Bassa in Monrovia. Generally, income follows from occupation and occupation from education. These relationships are commonly recognized, and Bassa explicitly link the ability to earn income and educational attainment. Almost everyone wants their children to go as far in school as they can. Nearly all migrants sent into Monrovia as children report they were sent to receive an education. In fact, however, the highest incomes are available most commonly only on entrepreneurial abilities. This income class, \$180 per month and over, is reported principally by people owning and operating such businesses as gin distilleries or sets of rooming houses. Because as much as 90 percent of the Bassa population in Monrovia earn incomes within these ranges, most important are the income classes of (1) between \$180–60 per month, and (2) under \$60 per month. These are the incomes made available by educational attainment (either or both formal schooling and technical training) qualifying people either for “skilled” (mechanics, drivers, clerks, nurses, teachers, lawyers, etc.) jobs or “unskilled” (messenger, farmer, dock-loader, petty traders, etc.) jobs.

The importance of these income classes lies in that they represent respectively relatively high and low incomes. The importance of the occupational categories lies in that—except for market selling which is notable for its production of reliable incomes for the astute—they represent respectively relatively stable and unstable positions. One is less likely to be fired or lose a job as a teacher, clerk, mason, electrician, and driver than as an unskilled manual laborer. If a job is lost, skilled persons seem to find other jobs more quickly and easily than unskilled and illiterate persons. Until recently, educational opportunities have been available almost exclusively to men. Fifty-four percent of the male household heads sampled reported either technical training or formal schooling past the elementary grades. Less than 1 percent of the female household heads sampled reported such training. At least in part, males have been sent to school over females be-

cause they were considered to be the ones who should be the principal sources of subsistence. The restrictions these practices place on the income earning power of women is recognized to be severe. As one man noted, without an education "a daughter is someone's property." Unskilled and illiterate men, however, are hardly in better positions. Minimum expenditures for food run between \$20–30 per month. Room rentals run between \$6–15 per month, without guarantees for running water, electricity, or indoor toilet facilities. Tuition, books, and uniforms for schooling past the elementary grades run \$50–75 per year, and even skill training—in apprenticeship, for instance, to a driver or tailor—may cost \$30 or more. Such expenditures leave little money to clothe and keep healthy a family whose monthly income is \$60 or less. Only in exceptional circumstances can relatives be expected to provide large sums of money for situations that are not emergencies. As people comment, "Monrovia is very hard and each of us mind our own business and help ourselves," and "We all have troubles; we have to make our own way."

Although the economic structure of Monrovia sets the immediate context for urban living, it followed from the establishment of industrial technologies in Liberia. First introduced by the Firestone Plantations Company in the 1920's, but principally since 1950 with additional plantations, iron mines, and subsidiary development in government, construction, finance, manufacturing, and local commerce, industrial technologies restructured income-earning possibilities and redefined work competences. Whereas the technologies of swidden agriculture applied the generalized knowledge of a few persons organized along lines of age and sex to produce food for household units, industrial technologies applied the specialized knowledge of many people to markedly differentiated task-sets to complete goals which themselves were only small steps in more complex production processes. Knowledge of subjects unrelated to swidden technologies was required for employment either in the bureaucracies operating the new technologies or in firms serving those bureaucracies or their employees. Moreover, the industrial technologies in Liberia, a world network of other industries,

and local organizations, encompassing both the Liberian government and one-person firms engaged in food marketing, were inter-linked through the use of money. Money came to be used for subsistence purposes and the amount available to job seekers was a function of their formal schooling or skill training. By creating a set of niches in which subsistence derived from money incomes but in which a premium (amount of money earned) was set on formal training, the introduction of these technologies within the last two decades established the primary conditions to which individuals (and in aggregate, patterns of residence and kinship) had to adjust as they sought to obtain and maintain a livelihood in Monrovia.

As its title indicates, this paper seeks to explain household configuration among urban Bassa. As will be shown, distinct residential alignments imply distinct patterns of kinship. These patterns, in turn, are implied by conditions established by the introduction of industrial technologies.

The household types (see Table 1) to be explained among Bassa in Monrovia include (1) single men, (2) single women, (3) mixed single persons, either exclusively male or female, including either unrelated adult males or consanguineally related males with no more than one adult, himself not the father of the child, or consanguineally related females with no more than one adult, herself not the mother of the child, (4) parent-child households restricted to single adults with resident children (under c. fifteen years of age), including both patri- and matri-focal structures, (5) consanguineal households, restricted to structures of consanguineally related kin in which there are two or more resident adults (cf. Gonzalez, 1969),³ including both matri- and patri-focal structures, each exhibiting a variety of forms (e.g., parent-child units in which the child is an adult often supporting the parent; Z-Z structures, with and without non-adult children; FB-BD structures; B-Z structures, and other), (6) nuclear family households including married couples with and without children,

³ It is the presence of adults one would expect to find elsewhere that needs explaining (e.g., why are adult women not married? or if married, not residing with their husbands? or if residing with kin, why with particular kin?).

Table 1. Frequency Distribution of Household Types,
140 Bassa Households, Monrovia, 1970

Household Types	Frequency	Percentage
1. Single male	16	11.4
2. Single female	11	7.9
3. Mixed	4	2.8
4. Parent-child Matrifocal Patrifocal	6 (5) (1)	4.3
5. Consanguineal Matrifocal Patrifocal	19 (14) (5)	13.6
6. Nuclear family	44	31.4
7. Nuclear family, extended with affines or both affines and consanguines	25	17.9
8. Nuclear family, extended only with consanguines	15	10.7
Total	140	100.0

(7) households in which nuclear families are the core unit, but which are extended to include as dependents a wide variety of kin, mostly children, tracing either both affinal and consanguineal relations to the household head or affinal ties only, and (8) households in which nuclear families form the core unit, but which are extended to include as dependents kin, mostly adults, whose only relation to the household head is a consanguineal one.

These household configurations take shape as a series of individuals and groups of people dependent on themselves or on one another for subsistence—single adults, single children, parent-child and other consanguineal structures, and nuclear families—coalesce in the process of obtaining and maintaining a livelihood.

People with saleable skills and/or formal schooling past the elementary grades obtain skilled jobs providing relatively large incomes and reliable employment. Males, primarily, occupy such positions, and can provide a largely adequate level of subsistence for their wives and children. These men contract marriages in accordance with the indigenous expectation that they will be the main source of support. The specific procedure may but rarely involve a church or civil ceremony,

most frequently payment of brideprice—the latter is commonplace in all marriages of this kind—but the couple reside together; and to the best of his ability, the husband complies with obligations to help his affines periodically. It is these men who become the focus of nuclear families and head households in which the core unit is that man's wife (or wives) and children. The children of rural dwellers sent into Monrovia for schooling unavailable to them for lack of money or school facilities upcountry are sent into such households. In such households, fostered children (both affines and consanguines) are most likely to be accepted, supported, and educated, not merely used for household help.

Unskilled, illiterate men obtaining unskilled jobs are set in sharp contrast. Unskilled jobs tend to be unstable and low-paying. Where nuclear families are found among such men, often unions were contracted prior to migration. Such marriages within Monrovia, while obligating men to provide gifts, money, and services to their wife's family characteristically delete payment of brideprice. It is not at all uncommon to hear the statement "My family no longer takes dowry for marriage." Moreover, children from such families may reside either with wealthier relatives to whom they were sent for schooling or

in boarding schools supported by such relatives.

There may be nuclear families transitional between the above income classes whose wages or salaries provided by males are supplemented by adult females engaged in petty trade. As noted elsewhere (Handwerker, forthcoming), some Bassa have come to expect that women will contribute to household subsistence in Monrovia. Ideally, when a migrant first comes to Monrovia he leaves his family (if any) upcountry. After he finds a job, he sends for his wife (or first wife if there are more than one). He is expected to give this woman a small sum of money with which to make market to meet her small needs for money, often to pay for the food eaten daily, and to help her husband in any emergencies that may arise. As additional co-wives (if any) come to the city, it is the responsibility of the first wife to give each a small sum of money so that these women also may help provision the household. Occasionally one finds women trading in market places because their husbands did not want them sitting around without making any contribution to household subsistence. More often, however, women have taken it upon themselves to provide money for tuition, books, and school uniforms for their children, to help their parents upcountry, to help relatives paying for the education of their children, or (among women beginning trade late in life) to be less of a burden to the sons, sons-in-law or daughters with whom they live. Market sellers generally come into being as women assume responsibility for subsistence inadequately handled by men (Handwerker, forthcoming). In some cases working women provide a basis for the maintenance of nuclear family households, occasionally with dependents, in Monrovia.⁴

The subsistence level for such households generally is minimal, however, and nearly half the market sellers in Monrovia appear to derive from still other marriage practices.

⁴ Women work even in families whose males are highly educated. However, women from such families are found in shops rather than markets and trade for different reasons. They work to help their husbands purchase land or build a house, and/or to provide added security in times of crises. Most often in such instances, women oversee a shop financed and usually administered by their husbands.

Although there are exceptions—marriage for love, a settled life, companionship, etc.—illiterate and unskilled men tend not to establish residential marriages and may not contract marriage. Non-residential marriages not only delete payment of a brideprice but also are contracted only by the individuals involved and consist only of an obligatory support for the spouse and resulting children in exchange for a continuing exclusive claim to the right of sexual access to the woman (cf. Goodenough, 1970:12–13). Although for some partners such unions involve no lasting commitment, many of these marriages endure. In either case, the children are often raised by the wife or the parents of the wife. But whereas traditionally a man's claim to his children rested on full payment of brideprice and/or completed brideservice, today in most cases his claim is acknowledged even in non-residential marriages.

All women interviewed and nearly all men wanted to marry. Women view marriage as "not having to worry about how to get money for your children," and "having a man to take care of your needs." Men view it as having someone to "help you save money." "A wife can take care of your home," and "Friends will respect you." Men also seek wives "to have children," and because wives will "help advise you." Both men and women emphasize the desirability of forming complementary and interdependent unions. Illiterate and unskilled men, however, cannot afford—or choose not to afford—the expenses and obligations incurred in a residential marriage. Women and their families have adjusted to this situation. By contracting individual unions, or by avoiding any type of marriage, men create a series of single male, single female, and matrifocal parent-child households in which women, solely or supplementing the \$5–15 occasionally provided by their husbands (and, perhaps, occasional contributions from their own kin), support themselves through market selling, domestic work, and so forth.

Single female and matrifocal parent-child households also are created by the death or divorce of a residential husband, but only when a woman is unable to make effective her claims to the support of her husband's brother, her adult children, or her brothers and sisters. If these people are unable to

offer support, the woman either returns upcountry or remains in Monrovia supporting herself and her dependents as noted above.

Single females and males may cluster in single households to share rent, and some mixed households are created when a single adult asks for or accepts a child or two to help with household tasks or firms engaged in petty trade.

Patrifocal parent-child and single male households also are created by the death or divorce of a spouse. Equally important however, such households are created when the wife or wife-child unit returns upcountry, or is sent, or remains upcountry to grow rice for the urban household or oversee purchased property situated outside Monrovia.

Consanguineal households and nuclear family households extended to include only dependents tracing relationships to the household head by consanguineal ties derive from such properties as land, shops, and dwellings. Such properties enable the owners either to invite kin or to accept those who come by producing income, reducing subsistence costs, or, for such properties as shops and rooming houses, by requiring personnel for their operation. Such properties attract consanguineal kin who have no jobs, or have lost them, because such kin are the ones with the strongest current or future claims to the benefits of those properties (free residence or actual income). Extended family households of one kind, or consanguineal households, follow from whether or not the original household structure was a nuclear family.

In summary, the Bassa moving to Monrovia have become enmeshed with an industrial system and money economy in which one's educational attainment establishes one's relation to cost structure. Bassa household configuration exhibits fundamental differences following from the education attainment of their heads (see Table 2). A person's education tends to be necessary and sufficient for the occupation in which he is employed. A person's occupational skill level tends to be necessary and sufficient for the amount of money he earns. High incomes, in this instance monthly incomes greater than \$60, tend to be necessary and sufficient for forming residentially unified nuclear families. Conversely, low incomes tend to be necessary and sufficient for the absence of such fami-

lies. Educational attainment sharply differentiates two major household configurations, one organized around nuclear families, and one not organized around nuclear families. Each major configuration exhibits variation.

Among the Bassa, there are three major variants within the configuration organized around nuclear families: (1) independent nuclear family households, (2) households extended to include as dependents a variety of kin, mostly children, tracing either affinal or both affinal and consanguineal relations to the household head, and (3) households extended to include as dependents mostly adults who trace only consanguineal ties to the household head, usually along one line. The differentiation of these households is tied to (a) the income available to the household, (b) the size of the original unit, (c) the presence of kinsmen whose children cannot go to school for lack of money or school facilities, and (d) the ownership of such income-earning properties as a shop, a house, or land (see Table 3). The amount of money available for subsistence among households organized around nuclear families tends to differentiate independent from extended nuclear family households. Income sources vary, however, including non-monetary income and income received from persons not resident in urban households. Moreover, the size and age-sex structure of the original nuclear unit and the presence of income-earning property modify judgments on the adequacy of the available money income. However, relatively high incomes do tend to be necessary for any household extension beyond the nuclear unit. Nuclear families themselves are necessary conditions for any affines to be included in the household. But whereas affines may be found in all subsistence contexts supporting nuclear families, the presence of clusters of consanguineal kin tends to imply that household subsistence is based on property.

Among the Bassa, there are five major variants within the configuration not organized around nuclear families: (1) single men, (2) single women, (3) mixed adults, (4) parent-child structures, and (5) consanguineal households. The constituents of these households tend to be uneducated, qualifying for unskilled work (messengers, dock loaders) and such entrepreneurial activities as prostitution and petty trade. Little in the way

Table 2. Distributions, Correlation, and Probability Values for (1) Educational Attainment by Occupational Skill Level, (2) Occupational Skill Level by Level of Income, and (3) Level of Income by the Presence or Absence of Nuclear Families within Households, 140 Bassa Households in Monrovia, Liberia, 1970

1. Education ^a	Occupation		Total
	Skilled	Unskilled	
Completed high school or skill training	37	1	38
Incompleted high school or skill training	15	7	22
Nonexistent high school or skill training	1	79	80
Total	53	87	140

2. Occupation ^b	Income			Total
	Over \$120/mo.	\$120-61/mo.	\$60/mo. and Under	
Skilled	10	32	11	53
Unskilled	5	25	57	87
Total	15	57	68	140

3. Income ^c	Nuclear Families		Total
	Present	Absent	
Over \$120/month	14	1	15
\$120-61/month	45	12	57
\$60/month and under	25	43	68
Total	84	56	140

^aGamma = +.9499, Chi-square derived P less than .001.

^bGamma = +.6873, Chi-square derived P less than .001.

^cGamma = +.7593, Chi-square derived P less than .001.

of general explanation may be suggested for the first four types of household. All four presuppose that individual adults are able to exist independently from other kin, and especially for women, that other kin are economically unable to accept them into their households. The first three also probably imply geographical or social barriers to residence with other kin. The fourth also implies the death, divorce, or migration of a spouse, conditions for which the present data yield no predictive variables. It is principally the presence of income-earning property that differentiates sub-categories among households not organized around nuclear families. Clusters of consanguineally related kin including at least two adults are set off from all other types of households not orga-

nized around nuclear families. Household subsistence based on such property tends to be sufficient for such clusters, whether they consist of a brother and sister, two brothers a parent-child structure in which the child is an adult, a three generation series of parents and children, a grandmother with the grandchildren of either a son or daughter including at least one adult grandchild, or a father's brother and brother's daughter, or some other configuration (see Table 4).

A variety of alternative explanations for some aspects of these data may occur to the reader, some suggested by other case-studies and some suggested by the complexity of the inter-correlations of the principal variables with which this paper has dealt (see Table 5). A full partial correlation analysis o

Table 3. Distributions, Correlation, and Probability Values for (1) Level of Income by Presence or Absence of Dependents in Households Organized around a Nuclear Family, (2) Presence or Absence of Nuclear Families by Presence or Absence of Affines as Dependents, (3) Presence or Absence of Income-earning Property by Presence or Absence of Dependents Tracing only Consanguineal Ties to Household Heads among Households Organized around Nuclear Families, and (4) Presence or Absence of Income-earning Property by Relations Dependents Trace to the Household Head among Households Organized around Nuclear Families Having Dependents, 140 Bassa Households in Monrovia, Liberia, 1970

1. Level of Income ^a	Dependents		Total
	Present	Absent	
Over \$60/month	35	24	59
\$60/month and under	9	16	25
Total	44	40	84

2. Nuclear Families ^b	Affines		Total
	Present	Absent	
Present	25	59	84
Absent	0	56	56
Total	25	115	140

3. Property ^c	Cluster of Consanguines		Total
	Present	Absent	
Present	14	29	43
Absent	1	40	41
Total	15	69	84

4. Property ^d	Relation to Household Head		Total
	Consanguineal	Affinal or Mixed	
Present	14	13	27
Absent	1	12	13
Total	15	25	40

^aGamma = +.4433, Chi-square derived P = .05-.02.

^bGamma = +1.00, Chi-square derived P less than .001.

^cGamma = +.9015, Chi-square derived P less than .001.

^dGamma = +.8566, 1-tailed Fisher's Exact Test P = .0134.

Table 5 yields the same variable interlinkages indicated above, albeit less parsimoniously (cf. Handwerker, 1972a), and its exposition here is unnecessary. However, two lines of thought deserve consideration at this point.

First, it might be supposed that the educational process is accompanied by attitude changes favoring the formation of nuclear families. There is a relatively strong rela-

tionship between educational attainment and nuclear families (Phi-square = .18, P less than .001). As the Bassa themselves and as this paper indicate, however, attracting spouses and supporting families are the principal bases for the formation of nuclear families. When individuals are asked why they have not married or have married only informally and live apart, they usually emphasize economic liabilities: marriage is too expen-

Table 4. Distribution, Correlation, and Probability Values for Presence or Absence of Income-earning Property by Presence or Absence of Households Consisting only of a Cluster of Consanguineal Kin with Two or More Adults among Households not Organized around Nuclear Families, 140 Bassa Households in Monrovia, Liberia, 1970

Income-earning Property	Household Configuration		Total
	Consanguineal	Other ^a	
Present	10	5	15
Absent	9	32	41
Total	19	37	56

Gamma = +.7534, Chi-square derived $P = .01-.001$

^a"Other" includes single men and women households, households consisting of mixed adults, and parent-child households.

sive, one is waiting until he gets a better job, another until he has some skill training or can receive further schooling. It is men with schooling beyond the elementary grades or skill training working at skilled jobs and earning relatively high wages who form nuclear families. When the effect of education on nuclear families is controlled by sex and income simultaneously, the correlation between education and residential nuclear families appearing at first sight to be strong disappears (.08). Although the educational process may play some role in producing nuclear families—and almost certainly does among the highly educated (e.g., Baker and Bird, 1959)—level and reliability of income are the principal considerations among such people as the Bassa whose education is limited to high school and skill training.

Second, it might be supposed that household configurations and the patterns of marriage and kinship implied by them vary by age. Younger persons are more likely than older (c. thirty-five years and older) to head households organized around their own nuclear families (Phi-square = .03, $P = .05-.02$), and older persons are more likely than younger to head households extended to include a cluster of consanguineal kin (Phi-square = .04, $P = .01-.02$). Perhaps here we see a trend toward the formation of nuclear families among the young and the retention of extended families among the old. Again, however, the alternative explanation of these data cannot be accepted. The role age plays

in producing the observable patterns of residence and kinship is either attenuated or indirect. On the one hand, if we consider the possibility that it is young men who form residential nuclear families and live in households not including dependents, our hypothesis is manifestly falsified. Young and old men alike are equally likely to live with their wives and children in households including no dependents (Phi-square = .001, $P = .70-.50$). The most plausible explanation for the weak and probably insignificant role age plays in producing residentially unified nuclear families is that the spouses of older people are more likely to have died, migrated or be divorced than are the spouses of younger people. On the other hand, age is important in producing households extended to include a cluster of consanguineal kin and dependents through its relation to property. By inheriting, by having more time to accumulate savings for purchase, and by being in Monrovia at earlier periods when land and dwellings were more readily purchased, older people are more likely than younger to own property (Phi-square = .27, P less than .001). When the effect of age on household configuration is controlled by property, the correlation almost disappears (.01).

CONCLUSION

In a preliminary way, this paper has attempted to raise questions about how the intertwining of technology and social structure

Table 5. Correlation Matrix of Phi-square Values,* 140 Bassa Households, Monrovia, Liberia, 1970

Variables	Variables								
	1	2	3	4	5	6	7	8	9
1. Sex ^a	--								
2. Education ^b	.17	--							
3. Occupation ^c	.14	.76	--						
4. Income ^d	.09	.17	.19	--					
5. Nuclear Family ^e	.41	.18	.11	.20	--				
6. Affines ^f	.06	.04	.05	.04	.14	--			
7. Property ^g	.01	.02	.01	.12	.06	.01	--		
8. Age ^h	.03	.00	.00	.02	.03	.02	.27	--	
9. Consanguines ⁱ	.07	.01	.00	.00	.03	.07	.11	.04	--

*Phi-square values correspond to the following Chi-square derived P's: .08, P less than .001; .08-.05, P = .001-.01; .05-.04, P = .01-.02. Phi-square values for correlations 3x8, 4x8, 5x8, 6x9, and 1x9 represent negative correlations.

^aMale or female household head.

^bPresence or absence of technical training and/or formal schooling past the elementary grades.

^cPresence or absence of skilled occupational level.

^dPresence or absence of monthly incomes over \$60.00/month.

^ePresence or absence of nuclear family units resident in households.

^fPresence or absence of dependents tracing affinal ties to the household head.

^gPresence or absence of either land, dwellings, shops, or a combination of such income-earning properties owned by the household head.

^hHousehold head aged c. 35 years or older.

ⁱPresence or absence of dependents whose only relation to the household head is consanguineal (consanguines clustering to the exclusion of other kin).

has been conceptualized. The data from Monrovia, and from African urban areas generally, cannot be explained by positing a trend toward conjugal family norms or independent nuclear family households. The inability of present conceptual schemes to offer an intelligible pattern for these data raises questions about the viability of relating household and kinship structure to technology, the nature of relations between these phenomena (granting a relation), our ability to generalize about those relations, and the extent to which we are actually studying change in contemporary settings. This paper could address itself explicitly

only to the first two questions. It has been shown that there are demonstrable linkages between the industrial context of Monrovia and some aspects of household and kinship structure. Before speculating on the nature of that linkage, it is possible to suggest why discrepancies exist between field reports and cross-cultural generalizations.⁵

⁵ In asserting that there are discrepancies between cross-cultural generalizations and field reports, this paper does not assume that the former claim invariant relations between the variables investigated. The basis for asserting discrepancy is that the complexity in household and kinship structure delineated in this paper is characteristic of data on most other urban areas in tropical Africa, irrespective of time

The data from Monrovia cast doubt on interpretations given to categories (e.g., "extended" family or household) often making no distinction between residential alignment and kinship structure and inevitably obscuring significant distinctions among each of these phenomena. Identifying significant distinctions may be a task only intensive field work will resolve. It is clear, however, that cross-cultural investigators will have to comb their original sources thoroughly if they are to grasp the probable intricacies of emergent patterns of these phenomena. Despite the common problem of finding field reports loosely using the concept of "extended" household or family to encompass a wide variety of phenomena, with African urban areas usually data are tucked away in case-histories and quotes from informants on who lives with whom, and why. One may object that such data do not indicate frequency. Although statements of frequency have some value—e.g., ascertaining the extent to which female-headed households are a function of male death rates—a fascination with averages appears to be equally important in producing questionable interpretations of correlations derived from cross-cultural research. The data from Monrovia cast doubt on the utility of defining trends by reference to the frequency with which a certain type of family or household appears in a community or society. What may appear to be a trend in the direction of nuclear family organization may well reflect only the relative absence of property owned by urban residents, the relative availability of school facilities in rural areas, and structural constraints, particularly governmental policy, affecting these variables.

More fundamental, interpretation of cross-cultural data appears to rest on questionable assumptions about the nature of the data and problem. It has been taken for granted that the nature of the data is such to distinguish between two types of family systems and only two types, extended and nuclear, and that the problem is one of socio-cultural change from the former to the latter.⁶ The

configurations has inflated the importance of extended family households in traditional societies. It is argued that demographic factors alone impose constraints on possible household configuration and suggest that large and complex households are uncommon in large populations.

These arguments properly remind us that independent nuclear family households are found in nearly all societies, perhaps as the mode in most societies. As the text indicates, the extent of change and the elements that change with industrialization are unanswered empirical issues. However, the arguments of these papers are vitiated by fundamental conceptual and methodological errors. Of least importance, these arguments equate household size with complexity of structure without empirical confirmation that these variables strongly covary. Because these arguments take no cognizance of households not organized around nuclear families, one must first decide which households can be called "extended." For convenience, we can divide the Bassa sample at the median household size, and include under "extended" family households the consanguineal households not organized around nuclear families. As one intuitively suspects, there is a real relationship between the two variables (Φ -square = .27, P less than .001). But whether such a correlation provides a sound basis for equating the two variables is questionable. An alternative measure more accurately indicates the relation between these variables (Γ = +.83). Among the sampled Bassa, extended family households tend to be sufficient, but not necessary, for households of large size. Before size and complexity are equated and used in other arguments, more empirical work is required to identify their relationship under different circumstances.

Of fundamental importance, these arguments appear not to recognize that "household" itself is a culturally highly variable phenomenon, and that statements of "rules of residence" often are not simultaneously statements of household composition. An independent nuclear family household may reside patrilocally even if the unit only resides in the village, or a quarter or the village, or in the camp of the husband's father. No control is exercised over differences in definitions used by census takers, and these definitions themselves appear rarely to derive from local definitions. Census data often is of little or no value in analyzing household and kinship structure. For instance, one might interpret the mean size of households in Monrovia reported by the 1962 Liberian census (\bar{X} = 3.924) as indicating the overwhelming importance of independent nuclear family households. However, the mean size of households among the sampled Bassa is wholly congruent with this figure (\bar{X} = 3.575). Partly because these means are lowered by relatively large numbers of households not organized around nuclear families, and partly because household size and complexity do not covary perfectly, data on mean household size from both the 1962 census and the sampled Bassa yield no indication of the important complexities in household structure exhibited by the data on the Bassa.

(1930's–1960's) or area (East, Central, South, West) of research.

⁶ Coale, et al. (1965) and Burch (1970), notably, have questioned this proposition, arguing that the question of change is in part spurious because emphasis on rules of residence rather than empirical

data from Monrovia suggest that a more fundamental division obtains between households organized around nuclear families and households not organized around nuclear families. Sometimes, we forget that extended family systems and residential units usually are constructed from nuclear family units. Perhaps we should ask first, "Under what circumstances do we find residentially unified nuclear families?" and only then ask "Under what circumstances do we find independent nuclear family households, and under what circumstances do we find various kinds of extended family households?"

The question of conceptualization is related to the question of change, which requires another paper. However, while granting that changes in household and kinship structure occur where people become enmeshed with industrial systems, such changes do not appear to be as all-sweeping as sometimes construed. Among the Bassa, for instance, the question raised earlier of a trend toward the formation of nuclear family households among the young and the retention of extended family households among the old was spurious from the first. Although reports from early travelers along the Pepper Coast suggest complex residential units (cf. Bosman, 1704:475-84), within living memory it has been commonplace for households to consist of a husband, wife or wives, and their children. Various kinds of households organized around nuclear families, both independent and extended, have existed in the past, and exist now in Monrovia. The question of change must take cognizance of the variety of household and kinship structures found in a community, not solely modal types or norms. Furthermore, analysis of the question of change must be more precise in identifying the data exhibiting equivalence or difference over time. Among the Bassa, the most striking change in household and kinship configuration is the emergence of a large number of households not organized around nuclear families.

The questions of conceptualization and change are related to the question of generalization. Like the question of change, the question of constructing theoretical generalizations requires another paper (Handwerker, 1972b). However, the data from Monrovia suggest two propositions:

(1) industrial systems of technology tend to be necessary and sufficient for households not organized around nuclear families.

(2) relatively high and reliable income (food crops or money) tends to be necessary and sufficient for the formation of residentially unified nuclear family units.

This paper is able to discuss the questions of change and generalization only minimally. The data presented here form a more substantive basis for speculating on the nature of the linkage between technology and household and kinship structure, and a set of variables systematic consideration of which may help clarify the intertwining of these phenomena. Briefly stated, people appear to be linked to technologies through income (food crops or money). Patterns of household and kinship appear to derive from the ways people obtain and maintain a livelihood within the constraints and options established by the technologies to which they are bound. Where "technology" is taken to include both tools and the goals to which they are put, it appears that different technologies establish different economic contexts of income and income-earning activities to which household and kinship structures adjust. In the case at hand, it appears likely that industrial systems establish two key properties of the economic context: (1) the use of money for subsistence, and (2) differential access to money incomes through skill training.⁷ As apparent in this paper, however, several other variables have implications for household and kinship structure. To disentangle these phenomena, it might be useful to investigate the intertwining of the following variables: (1) the types of activities yielding incomes and the types of income, (2) the amounts, frequency, and reliability of particular income streams, (3) the persons bringing in incomes of different kinds, and the controls over income exercised by different people, (4) the kinds, amounts, and frequency of use of different types of

⁷ Although these characteristics are set forth as universals, it should be emphasized that they are variables. This paper deals with only one set of properties these variables may assume. Differences in political structure and policy (possibly the most important consideration in variable class 5 in the text) in particular, may change the implications for household and kinship structure of both the use of money for subsistence, and the need for skill training.

capital inputs (including education, land, labor, money) people use to perpetuate and/or to increase existing incomes, (5) the types of restrictions and opportunities bearing on a person's access to different types of capital, (6) the types, frequency, and amount of consumption expenditure (i.e., a consideration of "wants," from new clothes to the maintenance of kinship ties), and (7) the sociodemographic structure of units supported, produced by, and, perhaps, producing variable classes 1-6.

REFERENCES

- Aberle, David F.
1961 "Matrilineal descent in cross-cultural perspective." Pp. 655-727 in D. Schneider and K. Gough (eds.), *Matrilineal Kinship*. Berkeley: University of California Press.
- Aldous, Joan
1962 "Urbanization, the extended family and kinship ties in West Africa." *Social Forces* 41 (September):6-12.
- Baker, Tanya and Mary Bird
1959 "Urbanisation and the position of women." *The Sociological Review* 7:99-122.
- Battelle-Institute
1963 Report of the Battelle-Institute, Frankfurt-Main, to the Republic of Liberia. City and Regional Planning, Monrovia/Liberia. Volume 6.
- Blumberg, R. L. and Robert F. Winch
1972 "Societal complexity and familial complexity: evidence for the curvilinear hypothesis." *American Journal of Sociology* 77 (March):898-920.
- Bosman, William
1704 [1967] *A New and Accurate Description of the Coast of Guinea*. J. R. Willis (ed.), London: Frank Cass.
- Burch, Thomas K.
1970 "Some demographic determinants of average household size: an analytic approach." *Demography* 7 (January):61-9.
- Caldwell, J. C.
1968 *African Rural-Urban Migration*. New York: Columbia University Press.
- Coale, A. J., L. A. Fallers, M. J. Levy and J. S. Tompkins
1965 *Aspects of the Analysis of Family Structure*. Princeton: Princeton University Press.
- Colson, Elizabeth
1962 "Family change in contemporary Africa." *Annals of the New York Academy of Sciences* 96 (January):641-52.
- Comhaire, J. L.
1956 "Economic change and the extended family." *Annals of the American Academy of Political and Social Science* 305:45-56.
- Davis, Ronald
1968 *Historical Outline of the Kru Coast, Liberia, 1500 to the Present*. Ann Arbor: University Microfilms.
- Driver, Harold
1956 "An integration of functional, evolutionary and historical theory by means of correlations." *Indiana University Publications in Anthropology and Linguistics* 22 (1), Memoir 12.
- Epstein, A. L.
1967 "Urbanization and social change in Africa." *Current Anthropology* 8 (October):275-96.
- Goode, William J.
1963 *World Revolution and Family Patterns*. New York: Free Press.
- Goodenough, Ward H.
1970 *Description and Comparison in Cultural Anthropology*. Chicago: Aldine.
- Gonzalez, Nancie S.
1969 *Black Carib Household Structure*. Seattle: University of Washington Press.
- Handwerker, W. Penn
1971 *The Liberian Internal Market System*. Ann Arbor: University Microfilms.
- 1972a "Causality and household organization in urban Africa." Paper delivered to the Northwest Anthropological Conference, Portland. Unpublished manuscript.
- 1972b "Technology and household configuration in urban Africa: patterns and explanation." Paper delivered to the 71st Annual Meetings of the American Anthropological Association, Toronto. Unpublished manuscript.
- Forth- "Changing household organization in the coming origins of market places in Liberia." *Economic Development and Cultural Change*.
- Hendrickson, Leslie C.
1970 *Kinship, Achievement and Social Change in Tribal Society*. Ann Arbor: University Microfilms.
- Lowie, Robert H.
1920 [1961] *Primitive Society*. New York: Harper and Brothers.
- Mair, Lucy
1969 *African Marriage and Social Change*. Reprint edition. London: Frank Cass.
- McEvoy, Frederick
1971 *History, Tradition and Kinship as Factors in Modern Sabo Labor Migration*. Ann Arbor: University Microfilms.
- Mitchell, J. Clyde
1966 "Theoretical orientations in African urban studies." Pp. 37-68 in Michael Banton (ed.), *A.S.A. 4, The Social Anthropology of Complex Societies*. London: Tavistock.
- Munsell, Marvin R.
1967 *Land and Labor in Salt River*. Ann Arbor: University Microfilms.
- Southall, Aidan
1961 "Introductory summary." Pp. 1-66 in Aidan Southall (ed.), *Social Change in Modern Africa*. London: Oxford University Press.

SOCIAL CHANGE, MIGRATION AND FAMILY INTERACTION IN BRAZIL *

BERNARD C. ROSEN

Cornell University

American Sociological Review 1973, Vol. 38 (April):198-212

The impact of an industrial city on rural migrant family structure and the socialization of boys is analyzed in an observational study of family interaction in 167 lower class Brazilian families. Four groups of families were selected to represent points on a rural-urban continuum, ranging from peasants on plantations, to recent rural migrants to the city, to rural migrants who have established a place for themselves in the city, and, finally, native urban dwellers. The study employs a theoretical model which stresses the ways in which experiences in the city change the personality and behavior of rural migrants, increasing their feelings of efficacy and altering perceptions and values. Observation of family interaction revealed that with a longer period of residence in the city, migrant families become more egalitarian, family relations become more open and responsive, and parents place greater emphasis on achievement and independence for their sons.

PEASANT families migrating to an industrial city in Brazil encounter conditions which can radically alter their lives. Time, space and human relationships in an urban environment are organized in ways markedly different from those characteristic of the countryside. The pace in the city is faster, congestion greater, social expectations different and interpersonal relations far more impersonal and competitive (Hutchinson, 1960; Foster, 1965; Kahl, 1969). Much has been written about the problems rural migrants experience adapting to these new conditions: their difficulty finding work, their poverty in a milieu of affluence and the sense of alienation this produces (Germani, 1961; Lopes, 1968). Little is known, however, about the city's impact upon relationships within migrant families, particularly as it affects parent-child interaction—the work of Lewis (1959) in Mexico being a notable exception. This is an unfortunate omission. For to the degree that urban life modifies family relationships and affects the socialization of children, massive migration to the cities will influence the development

of the next generation and change the course of a country for years to come. The omission is especially regrettable in the case of Brazil, where rapid population growth, industrialization and the changing structure of agriculture have induced a phenomenal movement of families out of the rural areas into the cities (Hutchinson, 1963a; Carmago, 1966; Shirley, 1970).

This paper reports a study of the impact living in an industrial city, São Paulo, has on interaction within rural migrant families in Brazil. As the third project in a series of researches conducted in Brazil over the past decade, this study specifically contrasts rural migrant families with peasants at one end of the rural-urban continuum and lower-class native city dwellers at the other. The data are presented in an analytical framework which emphasizes the ways experiences in the city change the personality and behavior of rural migrants. These changes are interacting elements in a complex social psychological process in which the perceptions, cognitions and values of migrants are altered under the impact of living in a city. Briefly stated, an industrial city affects migrants in the following ways: (1) it improves the migrants' standard of living; (2) increases the quantity and quality of resources available to them; (3) provides experiences which enhance their sense of efficacy; (4) alters their perception of the kind of world in which their children will grow up; and (5)

* This research was supported by grants from the National Science Foundation and the Cornell Latin American Studies Program. I wish to thank the following persons: Thomas Harblin for his invaluable help in designing the study and directing the field work, Anita LaRaia for her help in the data analysis, William Swallow who provided statistical counsel, and Barbara Francis for her critical comments on the manuscript.

fosters new values appropriate to industrial life. The effects of these changed social conditions and psychological states are new patterns of interactions within migrant families.

The process of change is set in motion by the impact on rural migrants of the city's relatively open opportunity structure. São Paulo's social structure as measured by its degree of social mobility is remarkably open. In the past two decades, there has been substantial upward social mobility in this rapidly industrializing city. Thus a study by Hutchinson (1963b) based on interviews with a random sample of adults in São Paulo, reports that 42 percent of the men had experienced upward mobility, a rate almost identical with a finding reported in a more recent study of social mobility in Brazil (Rosen, 1971b). Clearly the migrant in São Paulo has the opportunity to attain a higher position in life than was possible in the rural area. Jobs are more plentiful, new skills can be learned, wages are higher. Not all migrants, of course, improve their condition in São Paulo. Some return to the village, defeated in their search for a better life. Others remain in the city, crushed by poverty, isolated and hopeless. But many migrants establish a place for themselves in the city. They find work, usually in factories or in the construction trades; they find housing; they become part of the city. It is about the latter group that much of the following discussion is concerned.

As a consequence of the city's more open structure, the greater quantity and diversity of goods and services that can be found there means that the resources available to migrants are much expanded. These resources can be used to change traditional patterns of family interaction. New relationships develop between husband and wife as the sources and amount of income change. The material educational and recreational resources of the city also affect the socialization process. They enlarge the reward system available to parents. In contrast to the peasant, rural migrants can do more for their children; they can be more responsive to the child's needs. Rural migrants can use the city's resources to stimulate and motivate the child toward new goals, to elicit and reinforce new patterns of behavior.

The industrial city not only makes a higher

standard of living possible for migrants, it will for those who improve their situation provide experiences that increase their sense of personal efficacy. The migrant who finds work and makes a place for himself in the city will experience a psychological state denied most peasants—a sense of success. For he has in fact improved the conditions of his life. This sense of efficacy, a feeling that goals can be attained through one's own endeavor, has been identified as a salient difference between modern industrial man and the passivistic peasant (Rosen, 1964; Inkeles, 1966). Out of this experience of success will come a change in the migrant's perception of the kinds of behavior society rewards, and new notions of what is regarded as desirable and attainable. For the migrant can see that an industrial city rewards competition and achievement, and encourages independence (or at least tolerates it), to far greater extent than the rural community, where the hierarchical structure places a premium on passivity, conformity and obedience to authority.

The industrial-urban milieu is expected to heighten concern with independence and achievement. This would surely be the case with the father, whose immersion in urban life is most direct; he faces daily the reality of working in an industrial city. But mothers are also expected to change in the city. For a migrant mother is thrust into a variety of decision-making situations (shopping, working, allocating limited time and financial resources) which should increase her sense of personal competence, give her a better grasp of what is required for successful adjustment to urban life and make her want to communicate this information to her son. Migrants who recognize that competitiveness and self direction are necessary for success in an industrial city will come to value achievement and independence; authoritarian relationships in the family will seem less appropriate, and the encouragement of excellence in performance more desirable.

Support for these assertions can be found in several recent studies of modernizing societies, though regrettably none of them focus on migrants. Thus, Rosen and Simmons (1971a) found that it is partly because a woman can earn money in the city, thereby strengthening her leverage in decision-mak-

ing and enhancing her conception of the female role, that husband-wife relations tend to be more egalitarian in industrial communities than in non-industrial towns and villages (see also Safilios-Rothschild, 1970). Other researchers indicate that "modern" parents (i.e., those who live in cities, or engage in technical occupations, or belong to less traditional religious sects) tend to emphasize achievement and independence in the rearing of children (Prothro, 1961; Levine, 1968; Peterson and Miglino, 1970). And a study by Elder (1965) found that families in industrial countries tend to be less authoritarian than those in non-industrial nations.

Following this perspective, I expected that as the migrants' experiences in the city increased (operationally indexed by length of residence in São Paulo), relationships within the family would tend to become more egalitarian, communication and responsiveness of family members toward one another would grow greater, and more emphasis would be placed by parents on promoting achievement and independence in their children. These are the kinds of changes which improve the migrant's chances for survival in the city. Greater communication within the family facilitates the flow of information about how resources can be obtained and managed. The increased participation of everyone in decision-making leads to a pooling of skills for problem-solving. And by stressing achievement and independence in the socialization of their children, migrant parents are preparing the child to function in an industrial system which rewards the ability to be competitive and self-directed. These changes also involved the notion of directionality. I expected that as their length of residence in the city increased, migrant families would move away from rural patterns of interaction toward greater resemblance to the urban family.

RESEARCH PROCEDURE

The Comparative Design

Four groups of nuclear families, a total of 167 units, were purposively selected to represent points on a continuum of temporal exposure to an urban environment. All the subjects are native Brazilians, each family

has at least one male child in the ten to twelve year age range; the median age of the child in the four groups is eleven years. There are no significant differences between the groups in theoretically relevant social factors such as size of family or age of parents.

At the rural end of the continuum are families of *colonos*, farm workers living on corporate plantations located in the northern part of the state of São Paulo near the Minas Gerais border. Lists of farm workers were provided by the administrators of more than twenty farms. From these lists, only families that had never lived in a city were selected. The men work in the field; the women stay at home; most of the adults had little or no education; the children, however, attended school. All the families live on the land in simple houses provided by the farm owner. After an intensive search thirty-four families were found who met our criteria for inclusion in the sample frame; they formed the *rural group*.

The urban samples were drawn from neighborhoods (*bairros*) which comprise the northern zone of the city of São Paulo—an industrial metropolis of over five million inhabitants. Adequately serviced by transportation facilities which make the central city relatively accessible, this zone is known as a locus of migrants arriving from the northern part of the state, identical with or similar to the area from which the rural sample was drawn. Working-class people make up most of the area's population, though there is a scattering of lower middle class elements. The selection of the urban families proceeded in the following way. A one-page questionnaire was administered to students in the third and fourth grade in twenty elementary schools. Information provided by the questionnaires, plus interviews where necessary, enabled us to identify three separate groups of families. The first group of forty-four families (*recent migrants*) is composed of rural migrants, mostly from the state of São Paulo and nearby Minas Gerais, whose residence in the city averages about a year and a half. The second group of migrants, forty-six families in all, have lived in São Paulo for about six and a half years, on the average; this group is called *established migrants*.

And the last group, forty-three families, are natives of the city of São Paulo—commonly called *Paulistanos*.

It is difficult to know how representative these samples are of the universe from which they were drawn. No systematically gathered census data on migration or class distribution exist in Brazil against which our samples could be compared. In any case, our aim was the selection of criterion groups with different degrees of exposure to an urban ambience. To minimize the chance that the migrants had for one reason or another isolated themselves from urban experience, the samples were stratified so that in each urban group (recent migrant, established migrant and Paulistano) half of the men are factory workers, half are not; half of the women are exclusively housewives, the other half are gainfully employed outside the home. The effect was to create for each group a factorial design with the nature of the husband's work and the employment of the wife as classificatory variables. In each of the cells the number of cases is approximately the same. All the men are regularly employed in lower class occupations, as defined by a widely used scale devised by Hutchinson and Castaldi (Hutchinson, 1960). Many of the adults are without formal schooling; only a few have more than a year or two of primary education. Though the urban subjects are, like the rural group, lower class, their average income is higher; the farm families, however, enjoy the benefits of free housing and in some cases a plot of land on which to raise food.

Since our data are not longitudinal, there is no assurance that any differences found to exist between rural and urban groups are due to urban influences. Probably peasants who migrate to the city are different from those who remain in the village; hence, I expected the interaction patterns of recent migrants to be somewhat different from those of the rural group, though they had been in the city only a year and a half. More important, I expected migrant characteristics and urban influences to interact, producing changes in family structure which would become more pronounced over time, hence the inclusion of two migrant groups with different lengths of residence in the city. To the extent that the two migrant

groups are found to differ in ways predicted by the analytical model and congruent with previous research, I will regard my hypotheses about the impact of city life on interaction patterns in migrant families as being supported.

The Observational Design

Data on family interaction were obtained by systematically observing parents and children in their homes, under relatively controlled experimental conditions. In using this method I hoped to avoid several problems which have plagued studies of family interaction and socialization. One problem is the tendency for researchers to collect data from only one parent; the behavior of the other parent, usually the father, is either not reported or must be revealed by the spouse. Another problem is the tendency for researchers to focus only on the parental contribution to the socialization process (Bell, 1968; Osofsky, 1971). But socialization is a two-way process: the child initiates action as well as receives it. As any parent knows, the child's input is often critical; how a parent relates to the child frequently reflects what the child does; and yet this kind of information is seldom available. Also, the relationships between the parents are seldom reported, though the patterns of support and authority which characterize the parental dyad may significantly affect the way in which socialization is experienced by the child. For example, the behavior of parents toward one another may be as significant for the development of achievement and autonomy in the child as is their conduct toward him. Most troublesome is the common practice of relying entirely upon questionnaires or interview schedules to collect data, though there are a few notable exceptions to this rule (e.g., Strodbeck, 1958; Straus, 1968). Interviews are particularly vulnerable to errors flowing from inadequate recall or deliberate falsehoods. The advantage of direct observation is that the researcher can *see* how the father, mother and son relate to one another, rather than depend on what they *say* they do.

Each family was visited by a team of two field workers, one of whom was male, the other female; both were university students.

The male gave instructions, collected decision-making materials and generally took the role of the team chief—an appropriate definition of his position in a male oriented culture; the female focused largely on scoring interaction, though she was often an important element in creating and maintaining rapport. The parents were told that their son had been selected, among others in the area, to take part in a research project on children and how they learn. The observer explained that they wanted to observe how a boy performs certain tasks in the presence of his parents. After questions were answered and rapport established, the parents and boy were placed around a table, usually the kitchen table or some other flat surface such as a sewing machine top and three experiments were conducted. These experiments, based in part on my earlier study in the United States, enabled the field team to observe family interaction as the boy engaged in problem solving, or while the family played a competitive game (Rosen and D'Andrade, 1959).

Each experiment was intended to involve the parents in their son's performance; often they were faced with the choice of giving or refusing help. At times they were permitted to structure the situation according to their own norms; at other times the experimenters set the norm. In some situations the tasks were constructed so as to make the boy relatively dependent on his parents for aid; in other situations the boy could be independent if he wished. Where possible the family members were faced with situations which engendered discussion over what choice to make in the completion of a task. The observation and scoring of interaction between the three family members was intended to provide measures of (1) the degree of equality in family relationship, (2) the family's affection and communication structure, and (3) the emphasis parents placed on achievement, and independence. The investigators sought to get the subjects involved in the experiments by deliberately building some competition and stress into the situation. By so doing, it was hoped that the subjects would become immersed in the tasks, abandon their protective "company behavior," and permit us to observe more

authentic interaction in several hours than might be observed through casual observation over a much longer period of time. I believe that we were successful in achieving this goal. Of course, this method, like others, contains the potential for producing bias flowing from the nature of the experimental tasks and the presence of the observers.

The three tasks, each of which involved certain skills and judgmental abilities appropriate to the boys age and sex, are as follows:

1. *Block Stacking*. The boy was asked to build a tower out of irregularly shaped blocks. He was told that he could use both hands, but was blindfolded and thus made relatively dependent on his parents for information regarding his progress. The parents were told that this was a test of their son's ability to build things, and that they could say anything to their son but could not touch the blocks. A performance norm was set for this game by telling the subjects that the average boy could build a tower of four blocks before it fell. They were then asked to write down privately their own estimate of how high they thought the boy could build his tower. This experiment was designed to provide data on the parents' evaluation of the boy's competence (how high would their estimates be), and to see how self-reliant they expected or permitted their son to be (how much help would they give him) in a novel problem-solving situation. There were three trials for this task. The first two were identical. The procedure for the third trial differed from the others in that the boy was told that he would be given half of a cruzeiro (approximately 13 cents) for each block that he stacked. Each subject was asked privately to estimate how high the son should attempt to build his tower. No money would be given for blocks stacked higher than the estimate, but all money would be lost if the stack tumbled before the estimate was reached. Conservative estimates, therefore, provided security but little opportunity for gain; high estimates involved greater risk but more reward. After the estimates were made privately, they were revealed to all and the family was asked to reach a group decision as to how high the boy should attempt to build his tower. Apart from providing data on

aspiration level, this part of the experiment provided information on decision-making in a risk-taking situation.

2. *Tinker Toy Construction.* The group was presented with three Tinker Toy designs, clearly ranked by degree of difficulty, and asked to select one for the boy to construct. Each person privately indicated his choice; they were then revealed and where disagreement existed a group decision was made. Again the parents were told that they could say anything they wished, but were not permitted to touch any of the pieces. A time limit of fifteen minutes was set for this task. The purpose of this experiment was to provide data on expectation level and decision-making.

3. *Pick Up Sticks.* This game involved the active participation of all three family members. The parents were told that more than one player was needed and were asked if they would be willing to participate. Generally the response was enthusiastic. In order to generate the kind of data we needed, the rules conventionally employed in this game were modified. Two persons picked up sticks while the third acted as judge. The judge determined if a stick had been disturbed, keeping the stick touched as a penalty. The player with the most sticks at the end of each competition was the winner. After each round, the judge became a player and one of the players a judge; hence every person had a chance to be both player and judge. Before each round the participants were asked who would play and who would be judge. Careful note was taken of who spoke first, who decided, and who became judge. The principal purpose of this game was to ascertain how power was distributed within the family in terms of who allocated roles and how the decision was made.

Scoring Interaction

The subject's verbal and some non-verbal behavior (e.g., laughing, clapping, scowling) was scored according to a category system devised for this study, but based on a system I had used in earlier research in the United States (Rosen and D'Andrade, 1959). Instrumental and affective acts constitute the bulk of the interaction. Instrumental acts are intended to direct, control or change be-

havior toward a specific end (C), or avoid being directly controlled (C-). Affective acts seek to continue a person in a course of action (A), or to stop someone from acting in a certain way (A-). Two other categories of acts, independence (I), the active encouragement of self-reliance; and dependence (I-), the solicitation of help, occur infrequently. An act was defined as the smallest segment of verbal or motor behavior which could be recognized as belonging to one of the six categories in the system. Interaction was scored with the actor rather than the target of the act as to the observer's frame of reference. The category system is shown in Diagram 1.

This type of category system involves considerable skill on the part of the observer. Interaction is not easy to score even in a laboratory setting; scoring in the field is especially difficult. For this reason great care was taken in the selection and training of the field workers. Training sessions lasting several months were conducted in which the four teams of observers were provided with hypothetical family interaction protocols and asked to score them. Interaction settings with role players were also created, and the scorers asked to clarify a long series of

Diagram 1. A Category System for Scoring Family Interaction

C	Gives directions, suggestions, hints; seeks to control others.
A	Expresses approval; positive evaluation of performance; shows positive tension release, laughs and jokes.
I	Encourages others to be independent, do something by oneself.
I-	Seeks help or instruction as to what to do.
A-	Expresses disapproval; negative evaluation of performance; shows negative tension release, irritated gestures, coughs.
C-	Rejects directions, advice or suggestions; resists control from others.

rapid-fire interactions and to justify their decisions. The actual collection of data began when inter-scorer reliability ratings reached the .80 level. An analysis has been made of each observer's scoring; no systematic differences in their patterns of scoring have been detected.

Typically, a family visit lasted two to three hours, including time employed to establish rapport, administer the experiments and answer questions before leaving. The three urban samples were studied first, without any systematic priority being given to any of the groups; the rural sample was studied last. Observers who worked in the city also did the field work in the rural area. Periodically throughout the research the observers were accompanied by the field director or his assistant, and after the session meetings were held to discuss any problems encountered in the field. The response was excellent; only five percent of the families refused to participate, usually because of inability of the family to get together due to the demands of the husband's job. The data collection phase of the project required nine months of field work, beginning in the summer of 1968 and ending in 1969, late in the spring.

FINDINGS

Patterns of Interaction

Some dimensions of family structure can be discerned in the patterns of verbal interaction between family members as they engage in problem solving. The interaction data, as captured by the category system, provide information on the amount and type of behavior family members direct and receive from one another. Using the interaction data, I propose to examine group differences on three dimensions of family structure. First, *level of communication*—the sheer amount of each family member's participation in the problem solving process. These data reveal how overtly involved parents become in their child's performance, and inferentially tell us something about their inputs into the socialization process. Second, *degree of reciprocity*—the responsiveness of family members to one another. And third, *flow of affect*—the support and hostility problem solving generates in each fam-

ily. Data on these dimensions describe the form and quality of exchanges in a family, the patterned relationships that have developed between husband and wife, and between parent and child. It was expected that communication, reciprocity and positive affect would increase, and negative affect decline, in migrant families as the amount of time they lived in the city became greater.

Communication. The level of communication in the family was operationally defined as the amount of talking people do. Each participant's communication level was measured by summing the number of acts he made during the problem solving process. For the most part, the data in Table 1 confirm the studies about communication levels. Established migrant parents generate, on the average, more acts than parents in any other group. Indeed, the fathers in this group are the most talkative in the study. Established migrant mothers are also talkative; they speak twice as much as mothers in any other group. The boys in the established migrant group also talk more than do other boys, but like the other boys their participation level is low. Generally the boys in all groups tend to be preoccupied with performing the experimental tasks; they are the targets of much of the interaction in all four groups, the fathers speak more than any other family member: about twice as much as the mother, and far more than the son.

Although the amount of interaction among migrants increased with length of residence in the city, as expected, the overall relationship between level of communication and time in the city is curvilinear, not linear. Communication level is low among the rurals and recent migrants (interestingly, the recent

Table 1. Mean Number of Acts by Family Member and Group

Family Member	Rural	Recent Migrant	Established Migrant	Paulistano
Father	58.1	56.0	118.7	96.9
Mother	35.6	39.4	87.7	41.6
Son	.32	.80	1.3	.48
N =	34	44	46	43 (167)

migrants resemble the rural group very closely, suggesting that whatever selective factors were operative in the movement of the recent migrants to the city, they are not reflected in amount of family interaction), rises among the established migrants and drops again among the Paulistanos—the group with the longest period of residence in the city. This curvilinearity is a recurrent phenomenon, about which more will be said later.

Reciprocity. An important characteristic of family structure is reciprocity, operationalized here as the degree to which verbal exchanges between family members covary. The greater the level of reciprocity, the more open the family structure, for reciprocity indicates a willingness to receive as well as to send messages, some of which contain instructions involving an element of control. A measure of reciprocity between family members was obtained by correlating the amount of acts each person directed toward the others with the amount he received. To simplify the presentation of the data, only instrumental and affective acts in the block-stacking game are shown here. Acts of these types were by far the most frequent and the block-stacking game elicited much more interaction than any of the other task situations. (A separate analysis was made for the other tasks with very similar results.) The higher the correlation in any dyad (husband-wife, father-son, mother-son) the

greater the degree of reciprocity in that relationship. That is to say, the more a person speaks to the other, the more he is spoken to in return, although of course the absolute amount of acts may not be equal.

As can be seen in Table 2, only among the established migrants are reciprocity correlations high for all three dyads. Instrumental acts are weakly correlated with similar acts (C/C) for each of the dyadic relationships within the rural group; the correlations rise substantially among the two migrant groups and drop again for the Paulistanos. While reciprocity is relatively high between husbands and wives in the three groups living in the city, and between mothers and sons in the two migrant groups, only among the established migrants is there a high degree of reciprocity between fathers and sons.

Affect. Turning next to the emotional dimension of family interaction, I correlated instrumental and affective acts, and in this way obtained information about the affective structure of the family and the socialization process. The emotions aroused in the course of family interaction tell us something about how family members perceive each other's behavior, and how they feel toward one another. Since the majority of acts originated from the father, let us first look at how his wife and son respond to him (C/A, C/A-) when he behaves instrumentally. In the two migrant groups there is almost no

Table 2. Product-Moment Correlations between Amounts of Acts by Category Type in Four Groups of Families (Block-Stacking)

Type of Act	Rural				Recent Migrant				Established Migrant				Paulistano			
	C/C	C/A	C/A-	A-/C	C/C	C/A	C/A-	A-/C	C/C	C/A	C/A-	A-/C	C/C	C/A	C/A-	A-/C
Father/Mother	.08	.04	.62*	-.09	.71*	-.05	NC	NC	.66*	-.01	.11	.08	.41*	-.04	.33*	.02
Father/Son	.06	NC	NC	.48*	.15	NC	NC	NC	.64*	.09	NC	.07	.18	-.08	.10	.34*
Mother/Son	.16	.04	.08	.01	.75*	.08	NC	NC	.45*	.01	NC	-.07	.13	.05	.10	.34*

Legend C = Instrumental (control) acts
 A = Positive Affective acts
 A- = Negative Affective acts
 / = Correlated with
 * = Coefficient significant at .01 level
 NC = No correlation. One of the actors not emitting enough acts to warrant correlation analysis (less than 5 acts).

affective reaction to the father's instrumental behavior from either the mother or the son. They seem to accept directions from him without hostility, perhaps because his instrumental behavior is perceived as contributing positively to the accomplishment of the task, rather than as merely an expression of patriarchal control. Among the rural families, the wife's (though not the son's) negative affect ($C/A -$) increases as her husband becomes more instrumental. Her negative responses, it seemed to the observers, reflected the wife's irritation with her husband's lack of competence in this situation. Paulistano women, like their rural counterparts, also show more negative affect as their husbands' instrumental acts increase, but in this case the hostility appeared to develop out of the wife's resentment at the husband's efforts to control her behavior.

The boys' instrumental behavior is accepted in both migrant groups; an increase in this type of behavior does not elicit more negative affect ($A -/C$) from either parent. This is in sharp contrast to the rural group. Rural fathers do not like taking instructions from their sons; the father's negative affect increases with his son's efforts to control the situation ($A -/C$). The mothers are more accepting and more supportive, as might be expected in Brazilian society where the culture places a strong emphasis on the nurturant mother role (Rosen, 1962). This makes the behavior of the Paulistano parents all the more surprising, for neither parent is particularly receptive to instrumental behavior by the son. Paulistano fathers and mothers respond with increased hostility as the amount of instrumental behavior by the boy rises; they are not receptive to efforts on his part to control them, implicitly challenging their authority.

Achievement

Migrant parents were expected to place greater emphasis on achievement as the length of their residence in the city increased. Parents stress achievement when they express high expectations of their son, indicate a high evaluation of his competence to do a task well, and impose on tasks standards of excellence against which the

boy is expected to compete. Measures of parental expectations and evaluations were obtained from the estimates and choices made by the parents in the block-stacking and tinker toy tasks. In the first task the parents (and the boy) were asked how well their son would do in a situation involving competition against a stated performance norm; in the second case they were asked to choose a toy for the boy to construct from among three designs, each clearly graded by level of difficulty.

On first inspection the data provide little support for the notion that expectation levels would increase among the migrants over time. A slight difference was found in the tinker toy game: none of the rural parents chose Design 2, while a small number of the established migrant subjects did; but most subjects chose Design 1, the easiest. Virtually no differences exist between the four groups in their estimates for the boy's performance in the first trial of the block-stacking game. The subjects responded, for the most part, by staying close to norm; the mean estimate for the fathers and sons across all four groups is approximately 5.5 blocks; the mothers' estimates are somewhat lower; they tended to make conservative judgments of how well their boys would do.

Estimates for the second trial of the block-stacking tasks present a different picture; they provide a highly complex measure of expectation and evaluation levels. The second trial differs substantially from the first in that everyone has more information than was previously available. To begin with, there is now a known level of performance against which future estimates can be made. Once having observed the boy's performance, parents can make a more realistic evaluation of their son's competence and of the task's difficulty. But more is known after the first trial than merely how well the boy has done. Since first trial estimates were made public, each person now knows how skillful the other two members of the family think the boy is at performing a task under difficult circumstances. Thus, second trial estimates should tell us something about several important elements in achievement training: that is, the parent's expectation of how well the boy will do in a situation containing

a standard of excellence, an example of past performance and a private and public evaluation of his competence.

Undoubtedly all of these elements enter into a complex assessment which precedes the second estimate. Separating out the variables in this process, when making comparisons between the four groups of families, is difficult. The task is made even more complicated by the fact that the average performance level of the boys in the four groups is different. Established migrant boys built the highest towers, 8.0 blocks, on average; the average scores for the other groups were: Paulistanos, 7.6; recent migrants, 7.5; rural boys, 5.4 blocks. These differences emphasize the importance of taking the boys first trial performance into account when assessing the meaning of the parent's second estimate. The problem of understanding what went into the subject's second estimate can be approached by asking the question: On what apparently are the subjects basing their estimates? Or in the language of regression analysis: How well does the norm, the boy's performance on the first trial and everyone's previous estimate predict the second estimate of each family member? Answers to these questions can be found in Table 3 which shows the partial regression coefficients associated with each predictor variable.

Consider, first, the four groups of fathers. Perhaps the data's most striking feature can be found in an examination of how well the father's second estimate is predicted by the

son's previous performance. The partial coefficients are indications of the impact the boy's performance has had on the father's expectations: the higher the coefficient the more responsive the father appears to be to the boy's behavior, the more attention he seems to be paying to what the boy is actually doing. As Table 3 shows, the established migrant father's expectation (second estimate) is best predicted by the son's first performance. Of all the fathers, the established migrant is the most responsive to the boy's performance. More flexible than the other men, the established migrant father is least attached to his previous estimate and most willing to respond to the actual situation. Recent migrants also appear to be acting in a pragmatic way, combining the norm (an expert's opinion) with the evidence of their own eyes (the boy's performance) in arriving at an estimate. Among the rural and Paulistano families, the boy's performance has no predictive power so far as father's second estimate is concerned. Nor do the estimates of the wife and son carry much weight. Rural and Paulistano fathers use their own first estimates in arriving at a second estimate, paying little attention to what the boy did. It appeared to the observers that these fathers were primarily concerned with asserting their importance through a reaffirmation of previous estimates. The rigidity with which Paulistano and rural fathers clung to their previous estimates, as though a change in their opinion might be

Table 3. Multiple Partial Regression Coefficients Predicting for Second Estimate in Block-Stacking Task

		Norm	Father's 1st Est.	Mother's 1st Est.	Son's 1st Est.	Son's 1st Performance
Fathers	Rural	.34	.58*	.19	-.08	.17
	R. Migr.	.38*	.38*	-.13	.32	.33*
	E. Migr.	.42	.23	.06	.13	.43*
	Paul.	.70*	.47*	.05	-.14	.28
Mothers	Rural	.04	.15	.27	.19	.52*
	R. Migr.	.78*	-.02	.18	-.29	.56*
	E. Migr.	.53*	.01	.41*	-.05	.43*
	Paul.	.38*	.18	.13	.07	.45
Sons	Rural	.25	-.10	-.10	.31*	.74*
	R. Migr.	-.31	.23	.25	.23	.64*
	E. Migr.	-.54	.22*	.38*	.28*	.58*
	Paul.	-.10	.39*	-.19	.40*	.54*

* Coefficients significant at .01 level.

interpreted as a sign of weakness, reflects an emphasis on male certitude that is especially strong in the rural areas of Brazil, and not uncommon in the city as well.

A pragmatic orientation appears to be the hallmark of the mothers. Most mothers pay attention to what the boy has done; they tend also to take seriously the significance of the norm, except in the case of rural mothers. Established migrant mothers, and to a lesser degree the rural mothers, tend also to place some emphasis on their own first estimate. The mothers in all four groups were less likely than the fathers to stress achievement: their expectations were lower; they revised their estimates more cautiously; they tended to stay close to the boy's previous performance, not pushing too far beyond what the experts reported as the norm.

The best predictor of the boy's second estimate is his own previous performance. Stacking blocks in a difficult situation has made him a realist. Among the established migrant boys the judgments of both parents appear to carry some weight; Paulistano boys seem to have been influenced by their fathers; the coefficients for recent migrants are modest, but not significant. It is noteworthy that neither the father's nor the mother's first estimate has any value whatsoever in predicting the rural boy's second estimate. What does the rural boy's seeming indifference to his parent's opinions represent? Not, in my opinion, a rejection of his parents, even less an expression of independence, but a failure in communication. Rural parents were often painfully inarticulate, saying little or nothing as the boy built his tower of blocks. We know from Table 1 that they spoke less than the parents in the other groups. In this respect, the rural parent fits a familiar Brazilian stereotype: the laconic peasant—a comic figure in a society which admires verbal facility. In part, at least, parental influence was least effective in the rural group because the parents had difficulty expressing themselves. Except in situations involving a simple assertion of parental authority, rural parents often were unable to clearly state what they expected their son to do, and why.

These data suggest that the impact of industrial-urban life on the expectation levels of migrant parents does not reveal it-

self, at least at first, in the height of the parent's expectations. After all these are poor, mostly uneducated people; when faced with an unfamiliar task they tend to be conservative in estimating what their sons can do. The urban milieu has its greatest effect on the sensitivity of family members to one another's behavior, and the skill with which expectations are expressed. In this respect the established migrant fathers appeared to have been especially affected; they are attentive to what the boy is doing and willing to change their estimates in response to his performance. The significance of this for achievement training is that standards of excellence are much more likely to be internalized when parental expectations reflect a realistic and sensitive appraisal of a task's difficulty than when the expectations appear arbitrary and unresponsive.

Equality and Independence

Life in the city was expected to make the migrant family more egalitarian. In contrast to the rural family with its tradition of patriarchal dominance, I expected boys in migrant families would have more independence, and women would exert more influence in decision-making. The decision-making situations in the experimental tasks provide measures of equality and independence—the power to make one's own choice in a competitive situation. One measure of decision-making power is a simple enumeration of the number of times the subject made the decision in each of ten different situations in the Pick-Up-Sticks game. The average scores for the fathers, mothers and sons in each of the four groups are shown in Table 4; the higher the mean score the greater the power to decide what would be done in the game.

The data in Table 4 show that migrant boys have more independence (i.e., higher decision-making scores) than rural boys, or even Paulistanos. Indeed, *the established migrant boys made decisions more often than either of their parents*. In the other three groups the father tends to be the dominant figure; this is especially evident in the rural group, as expected. Decision-making in all four groups tends to be a male (father and son) prerogative; the mother on the

Table 4. Mean Decision-Making Scores in Pick-Up-Sticks Game

Family Member	Rural	Recent Migrant	Established Migrant	Paulistano
Father	4.23	4.14	3.30	4.05
Mother	2.38	2.07	2.48	2.60
Son	2.91	3.66	3.74	3.00
N =	34	44	46	43 (167)

average is the least powerful family member—rural, migrant or native urban. The generally subordinate role Brazilian culture assigns to women has been noted before (Rosen and LaRaia, 1972); but this is the first time, to my knowledge, that the decision-making aspect of family structure has been assessed by observing and measuring family interaction.

A measure of decision-making power, if accurate, will indicate whose will is decisive in a situation involving a choice among several alternatives; but it will not necessarily tell us what that choice will be. And yet, the choice itself reveals much about the *influence* family members exert on one another. The distinction between power and influence in decision-making can be seen in the third trial of the block-stacking task when we examine *what decision was made*, rather than who made it. This was a risk-taking situation in which the family was asked to make a group decision as to how high the boy should attempt to build his tower, having first made private estimates. We can get some idea of an individual's influence by asking: Whose third estimate best predicts the choice that was finally made? The answer can be seen in Table 5.

Among the established migrant families, it is the father's third estimate which best predicts the final group choice. Apparently the son (and the wife) accepted the father's judgment as to how high the boy should attempt to build his tower. What does this indicate about the father-son relationship among established migrant families? The son's submission to parental authority? A lack of confidence in deciding what to do? I think not. Established migrant boys enjoy more independence, talk more, and are more

Table 5. Multiple Partial Regression Coefficients Predicting for Joint Choice in Third Trial of Block-Stacking Task

	Father's 3rd Est.	Mother's 3rd Est.	Son's 3rd Est.
Rural	.18	.22	.55*
R. Migrant	.34*	.21	.46*
E. Migrant	.64*	.28	.09
Paulistanos	.36*	.37*	.23

* Coefficient significant at .01 level

competent at problem solving than boys in any other group. I believe the established migrant boy used his father's estimate because he respected his father's judgment, a respect based on the son's evaluation of his father's competence, commitment to the game, and sensitivity to what was going on. Established migrant fathers were noteworthy for their enthusiasm during the experiment; they talked more, and were particularly skillful at giving instructions. Under these conditions it is not surprising that the boy chose his father's estimate (i.e., internalized his father's expectation level) in preference to his own.

The situation is exactly the reverse in the two least urbanized groups (rurals and recent migrants). In these groups it is the son's third estimate which best predicts the final group choice! The rural father's third estimate has virtually no predictive power. This finding seems anomalous when we remember that rural fathers were more authoritarian than fathers in the other groups. It might be expected that they would use their power to insist upon their estimate being the final choice. Yet they did not. It is as though the rural father, having publicly asserted his authority to make the decision, indulged his son by letting the boy's choice be the final one. But this type of relationship does not enhance a boy's sense of independence. Rather, it increases his dependence on seemingly indulgent authority. I have suggested elsewhere that this kind of parent-child relationship is one source of a tendency among adult Brazilians to expect support from power figures (e.g., the *fazendeiro*, the *coronel*) in return for

their submission to absolute authority (Rosen, 1964; see also, Moog, 1964).

An almost egalitarian situation exists among the Paulistanos: the husband and wife have very similar regression coefficients; the son's estimate has less weight but it also significantly predicts the final estimate. This pattern fits our expectation of greater equality among families with the longest period of residence in an urban environment. The mother's coefficient is particularly interesting; it is higher than the coefficient of the fathers—though not significantly so—despite the fact that the fathers make decisions more often than other family members. Possibly the mother's coefficient represents a form of influence found in structures where power is concentrated in the male role; women learn to exert influence without openly challenging their husbands. Perhaps urban life makes women more skillful at exercising influence in this type of family structure.

CONCLUSION

In the context of this study, what can now be concluded about the impact of urban life on family interaction and parent-child relations in migrant families? Focusing first on family configurations, the data show that interaction within migrant families becomes more open and responsive as their length of residence in the city increases. Relationships between husbands and wives become more egalitarian; children enjoy more independence. Transactions in established migrant families, in particular, are characterized by relatively low levels of hostility and fewer reciprocal efforts to control the behavior of others.

An emphasis on achievement is visibly greater among migrants the longer they have lived in São Paulo. Established migrants—in contrast with the rurals and recent migrants—become deeply involved in the experimental tasks; they talk animatedly, exchange information freely and seem to enjoy the competitive challenge associated with each new problem. This high level of parental involvement, coupled with an open, non-hostile family structure, explains in part why established migrant boys do so well at stacking blocks. The parents in this group work

with the boy and with one another, exchanging information and relating to each other smoothly and efficiently like three well meshed gears; turning one turns the other, even though the size and function of the gears are different. Interestingly, a high level of parental involvement is a salient characteristic of American families with highly achievement motivated boys (Rosen and D'Andrade, 1959).

Parents in some migrant families perform complementary roles in the socialization process. In these families the mother's behavior is primarily directed toward protecting her son; the father's behavior emphasizes achievement, as reflected by his involvement in the tasks and responsive reactions to the boy's performance. The combination of maternal nurturant behavior and paternal achievement training increases achievement in boys—at least in Brazil. Maternal support in a culture where paternal dominance is common makes the father's stress on performance less threatening to the boy; while paternal emphasis on achievement keeps the boy sensitive to the importance of competing against standards of excellence. This complementary role relationship is significant for the development of achievement among boys in established migrant families. For in these families, the mother's traditional nurturant behavior blends with a new emphasis on achievement by the father; the effect is a boy who performs better. (This complex role relationship might not have been identified had I followed the common practice of interviewing or observing only one of the two parents.)

Finally, the relationship of time in the city to family structure and socialization is more often curvilinear than linear. Thus, an emphasis on achievement and independence is least evident in the rural group, becomes more apparent among recent migrants, increases further in the established migrant sample, but declines in the group with the longest period of residence in the city—the Paulistanos, contrary to our expectations. This expectation was based on an assumption that length of urban residence would provide an adequate—albeit rough and ready—index of experiences in the urban ambience. But logically, length of urban residence

relates more to exposure to the urban milieu than experiences there. And yet the analytical model argues that the experiences associated with an improvement in life conditions and chances are critical in changing traditional patterns of family interaction.

Lower class Paulistanos have not had these experiences. They have not benefited from the great structural mobility produced by São Paulo's rapid industrialization. Born in the lower class, they remain at the bottom of the heap—immobile, non-achievers in a growing economy. The established migrants—the group whose behavior most closely approximates the theoretical model—have in fact experienced a change for the better in the city, particularly when contrasted with their previous existence in the village. Objectively, the established migrant's position in the social structure is no different from that of the Paulistano; the two groups have the same standard of living. Subjectively, the difference is critical. For the meaning of that position and the experiences associated with attaining it, are different. In giving the migrant a chance to improve his position in life, the industrial city has made it possible for him to experience success. The results of this experience are an increased sense of efficacy, new values, and different perceptions of how the world is organized. When these changes in personal orientation and perspective occur, the family—at first the target of change—becomes its active agent. New patterns of family interaction develop, characterized by more equality, increased openness and responsiveness, and a greater concern with achievement.

REFERENCES

- Bell, Richard Q.
1968 "A reinterpretation of the direction of effects in studies of socialization." *Psychological Review* 78 (March):81-95.
- Carmago, Jose Francisco de
1966 *Exodo Rural no Brasil*. Rio de Janeiro: Conquista.
- Elder, Glen H.
1965 "Role relations, sociocultural environments and autocratic family ideology." *Sociometry* 28 (June):173-96.
- Foster, George M.
1965 "Peasant society and the image of limited good." *American Anthropologist*, 67 (April):293-315.
- Germani, Gino
1961 "Inquiry into the social effects of urbanization in a working class sector of Greater Buenos Aires." Pp. 206-34 in P. Hauser (ed.), *Urbanization in Latin America*. New York: Columbia University Press.
- Hutchinson, Bertram and Carlos Castaldi
1960 "A hierarquia de prestigio das ocupações." B. Hutchinson (ed.), *Mobilidade and Trabalho*. Rio de Janeiro: Centro Brasileiro de Pesquisas Educacionais.
- 1963a "The migrant population of urban Brazil." *America Latina*, VI, no. 2 (April-June): 41-72.
- 1963b "Urban social mobility rates in Brazil related to migration and changing occupation structure." *America Latina* Vol. no. 3 (July-September):47-61.
- Inkeles, Alex
1966 "The Modernization of Man." Pp. 138-50 in M. Weiner (ed.), *Modernization*. New York: Basic Books, Inc.
- Kahl, Joseph A.
1968 *The Measurement of Modernism*. Austin: University of Texas Press.
- LeVine, Robert A., Nancy H. Klein and Constance F. Owen
1968 "Modernization and father-child relationships." Pp. 558-74 in N. Bell and E. Vogel (eds.), *Modern Introduction to the Family*. New York: Free Press.
- Lewis, Oscar
1959 *Five Families*. New York: Basic Books, Inc.
- Lopes, Jaurez R. B.
1968 *Desevolvimento e Mudanca Social São Paulo*: Editora Nacional.
- Moog, Vianna
1964 *Bandeirantes and Pioneers*. New York: Braziller.
- Pastore, José
1969 *A Cidade E O Homem: Migração Adaptação E Planejamento Urbana*. São Paulo: Editora Nacional.
- Peterson, D. and G. Migliorino
1970 "Pan-cultural factors in paternal behavior in Sicily and the United States." Pp. 219-46 in K. Danziger (ed.), *Readings in Child Socialization*. London: Pergamon Press.
- Prothro, Edwin T.
1961 *Child Rearing in The Lebanon*. Cambridge: Harvard University Press.
- Osofsky, Joy
1971 "Children's influences upon parental behavior: an attempt to define the relationship with the use of laboratory tasks." *Genetic Psychology Monographs* 83:147-69.
- Rosen, Bernard C. and Roy G. D'Andrade
1959 "The psychosocial origins of achievement motivation." *Sociometry* Vol. 22, no. 3 (September):185-218.
- Rosen, Bernard C.
1962 "Socialization and achievement motivation in Brazil." *American Sociological Review* Vol. 27, no. 5 (October):612-24.

- 1964 "The achievement syndrome and economic growth in Brazil." *Social Forces* Vol. 42, no. 3 (March):341-54.
- Rosen, Bernard C. and Alan Simmons
1971a "Industrialization, family and fertility: a structural-psychological analysis of the Brazilian case." *Demography* Vol. 8, no. 1 (February):49-69.
- Rosen, Bernard C.
1971b "Industrialization, personality and social mobility in Brazil." *Human Organization* Vol. 30, no. 2 (Summer):137-48.
- Rosen, Bernard C. and Anita LaRaia
1972 "Modernity in women: an index of social change in Brazil." *Journal of Marriage and The Family* Vol. 34, no. 2 (May):353-60.
- Safilios-Rothschild, Constantina
1970 "The study of family power structure: a review of 1960-1969." *Marriage and The Family* 32 (November):539-52.
- Shirley, Robert W.
1970 *The End of a Tradition: Cultural Change and Development in the Município of Cunha, São Paulo*. New York: Columbia University Press.
- Straus, Murray
1968 "Communication, creativity and problem solving ability of middle and working class families in three societies." *American Journal of Sociology* Vol. 73, no. 4 (January): 417-30.
- Strodtbeck, Fred S.
1958 "Family interaction, values and achievement." Pp. 135-94 in D. McClelland (ed.), *Talent and Society*. Princeton: Van Nostrand Co.

NEW DIRECTIONS IN THE STUDY OF COMMUNITY ELITES *

EDWARD O. LAUMANN

University of Chicago

FRANZ URBAN PAPPI

Universität zu Köln

American Sociological Review 1973, Vol. 38 (April):212-230

Recent work in the study of community decision-making appears to be converging on a number of common theoretical and methodological strategies and assumptions. There still remain, however, important weaknesses in the overall theoretical framework and its implied methodology in directing research efforts. Attention is directed to a structural analysis of the community influence system that derives in part from Parsons. Several critical questions are raised concerning the identification of the relevant set of community influentials and the systematic description of their attributes as influentials and the ties that bind them into coalitions depending on the functional issue confronted. Recent advances in graph theory and smallest space analysis are used to examine the consensus-cleavage structure of the community influence system of Altnestadt, a small city in West Germany. Finally, a theoretical strategy and an empirical procedure are proposed for identifying community issues and tracing their impact on the formation of opposing factions and coalitions.

EVEN a cursory review of recent literature on community decision-making systems (cf. Clark, 1968a; Aiken and Mott, 1970; Bonjean et al., 1971) impresses the reader with the number of promising

developments in the field. After years of rancorous conflict on methodological issues concerning the *best* way to study the subject (cf. Walton, 1966a, 1966b) and on the relative merits of ruling elite and pluralist

* A version of this paper was originally read at the 67th Annual Meetings of the American Sociological Association, New Orleans, Louisiana, August 28-31, 1972.

We wish to acknowledge with deepest appreciation the advice, encouragement, and help of the following people: Professor Erwin K. Scheuch and Dr. Hans-Dieter Klingemann of the University of Cologne, our research assistant in Germany, Dipl. Soz. Regina Perner, who contributed much to the successful design and implementation of our data-collection procedures, and our American assistants, Richard Senter, John Blair, William Roy, Lois Verbrugge, and Daniel Ayres, of the University of

Michigan, who have provided invaluable assistance in the data-analysis phase of the project. We are especially appreciative of the facilities provided by the Zentralarchiv für empirische Sozialforschung under Professor Scheuch's direction as well as the Center for Research on Social Organization at the University of Michigan. Financial support from the following organizations is gratefully acknowledged: Landesamt für Forschung im Ministerium für Wissenschaft und Forschung des Landes Nordrhein-Westfalen, Ford Foundation Behavioral Science Postdoctoral Fellowship (held by the senior author in 1970-71), and the National Science Foundation (GS-32002).

models, investigators have begun to assess alternative strategies in designing new studies. The emphasis of the 1950s and early '60s on qualitative case studies, usually of one community at one point in time, following the classic leads of Hunter (1953) and Dahl (1961), has shifted to comparative and quantitative foci in which the objective is to study as many communities as possible, using a wide range of quantitative data.

The contemporary emphasis tends at times to be excessively empirical and pays insufficient attention to theoretical issues. Nevertheless, a fairly explicit theoretical model underlies current efforts—namely, an open-ended system, input-throughput-output model, of community decision-making (cf. Clark, 1968b, 1968c, 1968d). Figure 1 reflects reasonably well the accounting scheme employed in a number of recent and ongoing studies (see, e.g., Clark, 1968b:18; Downes, 1968). This open-ended model posits that certain features of communities, such as population size, regional location, age, industrial and economic base, population stability, and economic and ethnoreligious heterogeneity (i.e., "inputs"), together with attributes of their political institutions, are associated with or determine certain features of their decision-making apparatus, such as the degree of centralization or diffusion of decision-making (i.e., "throughput"). These, in turn, determine which issues will be brought to decision and the decision outcome (i.e., "outputs"). Since "hard" data on inputs and outputs are more readily available and less ambiguous than information regarding the nature of the decision-making ap-

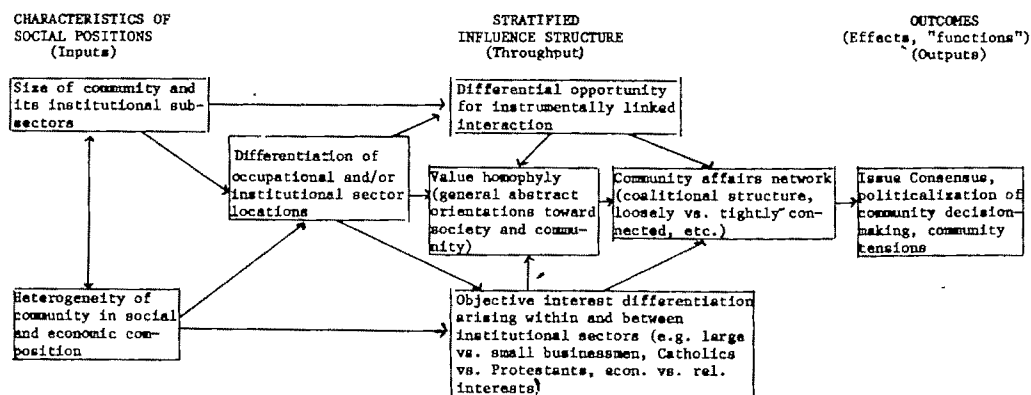
paratus itself, the tendency has been to treat the throughput or "elite decision-making core"—the central object in earlier case studies—as a relatively unobservable "black box" about which only inferences or approximations can be made.

Since the contents of this black box will be the central concern of this paper, we must be especially careful in conceptualizing and measuring social structure—in this instance, the influence structure on the elite level of a community social system. Structure and related descriptive terms such as hierarchy, dominance, differentiation, structural change, and power or class structure, are among the most popular concepts in the sociological lexicon. Despite differences in nuance associated with "structure," the root meaning refers to a persisting order or pattern of relationships among units of sociological analysis, be they individual actors, classes of actors, or behavioral patterns (cf. Nadel, 1957:1-19; Mayhew, 1971; Laumann, 1973). This apparent consensus masks the lack of agreement on the concepts and methodology in terms of which given "social structures" are to be measured, or more modestly, described. Without this agreement, researchers can hardly turn to the more challenging problems of describing structural change.

A TOPICAL OVERVIEW

Our approach thus may appear to be a step backwards inasmuch as we want to describe the theoretical and research strategies of an intensive case study of one small city, Altneustadt (a pseudonym) in West Ger-

Figure 1. An input-throughput-output model for the analysis of community decision making systems.



many. By redirecting attention to the black box, we hope to elucidate the mechanisms whereby inputs are converted into outputs. Although we shall present some substantive results, we wish to stress the more general implications of our theoretical and methodological approach for community elite research. Consequently, we may sometimes be overly brief about the detailed empirical procedures used.

The discussion will proceed in two parts. First, we shall sketch a frame of reference for delineating community influence systems. The community elite is viewed as a set of incumbents of theoretically identified categories of social positions. Description of the structure which results when these positions are linked in a pattern of specified relationships will be a central concern. Two features of individual elite members will be noted: (1) their primary and secondary locations in functionally defined institutional sectors, and (2) their relative influence statuses. Systematic propositions about their respective distributions in the influence structure will be advanced. A methodology will be described that is designed to generate a theoretically relevant description of this influence structure. Second, we shall discuss the structure of conflict in community influence systems. It is difficult to imagine a community comprised of a socially and economically heterogeneous population that lacks disagreements regarding the allocation of scarce community resources. More formally, being good Parsonsians, a central premise of our analysis is that conflict is an endemic, necessary feature of *any* community decision-making apparatus, posing the fundamental functional problem of integration for such structures, that is, the problem of establishing binding priorities among competing goals. Consequently, using a theoretically grounded strategy for identifying community issues and tracing their impact on the formation of opposing factions and coalitions, we shall study how conflict or cleavage patterns are superimposed on the elite structure. We shall first briefly describe the community context which provides the empirical basis for our discussion.

The Community Context: Altneustadt

Altneustadt is a town of 20,000 inhabitants which is not dominated by a nearby larger

city. The town is in rich farming country and serves the needs of a large agricultural hinterland. It is the district (county) headquarters for a range of governmental agencies with a correspondingly large number of public officials and bureaucrats. It also has a number of small and intermediate light manufacturing plants owned by local businessmen. Thus, the town has long had a fairly diversified, mainly "middle class" occupational composition. About fifteen years ago, the state government decided to build one of Germany's largest natural science research centers in Altneustadt. This center is now the largest and most important employer in the community.

The Research Center brought a major migration of people to Altneustadt. Approximately one third of the inhabitants are *Neubürger* with status characteristics greatly different from those of the *Altbürger*. Being for the most part university-educated and highly salaried workers, often of urban origin, the *Neubürger* have strong cosmopolitan values and perspectives. In addition, they tend to be Protestants while the *Altbürger* are predominantly Catholics. These basic differences in world views and life styles have led to many conflicts and tensions related to accommodating (if not assimilating) the newcomers. These conflicts have resulted in clearly delineated and perceived coalitions and interest structures which provide the setting for a study of "status" as opposed to "class" politics (cf. Lipset, 1963), since most major groups share "middle-class" socioeconomic status positions but differ fundamentally in their conceptions of appropriate status behavior and styles of life. This, in fact, is the basis of our expectation that the central axis of structural cleavage in Altneustadt will be in the pattern-maintenance rather than the economic or adaptive sector.

Of particular interest is the fact that the SPD (German Social Democratic Party), a party historically rooted in a working-class and predominantly Marxist world view, has been co-opted by the *Neubürger* as the vehicle for expressing their urban, secular, middle-class demands for social change in Altneustadt. Natural scientists and engineers have not been traditional recruits for the SPD. The Christian Democratic Union (CDU) has proved to be a reliable vehicle of control for the *Altbürger*, who maintain a

dominant, but increasingly insecure political coalition.

SOCIAL POSITIONS AND INCUMBENTS

A. Identifying Community Influentials and Their Respective Institutional Sectors

For our purposes, the unit of structural analysis will be the individual actor (or set of actors) in a particular kind of social position (cf. Parsons, 1951). We thus come to the first crucial question: how are we to identify the domain of relevant social positions for the community influence system? Objections to the reputational and issue approaches to identifying elite personnel are well known. Adherents of the reputational technique argue that the issue approach is conservative insofar as it is impossible to detect the impact of nondecisions on the *status quo*. Adherents of the issue approach retort that the reputationalists measure reputations rather than power. Both groups, however, are asking the same question: who governs? The structuralist, or positional approach asks instead, "which positions possess authority or generalized influence in that their incumbents can make binding decisions in their respective institutional sectors or will be consequential in the resolution of community-level issues?" Generally, following Parsons' view of the community as a territorially grounded social system embracing all aspects of social life (cf. Davis, 1948:312; Parsons, 1960:250-79), we first identified prospective community influentials as incumbents of the highest positions of authority in organized collectivities whose primary responsibilities are in one of the four functionally specialized institutional subsectors at the community level of analysis (see Clark, 1968c, for a recent exposition of the AGIL paradigm applied to community institutions; also D'Antonio et al., 1961, for a less theoretically grounded, more "commonsensical" listing of types of community leadership personnel).¹ Parsons (1960:59-

69) argues that there are three levels in the hierarchical structure of organizations: the technical, the managerial, and the institutional. The last is concerned with the articulation of the organization with its larger institutional environment, both by securing its legitimacy in the community and making its claims on scarce community resources, often at the expense of other organizations' claims. On precisely these grounds we can analytically treat the community influence structure as the focus of the integrative subsystem of the community.

Not all community subsystems are likely to be organized into a structure of fully institutionalized and functionally specialized organizations with a full complement of explicitly identified leaders. This is especially true in the integrative and pattern-maintenance sectors of the community which tend to have more fluid organization. We attempted to compensate for this bias of the positional approach by supplementing our list of prospective influentials with nominations by well-informed community members of community influentials not in formally recognized positions.

The distinction we maintain between a social position and the particular actor who occupies that position is crucial. In general, incumbents of "influential" positions spend most of their time devoted to the tasks associated with these positions. But empirical analysis is complicated by the fact that a

with *adaptive* primacy; top governmental administrative positions, judges, and legislative decision-making bodies as having *goal-attainment* primacy because they make binding decisions for the community as a whole; voluntary associations including unions and political parties as having *integrative* primacy as foci of interest group demands on the polity; and positions in educational, health, religious and cultural organizations as having *pattern-maintenance* primacy. *Notars* in Germany are a specialty in the legal profession concerned with economically relevant activities, such as the preparation of contracts and property transfers, and, consequently, were treated as in the adaptive sector. Although the Natural Science Research Center is the largest employer in Altnestadt, having many important economic consequences, we decided to code it as a pattern-maintenance collectivity, both because its goal objectives are themselves distinctively cultural in their focus and consequences and because, from the community's viewpoint, it poses the problem of the assimilation of its personnel with their distinctive cultural characteristics into a more inclusive pattern of community life.

¹ Parsons' AGIL paradigm was used as the analytic framework for classifying organizations according to their primary functions in the community social system. Given the abstract character of his original formulations, there are some operational difficulties in coding organizations as belonging primarily to one of the four sectors. We coded business firms and banks as economic organizations

given actor may simultaneously occupy several "influential" positions in community decision-making—that is, he may wear several hats. We propose to deal with multiple role occupancy operationally by distinguishing an individual's *primary* institutional location or position from his *secondary* position(s) on the basis of the amount of time he spends performing the duties of each.²

B. The Rank Order of Influence

Guided by these principles we obtained a list of fifty-one community influentials in Altneustadt, of whom forty-six were successfully interviewed. We then asked: what is the relative influence status of these influentials? That is, can they be differentiated into a hierarchy of influence? This has been a classic concern, especially among those using the reputational approach. Procedurally, we simply asked our influentials to indicate those on the list whom they considered "now in general very influential in Altneustadt" and rank-ordered the number of votes received by each person on the list.³ There is remarkable consensus among the forty-six respondents concerning the top seven influentials, the top three of whom re-

ceived forty-six and thirty-seven votes respectively. When we asked Herr K., who was unanimously regarded as "very influential," to name the most influential person in the community, he replied, "*Das bin ich.*"

In an effort to validate this influence rank-order at least indirectly, we considered the following evidence. At the beginning of the interview before any mention of our list of influentials, respondents were asked to name persons and groups perceived to be on the supporting and opposing sides of five major community issues. Most people mentioned frequently were on our influentials list. Thirty-eight persons mentioned were not included on that list. However, all were seldom mentioned and only for one issue. We simply multiplied the number of times each person was mentioned as being on one or the other side of an issue by his influence rank (assigning a rank-order of 55 to persons not included in the original list), summed the resulting numbers for each side, and divided by the total number of mentions on the respective side. This number can be regarded as the average influence status of proponents or opponents—the lower the number, the higher the average influence status. (See Table 1.) We were able to predict correctly the winning side for all five issues ($p = .03$) by picking the side with the higher average influence status.⁴

SOCIAL RELATIONSHIPS

A. The Theoretical Rationale for Describing Community Influence Structures

When we consider how individual influentials interact with one another, we become interested in describing the structure of their interrelationships. It is to this structural analysis that we now turn.

Social structure will be defined as a persisting pattern of social relationships among social positions (cf. Laumann, 1966; especially 1973 for an extended theoretical rationale). A *social relationship* is any link

² People who spent most of their time in non-authority positions were coded separately, and thereby distinguished from individuals whose primary positions of authority were in economic, political, voluntary association, science center, religious or educational/cultural organizations.

³ Two different questions were asked to measure general influence rank. First, as already discussed, the respondents were asked to name all persons they would say "are now in general very influential in Altneustadt." Second, they were asked to indicate the top three persons from those they had identified in order of their community influence. The rank-order correlation between influence status on the basis of the simple number of mentions and on the basis of a weighted sum of nominations for the top three influentials is .84 ($N=31$). Given the high correlation between the two procedures and the fact that the "simple mentions" method provided an order for the entire population while the "top three" method covered only the top thirty-one persons, we decided to use the simpler measure as our measure of influence status.

Each respondent was also asked to name other people he felt should be included in our list of community influentials. While a number of suggested additions were made, all but one were mentioned only once. The exception received five nominations and was, consequently, added to our list and interviewed.

⁴ By looking at means and standard deviations of the influence ranks attached to each side of an issue, we can also assess the degree to which a given issue tended to be confined to the higher reaches of the set of influentials (i.e., an internal elite disagreement) or was a broader-based community issue which involved the mobilization of personnel outside the top influential group.

Table 1. The Average Influence Status of Proponents and Opponents on Five Community Issues, with Standard Deviations and the Winning Sides Indicated by Asterisks

Issue	Proponents		Opponents	
	Average Influence Status	Standard Deviation	Average Influence Status	Standard Deviation
Adaptive issue primacy: Industrial resettlement	13.7*	18.2	18.9	13.2
Goal-attainment issue primacy: Construction of new city hall	7.4*	14.8	22.5	14.9
Integrative issue primacy: Community annexation	10.6*	15.2	50.5	14.6
Pattern-maintenance issue primacy: Secular vs. confessional school	26.6*	17.8	28.3	23.3
Permission to hold Pop-festival	29.2	21.0	15.8*	18.1

between incumbents of two social positions that involves mutual but not necessarily symmetric orientations and activities (cf. Homans, 1951; Parsons, 1951; Blau, 1964). If *social differentiation* is defined as the differing allocation of tasks and responsibilities among positions in a social system, then a *differentiated social structure* is one whose actors tend to confine their consensual relationships with others performing similar tasks. In other words, similar positions will tend to cluster, that is, be in closer proximity in the structure, as a function of the higher density of their social ties relative to those with more dissimilar positions.

An important implication of these definitions is that models of social structure will differ to the extent that different social relationships are used as the linking mechanisms for the set of social positions, e.g., informal social contacts as compared to professional or business contacts. We wish, therefore, to devise a methodology that reveals how the pattern of given types of social relationships is structurally differentiated along specifiable dimensions or facets (cf. Guttman, 1959).

In order to interpret the underlying dimensionality of a structure, we must accept a crucial postulate or assumption:

Similarities in social positions, interests, attitudes, beliefs, and behavior facilitate the formation of consensual relationships among incumbents of social positions.

The corollary is that the more dissimilar two

positions are in status, attitudes, beliefs and behavior of their incumbents, the less likely the formation of consensual relationships and, consequently, the "farther away" they are from one another in the structure. This postulate asserts the *distance-generating mechanism* among social positions and incumbents. There is ample theoretical and empirical justification for accepting such a postulate as a reasonable starting point for analysis (cf. Homans, 1951; Newcomb, 1961; Fararo and Sunshine, 1964; Lauermann, 1966, 1973).

B. The Methodology of Structural Analysis: Graph Theory and Smallest Space Analysis

We shall focus on three social relationships among our influentials that provide critical vantage points for viewing a community's influence structure.⁵ First, from an instrumental point of view, we shall describe the pattern of *business-professional relationships*, since these are seen in both the functionalist and Marxist literature on community decision-making as important sources of common

⁵ Three questions are the source of information on these relationships:

- Q33. Would you please indicate the three persons from the list with whom you most frequently meet socially (informally)?
- Q37. Could you now indicate the three persons out of our list with whom you have the closest business or professional contact?
- Q38. Could you please indicate the three persons with whom you most frequently discuss community affairs?

interests and claims on the polity and should, therefore, help determine the lines of coalition and cleavage in the community. Respondents were asked to report the three other persons on the list of influentials with whom they were most often in contact in pursuing their primary institutional responsibilities. These are the task-linked, or instrumental, relationships that tie various organizations and collectivities together. Second, we shall describe the pattern of "social" or expressive relationships as it reflects the common interests arising from the influentials' instrumental activities in their primary institutional areas and the shared values, attitudes and concerns arising from their participation in other spheres of community life. These latter derive from such secondary characteristics of the influentials as their religious and educational backgrounds and residence status (*Alt* vs. *Neuburger*). Finally, we shall describe the pattern of "*community affairs*" relationships which are coalitional links among persons with regard to community affairs and may be hypothesized to result from the business-professional and social relations structures and the distinctive political arrangements of the community (cf. Rossi, 1960).

A major objective of recent sociometric efforts (e.g., Alba and Kadushin, 1970, undated; Bonacich, 1972a, 1972b; Coleman and McRae, Jr., 1960; Gleason, 1969; Hubbell, 1965; Levine, 1972; Rosen and Abrams, 1970; Rapoport and Horvath, 1961) has been to develop theoretically grounded, routine procedures to identify cliques, defined according to varying criteria of interrelatedness or "choice" patterns, in a large set of persons. A corollary objective has been to develop graphic techniques for describing how these cliques and persons who belong to no cliques are in turn interrelated.

The "sociogram" whereby individuals are represented by points and choice relations among individuals by (directed) lines was an early effort at graphic representation of the structure of interpersonal relationships (cf. Hunter, 1953; Moreno, 1953; Loomis and Beegle, 1951). But once the set of persons and number of choices (i.e., relationships) exceeded a rather small number, it was discovered that the resulting diagrams become far too complex to be readily interpreted. Indeed, two different investigators could

come up with quite different but "equally justifiable" graphic representations of the same matrix of choices that might suggest different interpretations of the same structure. The advent of the computer and the development of several mathematical and statistical techniques that require the computer's large computational resources for their successful application have spurred several strategies for analyzing large sociometric matrices (e.g., Bonacich, 1972a, 1972b; Gleason, 1969; Alba and Kadushin, 1970).

We have combined two recent developments, graph theory and smallest space analysis, to describe our three "relational" structures. Systematic introductions to these developments and discussion of their merits are found in Harary et al. (1965) and McFarland and Brown (1973).

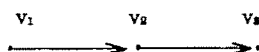
The mathematical theory of digraphs is concerned with postulates and theorems relating to "abstract configurations called digraphs, which consist of 'points' and 'directed lines'" (Harary et al., 1965:v). A graph consists of a set of points and connecting lines in which the direction of the lines is disregarded. Three graph theoretic ideas are of special interest to us: an adjacency matrix (from which all our subsequent analysis proceeds), reachability, and path distance. Consider the following sociometric (adjacency) matrix in which the rows and columns represent three persons, v_1 , v_2 , and v_3 , and the entries in the cells are either "1" or "0" to indicate whether or not v_i (in rows) chooses (is in a relation with) v_j (in columns).

MATRIX 1. AN ADJACENCY MATRIX

Chosen		v_1	v_2	v_3
Chooser	v_1	0	1	0
	v_2	0	0	1
	v_3	0	0	0

This matrix may be diagrammed, as in Figure 2, where points represent persons and directed lines (arcs) between two points represent a relationship. A point v_j is *reachable* from point v_i if there is a path from v_i to v_j , that is, if there is a set of directed lines from v_i to v_j . In our illustration, v_1 can reach v_2 in a

FIGURE 2. GRAPHIC REPRESENTATION OF MATRIX 1



path of length "1" and v_3 in a path of length "2", and v_2 can reach v_3 in a path of length "1", but v_2 and v_3 cannot reach v_1 . The reachable set $R(v)$ of a point v is the collection of points reachable from v . The *path distance* between two points in a digraph is the *minimum* number of directed lines that must be traversed in order to reach the second point from the first. (The path distance between two points in a graph is the *minimum* number of lines disregarding direction (i.e., the adjacency matrix is symmetric) that must be traversed in order to reach the second point from the first.) Gleason (1969) has devised a computer program that computes the reachability and path distance matrices from adjacency matrices containing up to eighty points. Since we are interested in the presence of a particular relation between two persons, whether or not it is reported by both persons as a mutual choice, we decided to disregard the direction of choices by the simple expedient of symmetrizing the adjacency matrices.

An inspection of the reachability matrix (consisting of "1" if v_j is reachable from v_i in some number of steps and "0" if v_j is not reachable from v_i) immediately tells us which persons were disconnected from which others in the total set of influentials—that is, their pattern of choosing and being chosen were such that they could not reach particular others in the structure. In our data, all respondents were reachable from all other respondents in the social and community affairs graphs in some finite number of steps, while five respondents in the business/professional structure were not reachable by some others. The maximum number of steps along a shortest path from one influential to any other was five both in the business/profession and in the social relations graphs and six in the community affairs graph. One individual, Herr K., who ranks as the most influential man in town, could reach in two or fewer steps 91 percent of the others in the community affairs structure and 73 percent of the others in the social relations structure and in the business/professional structure, respectively. Thus, from one point of view, we might conclude that our influence structure is highly integrated since nearly every leading influential can reach and be reached in each of the three networks by every other

influential in the community. Many disconnected individuals or sets of individuals would indicate a less integrated influence structure, with presumably greater difficulties in coordinating community affairs or resolving issues.

C. Graphic Representations of Influence Structures

By submitting each path distance matrix to a symmetric smallest space analysis (cf. Roskam and Lingoes, 1970; Guttman, 1968), we obtain an acceptable Euclidean two-dimensional representation of each matrix. The program takes account of the rank-order of the path distances (and not their absolute size); it attempts to preserve this rank-order while arranging the points in a space of few dimensions. In each representation the derived Euclidean distances among the points (persons) are a monotonic function of the original path distances among the points. We propose to interpret these pictures according to the theoretical principles suggested in the discussions of characteristics of individual influentials and differentiated social structures. Figures 3, 4, and 5 are the graphic representations of the smallest space solutions. Each person has been uniquely identified with a code providing information regarding his influence status, institutional sector responsibilities, party membership and religious preference. (See the legend for Figure 3 for the complete explanation of the abbreviated code.)

In general, we shall employ two basic principles for interpreting the spaces: the principle of integrative centrality and the principle of sector differentiation. The *principle of integrative centrality* holds that persons playing key integrative or coordinating roles in a given structure will tend to be located in the "central region" of their space—this will, on the average, minimize their distances from (access to) any other person in the space—while persons located increasingly in the periphery should be of declining importance in performing integrative activities for that structure and possibly of increasing importance in representing narrowly defined or interest-specific demands in that structure. This principle of interpreting the spatial solutions implies the identification of a coordinating central re-

FIGURE 5.

localized or regionalized a scatter of points (persons) sharing a common institutional locus, the more likely they will divide on issues of common institutional concern. The more localized a cluster of persons in a common institutional sector, the more homogeneous they will be in attitudes and values and the more they will function as a coordinated proactive or reactive claimant group (coalition) on community issues.

By combining these two principles, we can offer two additional speculations about the structure of the integrative center. First, we hypothesize that a position's location toward the center of the space but in a particular sector may be seen to reflect its potential integrative role as a representative for that sector's interest since, on the one hand, its position close to the center makes it relatively more influential, and, on the other, its location in an institutional sector ties it to other positions in that sector. Second, integrative centers may be seen to be highly biased in composition, over-representing certain sectors while under-representing or completely excluding others. To the extent that certain sectors are excluded from central zone locations (i.e., all their personnel are located in the periphery), we may infer that their impact on decision-

⁶ The centroid is the center of the smallest space solution. A physical analogy gives an intuitive sense of its meaning: if all points in a two-dimensional smallest space solution were a set of equal weights resting on a weightless plane, the centroid would be that point on which the plane would balance. For a technical discussion, see Roskam and Lingoes (1971).

merchants, to the periphery, which includes managers and owners of large manufacturing, financial, and agricultural enterprises located outside the city limits. The research personnel are located by themselves at some considerable distance from the center and from the other sectors, reflecting their highly segregated existence in the "social life" of the community. The traditional religious and educational elite are located opposite them in the space. Herr N., 12 SabCK, is the only Research Center person who has been fully assimilated in the sense of being located in the central zone of the social space; but he differs from his colleagues at the Research Center on nearly every key count—he is a Catholic rather than a Protestant, a social scientist rather than a natural scientist, and a convert from the SPD to the CDU since his arrival in Altneustadt.

Finally, the central core of the *community affairs space* includes a higher density of personnel than the other spaces whose members are recruited from much more heterogeneous institutional sectors and political and religious backgrounds. As one should expect, center personnel are more homogeneous on their reputed influence status in that they tend to be seen as belonging in the upper reaches of the influence hierarchy. The sector divisions, especially toward the periphery, are very similar in character to those of the other two spaces.

If we correlate reputed status as a community influential with distance from the centroid of each of three spaces, we find significant correlations for the business/professional structure (.40) and the community affairs structure (.30) but an insignificant correlation of .17 for the social relations structure. If we are prepared to regard reputed influence status as a crude indicator of relative "integrative" status in the community social system, then we can take these correlations as at least consistent with but by no means dramatic confirmation of our principle of integrative centrality. We might speculate further that integrative status may mean rather different things in these three relational contexts. Reputed status as a community influential is clearly more relevant to the business/profession and the community affairs structures but is

not especially relevant for the social integration of the community elite. If we had assessed reputed "social prominence and esteem" in the sense of Robert Dahl's (1961) "social notables" for all the influentials, we might well have found that this ranking was a more appropriate indicator of integrative status in the social relations structure and was significantly associated with centrality in that structure.

We can make two general statements summarizing our findings regarding the sector structural differentiation of Altneustadt. First, pattern-maintenance personnel are divided into two clearly identifiable regions or clusters at roughly opposite ends of an axis running through the center, with Research Center personnel located at a relatively greater distance from the center (reflecting their weaker influence on community decision-making) and traditional religious and educational leaders at the other end of the axis, some of whom enjoy closer proximity to or inclusion in the integrative cores of the three spaces. It is this axis of differentiation among pattern-maintenance personnel that reflects the principal axis of recurrent, intense cleavage on community issues. Secondly, there is somewhat less differentiation of the adaptive sector since economic personnel tend to cluster in the central region and an immediately adjacent peripheral zone rather than to fall into sharply separated clusters on opposite sides of the central region. The portrayal of the social relations space in Figure 4 reflects most clearly the somewhat weaker potential for an oppositional axis of adaptive personnel where Herr S., 13 PachSK, the only workers' representative in the SPD and the only union member found in the elite, is located diametrically opposite the largest factory owner, Herr F., 22.5a EvNP, and a number of other large businessmen. As expected for this predominantly middle-class community, economic interest differentiation is not so extensively developed and seems to involve primarily differentiation of small business, mercantile interests located in the city from the larger manufacturing and agricultural interests located outside the city limits, rather than labor-management differentiation.

THE OUTCOME OF INFLUENCE STRUCTURES

A. Community Issues

Readers concerned with analyzing community conflict over issues may well ask how our structural analysis deals with such matters. Our emphasis on describing the structure of community integration from a Parsonian standpoint seems to confirm the often repeated charge that the framework is simply too static and cannot handle conflict and change adequately (cf. Dahrendorf, 1961:77-82; Gouldner, 1970:353-5). Although we cannot answer all these objections satisfactorily, we would like to consider these questions.

It is useful to distinguish between two broad types of issues and their related outcomes. On the one hand, *instrumental issues* are concerned with controversies over the differing allocation of scarce resources, such as land, jobs, and money, and find their particular locus in the adaptive and integrative sectors of community concern. Lipset (1963) and others have spoken somewhat more narrowly of "class politics" when discussing such issues. For such issues there usually is a fairly obvious calculus of costs and benefits to various interested parties. Conflict over such issues tends to be moderate, often characterized by bargaining and compromise among the contending parties. The specific outcome is the direct result of their relative power or influence. Some political scientists have even thought it possible to devise means for the "rational" or "optimal" resolution of such controversies.

Consummatory or *expressive issues*, on the other hand, are concerned with controversies regarding the maintenance or change in the organization of basic values, commitments, and orientations that shall guide or control community affairs. Such controversies, sometimes termed "status politics" (cf. Lipset, 1963), are usually highly charged with emotional affect and have an "all or none" nature that usually precludes or makes very difficult negotiated settlements among the contending parties. Thus, the nature of the outcome and the level of community tensions often directly depends on how a given issue comes to be defined as one or the other type of issue.

One of the most unsatisfactory aspects of

the literature on community decision-making has been the basically atheoretical, ad hoc selection of community issues for analysis such that comparative study of community decision-making is difficult if not impossible. One can identify two favored strategies for identifying and selecting community issues. In the first strategy, the investigator identifies a set of recent issues in a community from newspaper accounts and community informants and selects those for intensive study that meet some criterion of "importance to the community," such as the level of public controversy and mobilization (cf. Dahl, 1961; Polsby, 1963; Freeman *et al.*, 1963). In the second strategy, the investigator selects an issue in which he already has some interest, perhaps because of his interest in a preferred outcome, such as fluoridation of the water supply (cf. Gamson, 1966; Rosenthal and Crain, 1966) or urban renewal (Hawley, 1963; Clark, 1968d), and which has come up for resolution in a number of communities. He wants to ascertain what factors determine a particular outcome. While both strategies enjoy the obvious advantage of relatively clear, unambiguous operational procedures, they both suffer from being heavily tied to all the historical particularities of the specific issues studied and pose serious problems, especially in the first strategy, for comparative analysis.

As Polsby (1963:96; also see Wolfinger, 1971:1078) pointed out some years ago, "there seem to be no satisfactory criteria which would identify a universe of all decisions (issues) in the community." The problem of defining the universe of content from which to sample issues is especially important when one wants to identify "non-issues" or check whether the actual issues are a biased sample (cf. Bachrach and Boratz, 1962, 1963). We think that it is impossible to define a universe of content without an adequate frame of reference for studying community power. At present only two frames of reference seem to be available: the interest group approach and the functional approach. The interest group approach looks for possible partisan groups in a community and identifies possible issues according to some notion of the objective interests of these groups. Since we used the

functional approach for analyzing the decision-making structure, it follows that we should use the same approach to define the universe of content of possible issues.⁷

A theoretical scheme for defining and classifying community issues is needed that permits: (1) a definition of the universe of content of possible community issues; (2) a means of defining the biases in the set of issues that actually arise in a community during a given period (that is, communities confront issues sequentially and, therefore, in any period of time may not face issues from the full range of the issue space); (3) a translation of the historical individuality of a given issue into a more theoretically meaningful category that permits comparative analysis; and (4) the generation of hypotheses linking the type of issue to structural characteristics of the community decision-making system. We hope that a step toward constructing such a scheme was taken by our decision to classify community issues according to their functional primacy in the AGIL paradigm of functions confronting any social system (cf. Parsons, 1951; Parsons et al., 1961; Clark, 1968c; Mayhew, 1971).

Obviously issues will often have implications for several functional sectors of the community social system. Much in the same way that we proposed to distinguish between primary and secondary functional foci for our influentials, issues may be seen to have primary and secondary impacts in different institutional sectors. Which of the possible functional definitions of an issue becomes focal or primary will depend on a series of considerations about its emergence in a particular community at a particular time with particular sponsors and opponents.⁸

⁷ We are inclined to agree with such commentators as Ossowski (1963), Lenski (1966), and Stinchcombe (1968) that Marxian and functionally oriented perspectives are by no means as radically incompatible as has sometimes been assumed. They have, of course, obvious differences of emphasis and concern. Marxian oriented analysts tend to see instrumental issues as the substrate of actual or potential community controversy; while functionalists tend to stress expressive issues, seeing group disagreements over fundamental values to arise from considerations in addition to their different relations to the economic structure.

⁸ Coding an issue into its appropriate functional

With our preceding analysis of the structure of community influence, we should be able to predict how given issues will be resolved by determining the functionally specialized sectors likely to be activated by a given functional issue. We can also assess the likelihood of a sector being divided on an issue by examining the relative spread or clustering of personnel in a particular institutional sector in the spatial solutions and their locations with respect to the central integrative core. If there is significant sectoral or integrative differentiation, we can predict the winning coalition as the one favorably located relative to the integrative core and including a higher average level of reputed community influence.

With these general considerations in mind, five issues were selected for intensive study according to two criteria. First, each issue must have had a major impact on community affairs within the past three or four years or might realistically be argued to have such an impact if it became a matter for decision in the near future. Second, the issues as a whole had to be distributed across the four functional problem areas identified in the AGIL paradigm. The issues meeting these two criteria were the following:

- (a) re-location of a large industrial firm to Altneustadt (economic or adaptive primacy);
- (b) construction of a city hall (political or collective goal-attainment primacy);
- (c) incorporation of outlying communities into an expanded city administrative unit (integrative primacy);
- (d) establishment of a secular primary school as opposed to the existing confessional school (latent pattern-maintenance).

sector is not a simple matter of identifying the institutional sector of the collectivities most likely to be affected. For example, a school bond issue is obviously concerned with the educational system, which is usually treated as functionally specialized with regard to pattern maintenance. But the issue may develop in two quite different directions. It may be regarded as a purely instrumental issue whether the community can afford to pay for another school, given its current obligations. Its functional locus is, therefore, integrative as it concerns establishing its claim of priority in the budget. But the issue may become expressive by focusing not on costs and alternatives foregone but on what type of school program is to be implemented in the new building. In this case, prospective changes in the organization of pattern-maintenance activities are at issue.

nance primacy: education and religion); and

- (e) permission to hold a Pop Festival in Altneustadt (latent pattern-maintenance primacy: public morality, status of youth as a "minority" group with low access to the center of power, intergenerational conflict).⁹

The underlying notion here of sampling issues from various institutional sectors was to provide an opportunity to determine if the elite tended to be correspondingly differentiated into coalitions functionally specialized for "control" in specific sectors or if there was a functionally and structurally undifferentiated unitary elite core (domi-

⁹ Altneustadt actually confronted issues b, c, and d in the past several years. But because it had not confronted issues having special relevance to the economic subsystem in the recent past and because we saw the city as having especially acute pattern-maintenance problems due to the rapid in-migration of distinctive newcomers, we decided to develop two *hypothetical* issues (a and e) for these two sectors. (See Perrucci and Pilisuk, 1970, for another recent study using hypothetical issues.) Both issues were quite realistic in that they could easily become matters of public or elite debate in the foreseeable future and, in fact, generated considerable disagreement among our respondents.

nant coalition) that made the crucial decisions for all institutional areas (perhaps with specialized "lower-level" personnel to implement these decisions). (See Table 1 for the average influence status of opponents and proponents on each issue.)

B. Graphic Representation of the Cleavage Structure

Figure 6 is identical to the spatial representation of the community affairs structure in Figure 5, but now we have drawn in "fault lines" for each of the five issues that divide the space into proponents and opponents on each issue. Before discussing this consensus-cleavage structure in greater detail we should discuss the operational independence of our various procedures. First, each respondent was asked at the beginning of the interview a series of questions about each issue probing such matters as the individuals and groups he perceived to be most strongly in favor or opposed to the issue, his own position on the issue and degree of actual or likely participation in the decision-process on the issue, as well as his estimate of the level of conflict over the issue and

COMMUNITY AFFAIRS NETWORK

SMALLEST SPACE ANALYSIS
EUCLIDEAN METRIC
TWO DIMENSIONAL SOLUTION
GUTTMAN LINGOES COEFFICIENT
OF ALIENATION = .131

F = for the proposal
A = against the proposal
X = stand on the proposal
could not be determined

EXAMPLE:

10.5a xxxax
POP FESTIVAL ISSUE
INCORPORATION ISSUE
CITY HALL ISSUE
SCHOOL ISSUE
INDUSTRIAL RESETTLEMENT ISSUE
INFLUENCE RANK

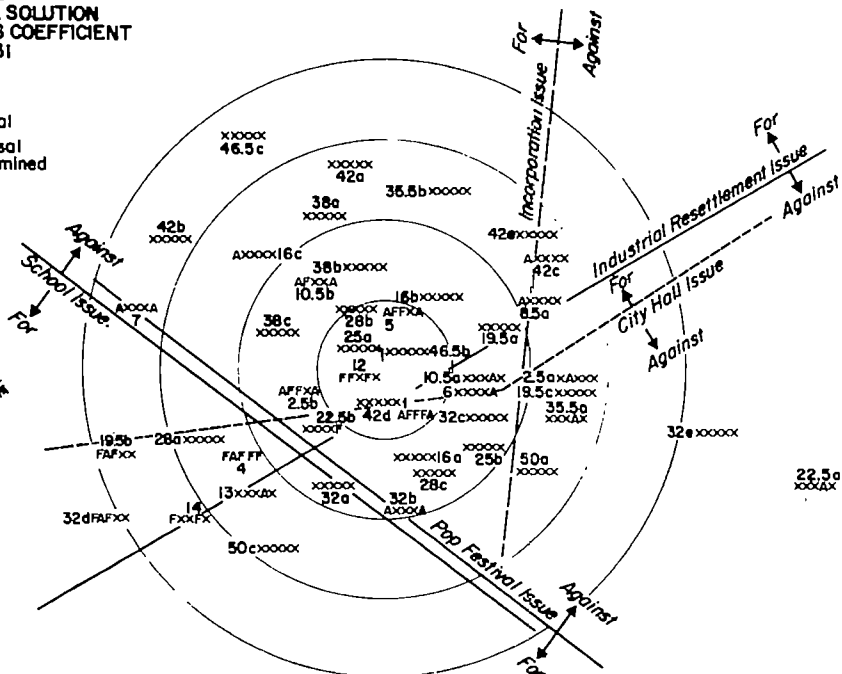


FIGURE 6.

whether the conflict would be public or confined to the "inner circle" of community influentials. The names of the active participants, pro and con, were spontaneously generated by the respondents—the list of influentials we had identified was not presented until much later in the interview. Second, in order for us to designate an influential as an active proponent or opponent on an issue, at least two respondents had to have spontaneously mentioned him in one or the other capacity. (In most cases attributed and self-reported position and involvement in the issue were the same, but in a number of important cases they were not. Persons on the losing side tended to report themselves on the winning side.)¹⁰

¹⁰ Since this stringent coding rule tended to identify only the most publicly prominent leaders on the opposing sides many of the elite had to be coded as "indeterminate" for any given issue on the basis of the perceptions of others. Did this mean that the fault lines, based on codings of only some elite members, were completely arbitrary? To check this possibility at least indirectly, the following procedure was adopted. All elite members were coded "for" or "against" an issue depending on which side of a fault line they were located. These new codings were then cross-tabulated against the elite members' self-reported positions on the corresponding issues.

Agreement was best for the incorporation issue where 79 percent were placed on the side of the issue in the figure corresponding to their self-reported stands. Agreement was not quite as high for the pop festival and the industrial resettlement issues (69 and 64 percent, respectively), partly because, we suspect, there were hypothetical issues on which elite members could only guess which side of an issue other elite members would take. It declined further for the city hall issue where only 56 percent were correctly placed, but note again that this issue was regarded as the least controversial of all issues posed, and few elite members reported special interest or involvement in it. Only the fit for the school issue was poor (24 percent agreement), but this lack of agreement is more apparent than real. More than ninety percent of the elite members claimed to be for the secular school (the side that won), many more than could actually have favored it given the controversial, divisive and extended character of the public debate. Many persons reporting themselves on the winning side were in fact perceived by other elite members as having been active on the losing side. In general, we detected a definite tendency for people to report themselves on the winning sides of all issues even though their knowledgeable peers saw them to have been on the losing side. Apparently here as elsewhere no one likes to be regarded a loser. We are, therefore, inclined to place much more

It is readily apparent from an inspection of Figure 6 that the fault lines of the oppositional structures and the personnel active on each of the issues do change from one functional issue to another and that some persons, most notably those in the central integrative zones, are likely to be active in more than one issue. In fact, only one person, the most influential man in town denoted by a "1," was perceived to be involved in all five issues.

The fault lines are almost identical on the two pattern-maintenance issues, the school and the Pop Festival, with the newcomers at the Research Center and their allies opposed to nearly everyone else. Although these issues differ considerably in their substantive content, they generated the most public controversy and mobilization of the issues considered. The integrative issue (community incorporation), on the other hand, united all city factions against the county political leadership; while the polity issue of building a new city hall was an inner-elite controversy (there was low public controversy about this issue), arraigning the "city hall crowd" located in the central zone against the periphery who, of course, lost. Finally, the industrial resettlement issue split the economic sector, with the large employers who might be fearful of such a large competitor for a limited labor supply and of their possible unfavorable dislocation in the influence structure opposed to the small retail tradesmen and business people who would probably welcome the expanded business opportunities arising from the population growth likely to be generated by the new employer. (An inspection of the fault lines drawn for the business/professional and social relations spaces, in two figures not reproduced here leads us to similar conclusions.)

Thus, for even this relatively small community, we see that structural differentiation is extensive enough to generate relatively stable coalitions that are activated differentially depending on the functional issue. Much more could be said about the internal structure of the various coalitions, their influence resources, value orientations, and preferred leadership strategies, which support

confidence on perceived positions than self-reported positions.

some of the interpretations we have been making. Hopefully the limited evidence presented has sufficiently indicated the ways our procedures greatly facilitate the systematic description of structural cleavage and consensus (e.g., by identifying who would be "impossible" coalition partners). In addition, this evidence seems to be reasonably consistent with the explicit structural-functional model we have been developing. Consensus-cleavage structures do, of course, change over time. We believe that these techniques could also be used to generate meaningful snapshots at different points in time which in turn could be juxtaposed to describe stability and change in community influence structures over time.

SUMMARY

Recent work in the study of community decision-making appears to be converging on a number of common theoretical and methodological strategies and assumptions. There still remain, however, important weaknesses in the overall theoretical framework and its implied methodology in directing research efforts. Attention is directed to a structural analysis of the community influence system that derives in part from Parsons. Several critical questions are raised concerning the identification of the relevant set of community influentials and the systematic description of their attributes as influentials and the ties that bind them into coalitions depending on the functional issue confronted. Recent advances in graph theory and smallest space analysis are used to examine the consensus-cleavage structure of the community influence system of Altnau, a small city in West Germany. Finally, a theoretical strategy and an empirical procedure are proposed for identifying community issues and tracing their impact on the formation of opposing factions and coalitions.

REFERENCES

- Aiken, Michael, and Paul Mott (eds.)
1970 *The Structure of Community Power*. New York: Random House.
- Alba, Richard D. and Charles Kadushin
1970 *The Construction of Sociograms by Computer Methods*. Mimeo. New York: Bureau of Applied Social Research, Columbia University.
- Undated "A note on the application of multidimensional scaling to the problem of sociometric clique identification." Unpublished manuscript. Columbia University.
- Bachrach, Peter and Morton S. Baratz
1962 "Two faces of power." *American Political Science Review* 56 (December):947-52.
1963 "Decisions and non-decisions: an analytical framework." *American Political Science Review* 57 (September):632-42.
- Blau, Peter M.
1964 *Exchange and Power in Social Life*. New York: Wiley.
- Bonacich, Phillip
1972a "Technique for analyzing overlapping memberships." Pp. 176-85 in Herbert L. Costner (ed.), *Sociological Methodology* 1972. San Francisco: Jossey-Bass.
1972b "Factoring and weighting approaches to status scores and clique identification." *Journal of Mathematical Sociology* 2 (January):113-20.
- Bonjean, Charles M., Terry N. Clark, and Robert L. Lineberry (eds.)
1971 *Community Politics: A Behavioral Approach*. New York: Free Press.
- Clark, Terry N.
1968a *Community Structure and Decision-Making: Comparative Analyses*. San Francisco: Chandler Publishing Company.
1968b "Who governs, where, when, and with what effects?" Pp. 15-23 in Terry N. Clark (ed.), *Community Structure and Decision-Making: Comparative Analyses*. San Francisco: Chandler Publishing Company.
1968c "Community structure and decision-making." Pp. 91-126 in Terry N. Clark (ed.), *Community Structure and Decision-Making: Comparative Analyses*. San Francisco: Chandler Publishing Company.
1968d "Community structure, decision-making, budget expenditures, and urban renewal in 51 American communities." *American Sociological Review* 33 (August):576-93.
- Coleman, James S. and Duncan McRae, Jr.
1960 "Electronic processing of sociometric data for groups up to 1,000 in size." *American Sociological Review* 25 (October):722-7.
- Dahl, Robert A.
1961 *Who Governs?* New Haven: Yale University Press.
- Dahrendorf, Ralf
1961 "Struktur und Funktion. Talcott Parsons und die Entwicklung der soziologischen Theorie." Pp. 49-84 in Ralf Dahrendorf, *Gesellschaft und Freiheit. Zur soziologischen Analyse der Gegenwart*. Munich: R. Piper Verlag.
- D'Antonio, William V., William H. Form, Charles P. Loomis and Eugene C. Erickson
1961 "Institutional and occupational representations in eleven community systems." *American Sociological Review* 26 (June):440-6.

- Davis, Kingsley
1948 *Human Society*. New York: Macmillan Company.
- Downes, Bryan T.
1968 "Suburban differentiation and municipal policy choices: A comparative analysis of suburban political systems." Pp. 243-67 in Terry N. Clark (ed.), *Community Structure and Decision-Making: Comparative Analyses*. San Francisco: Chandler Publishing Company.
- Fararo, T. J. and Morris H. Sunshine
1964 *A Study of a Biased Friendship Net*. Syracuse University, Youth Development Center.
- Freeman, Linton C.
1968 *Patterns of Local Community Leadership*. Indianapolis: Bobbs-Merrill
- Gamson, William A.
1966 "Rancorous conflict in community politics." *American Sociological Review* 31 (February):71-81.
1968 *Power and Discontent*. Homewood, Ill.: Dorsey Press.
- Gleason, Terry C.
1969 *D.I.P.: A Directed Graph Processor*. Mimeo. Ann Arbor, Michigan: Institute Social Research, University of Michigan.
- Gouldner, Alvin W.
1970 *The Coming Crisis of Western Sociology*. New York: Basic Books.
- Guttman, Louis
1959 "Introduction to facet design and analysis." Pp. 130-2 in *Proceedings of the 15th International Congress of Psychology*. Brussels.
1968 "A general nonmetric technique for finding the smallest coordinate space for a configuration of points." *Psychometrika* 33 (December):469-506.
- Harary, Frank, R. Z. Norman and Dorwin Cartwright
1965 *Structural Models: An Introduction to the Theory of Directed Graphs*. New York: Wiley.
- Hawley, Amos H.
1963 "Community power and urban renewal success." *American Journal of Sociology* 68 (January):422-31.
- Homans, George C.
1951 *The Human Group*. New York: Harcourt, Brace, and World.
- Hubbell, Charles H.
1965 "An input-output approach to clique identification." *Sociometry* 28 (December):377-99.
- Hunter, Floyd
1953 *Community Power Structure*. Durham, North Carolina: University of North Carolina Press.
- Laumann, Edward O.
1966 *Prestige and Association in an Urban Community*. Indianapolis: Bobbs-Merrill.
1973 *Bonds of Pluralism. The Form and Substance of Urban Social Networks*. New York: Wiley Interscience.
- Lenski, Gerhard
1966 *Power and Privilege: A Theory of Social Stratification*. New York: McGraw-Hill.
- Levine, Joel
1972 "The sphere of influence." *American Sociological Review* 37 (February):14-27.
- Lipset, Seymour M.
1963 "The sources of the 'radical right,' " "Three decades of the radical right: Coughlinites, McCarthyites, and Birchers." Pp. 259-377 in Daniel Bell (ed.), *The Radical Right*. Garden City, New York: Doubleday.
- Loomis, Charles P. and J. Allen Beegle
1950 *Rural Social Systems*. New York: Prentice-Hall, Inc.
- MacFarland, David and Daniel Brown
1973 "Social distance as a metric: a systematic introduction to smallest space analysis." Pp. 213-53 in Edward O. Laumann, *Bonds of Pluralism. The Form and Substance of Urban Social Networks*. New York: Wiley Interscience.
- Mayhew, Leon
1971 *Society: Institutions and Activity*. Glenview, Illinois: Scott, Foresman and Company.
- Moreno, Jacob L.
1953 *Who shall Survive? Foundations of Sociometry, Group Psychotherapy and Sociodrama*. New York: Beacon.
- Nadel, S. F.
1957 *The Theory of Social Structure*. London: Cohen and West
- Newcomb, Theodore
1961 *The Acquaintance Process*. New York: Holt, Rhinehart, and Winston.
- Ossowski, Stanislaw
1963 *Class Structure in the Social Consciousness*. London: Routledge and Kegan Paul.
- Parsons, Talcott
1951 *The Social System*. Glencoe, Illinois: Free Press.
1960 *Structure and Process in Modern Societies*. New York: Free Press.
- Parsons, Talcott, Edward Shils, Kaspar D. Naegle and Jesse R. Pitts (eds.)
1961 *Theories of Society: Foundations of Modern Sociological Theory*. New York: Free Press, especially pp. 30-79.
- Perrucci, Robert and Marc Pillsuk
1970 "Leaders and ruling elites: the interorganizational bases of community power." *American Sociological Review* 35 (December):1040-57.
- Polsby, Nelson W.
1963 *Community Power and Political Theory*. New Haven: Yale University Press.
- Rapoport, Anatol and William Horvath
1961 "A study of a large sociogram." *Behavioral Science* 6 (October):279-91.
- Rosen, Richard and Peter Abrams
1970 "CHAIN: A sociometric linkage program." Mimeo. New York: Bureau of Applied Social Research, Columbia University.
- Rosenthal, Donald B. and Robert L. Crain
1966 "Structure and values in local political

- systems: the case of fluoridation decisions." *Journal of Politics* 28 (February):169-96.
- Roskam, E. and James C. Lingoes
 1970 "MINISSA-I: A FORTRAN IV (G) program for the smallest space analysis of square symmetric matrices." *Behavioral Science* 15 (March):204-20.
- 1971 "A mathematical and empirical study of two multidimensional scaling algorithms." *Michigan Mathematical Psychology Program* 1:1-169.
- Rossi, Peter H.
 1960 "Power and community structure." *Midwest Journal of Political Science* 4 (November):390-401.
- Stinchcombe, Arthur L.
 1968 *Constructing Social Theories*. New York: Harcourt, Brace and World.
- Walton, John
 1966a "Substance and artifact: the current status on research of community power structure." *American Journal of Sociology* 71 (January):430-8.
- 1966b "Discipline, method, and community power: a note on the sociology of knowledge." *American Sociological Review* 31 (October):684-9.
- Wolfinger, Raymond E.
 1971 "Nondecisions and the study of local politics." *American Political Science Review* 65 (December):1063-80.

THE ECOLOGICAL APPROACH IN MEASURING COMMUNITY POWER CONCENTRATION: AN ANALYSIS OF HAWLEY'S MPO RATIO *

JAMES M. WILLIAMS

University of Wisconsin, Eau Claire

American Sociological Review 1973, Vol. 38 (April):230-242

Comparative research into the causes and consequences of variation in community power structure has been hampered by the absence of a reliable objective indicator of power concentration. In an earlier work, Amos Hawley suggested that the "MPO ratio" (the proportion of the labor force classified by the U.S. census as Managers, Officials and Proprietors), could be used as an index of power concentration in a community. The lower the MPO ratio, the more concentrated the power. This hypothesis is tested by computing MPO ratios for three groups of communities whose power structures had been previously studied by the reputational or decisional methods. The results show that MPO ratios are larger in communities with concentrated power structures, directly contrary to Hawley's prediction. This may be due to the MPO's relationship to community stratification and type of political organization. The small amount of variation in power concentration "explained" by variation in MPO ratios leads to the conclusion that it is not a valid indicator of relative power distribution.

THE phenomenon of power in local communities has been the central concern of a great many social scientists, particularly during the last twenty-five years.¹ While the distribution, sources and consequences of power arrangements have been studied by persons in a number of different disciplines, only recently have researchers begun to assemble their concepts into a unified body of theory which can be used to

test hypotheses in a comparative perspective.² Unfortunately, only a few comparative studies have included more than a handful of communities.³

Although the earlier studies of power were concerned mainly with identifying different types of power distributions, recent investigators have been more concerned with the effects of different power distributions (Aiken and Alford, 1970); Clark,

* I am indebted to Charles M. Bonjean and the anonymous referees of the *American Sociological Review* for useful comments on an earlier draft of this paper. I am also grateful to the researchers who supplied me with the "real" names of the communities they studied. Without these the present study could not have been completed.

¹ See the bibliography by Pelligrin (1967).

² Recent attempts at conceptual clarification and theory building have been made by Clark (1967, 1968a), Bonjean and Olson (1964) and Mott (1970).

³ The major exceptions are Clark's (1968b) study of fifty-one communities and Aiken and Alford's (1970) effort involving all communities in the U.S. over 25,000 population.

1968b). Efforts in this direction would be facilitated by an easily computed indicator of the salient aspects of power distribution in a community. Ideally, this would be a quantitative measure, computed from one or more demographic or ecological characteristics that are periodically collected and published for communities in all size classes.

A measure that purports to fit these criteria has existed for nine years. In 1963, Amos Hawley proposed that the degree of power concentration in a community could be adequately determined by computing a simple ratio of the numbers of managers, proprietors, and officials in a community to its local labor force. The smaller this "MPO" ratio, the more concentrated the power structure. Hawley went on to show that the MPO ratio was lowest in cities that had reached the execution stage of the urban renewal implementation process, and was highest in cities that had never attempted urban renewal. Cities that started the process but "dropped out" had a mean of MPO scores that fell between the two extremes. Hence, the MPO ratio was inversely related to urban renewal "success," suggesting that a concentrated power structure made it easier to get things done in community affairs. He argued that problems of coordination and conflicting interests characteristic of dispersed power arrangements will make it more difficult to move projects toward completion.

Although the MPO has been used by a few investigators (Paulson, Butler and Pope, 1969; Aiken and Alford, 1970), the validity of the measure has never been clearly established. Shortly after its introduction, the measure was criticized by Straits (1965), and more recently its validity has been called into question by Aiken (1970:502-3). The purpose of the present study is to raise and provide tentative answers to two questions relative to the MPO ratio. First, is Hawley's theoretical basis for proposing the MPO as a measure really sound? Is the logical structure of his argument consistent? Secondly, does the MPO ratio do a good job of estimating the degree of concentration of power in communities studied by survey techniques? If the answers to both these questions are yes, then comparative power researchers will have a

powerful tool to use in determining the effects of power arrangements on a wide variety of outcomes, including urban renewal, educational expenditures, open housing referenda, etc.

THE MPO RATIO

Unlike most theoretical work on the subject (Dahl, 1968; Goldhamer and Shils, 1939; Simon, 1953), Hawley views power as an attribute of social systems, not individuals. All social systems of the same kind are characterized by more or less equivalent amounts of power (1963:423-4), but they may vary in the way this power is distributed among the various parts of the system.⁴ These parts (subsystems) possess two "kinds" of power, functional and derivative. Functional power is that related directly to the tasks allocated to the subsystem in the overall division of labor, while derivative power is "that which spills over into external relationships and regulates the interaction between parts"⁵ (1963:423). Functional power takes care of routine chores; but when a crisis emerges, putting a strain on the system, the magnitude and distribution of derivative power become crucial. Hawley's central argument is that crisis situations can be met more effectively when this power is concentrated rather than dispersed.⁶

⁴ This appears to be a questionable assumption. To argue that all communities (as concrete social systems) possess equivalent amounts of power would seem to fly in the face of most theory and research regarding relations between metropolis and hinterland (e.g., Bogue, 1949).

⁵ The relationship between these two aspects of power is never made explicit, beyond indicating that subsystems with the key function of "relating the system to its environment" will have the greatest amount of derivative power (1963:423).

⁶ The criticisms leveled at the decisional method of studying power by Bachrach and Baratz (1962) are equally applicable here. Community elites can suppress issues that might embarrass them politically, or threaten to weaken their power position. A complete understanding of power relations must include the "nondecision," or matters which the leadership does not even want raised, much less acted upon. Given this, prediction of community outputs simply from knowledge of power concentration becomes very difficult, if not impossible. One would have to know something also about the "mobilization of bias" in the elite as well. On the

How can the distribution of power in a community system be determined? Hawley's answer is the MPO ratio, as developed in the following passage:

Proceeding from the notion that system power resides in the subsystems or functional units of a community, we can infer that it must be exercised through the managerial functions of the subsystems. For it is those functions that co-ordinate the several other functions in their respective subsystems and articulate the latter with the larger system. In the absence of data on the number of managerial functions, I shall use the number of managerial personnel, that is, the number of people who reported occupations as manager, proprietor, or official in the Population Census, to measure concentration of power. Personnel, it should be stressed, is used only as a substitute for, and as an index of, functions. Since the significance of the number of functions varies with the number of all other functions (i.e., the size of the employed labor force), it should be expressed as a ratio to the latter. Hence the lower the ratio of managers, proprietors, and officials to the employed labor force the greater is the concentration of power. This measure will hereafter be called the MPO ratio (Hawley, 1963:424).

Note that Hawley uses the total number of Managers, Proprietors and Officials in the entire community in constructing his measure. This is clearly a serious logical flaw in his argument, since the total number of these persons (functions) tells us nothing about the way that they are distributed among the various subsystems. If anything, Hawley appears to be measuring the total amount of power, not its distribution. Given this, if his assertion about systems generating equivalent amounts of power is correct, one might expect to find that MPO ratios do not differ significantly

other hand, studies of actual urban renewal projects generally support Hawley's thesis about the effect of diffusion of power. According to Kaplan (1963:4), most of the delays in progressing through the various stages can be attributed to the difficult task of accommodating diverse and competing interests. He shows that Newark had less trouble because the number of actors concerned with the outcome was quite small, and the political system was fairly integrated. In contrast, Davies (1966) shows how citizens' groups that oppose renewal have an easy time in New York City, because the extremely fragmented political system provides numerous points of entry for them.

from one community to the next.⁷ Actually, Hawley should have constructed a measure that reflects the relative concentration of power functions in a community system.⁸

The research performance of the MPO ratio has been spotty and inconsistent. Hawley (1963) found MPO ratios to be smaller in cities that had made more progress in urban renewal, consistent with his hypothesis. A similar conclusion was reported by Aiken and Alford (1970). On the other hand, Straits' (1965) reanalysis of Hawley's data, using multiple regression techniques, showed that size and age of city housing are better predictors of urban renewal success than the MPO, and concluded that most of the association that Hawley found was due to the MPO's relationship with other variables. Moreover, Paulson, Butler and Pope (1969) found that when using outputs in the welfare sector as a dependent variable, MPO ratios were larger in cities with the highest output, directly contrary to the Hawley thesis.

In the face of these criticisms, additional attention to the MPO ratio may seem unwarranted. It is possible, however, that the MPO ratio may tell us something important about community power in spite of its logical inconsistency and ambiguous performance in research. It is to this end that we compare the MPO ratio with independent reports made by investigators using direct techniques of measuring power.

EMPIRICAL ANALYSIS OF HAWLEY'S MPO RATIO

With a single exception,⁹ no prior attempt has been made to validate the MPO ratio

⁷ This is, of course, not true, as the present study as well as Hawley's demonstrates.

⁸ The author has constructed such a measure, basing it on Gibbs' Index of Concentration (1961:238). It is currently being evaluated in a manner similar to the one to be described presently.

⁹ Shortly before the completion of the present project, Aiken (1970) published an independent evaluation of the MPO which partially parallels the present one. Aiken chose to use most of the cities in the Walton group (see below) but added others from published and unpublished sources. Although differing slightly in data manipulation and statistical techniques, both studies are in agreement as to the relationship between the MPO and concentration of power. Contrary to Hawley's

as a measure of community power concentration. According to Hawley's argument, as the MPO ratio increases, the power structure becomes more "pluralistic," or less concentrated. Whether this is in fact the case is an empirical question to be partly answered by the present research.

The overall research strategy was to compare MPO ratios with the results of field studies on individual communities, using them as independent indicators of the distribution of power. To increase the scope and depth of the analysis, three different sets of data were used to estimate the validity of the MPO as an index of the distribution of power in a given community: (1) forty-four of the fifty-five communities included by Walton in his study of the relationship between discipline, method, and type of power structure (Walton, 1966a, 1966b, 1967), (2) seventeen communities studied by Bonjean's two-step approach to the study of power (1963, 1971) and (3) fifty-one cities in the NORC Permanent Community Sample (PCS) reported by Clark (1968b).

TEST 1: THE WALTON GROUP OF COMMUNITIES

Many of the communities in Walton's list are identified by pseudonyms, and an attempt was made to contact each of the authors for the real name of the city. This was successful in all but a few cases, and these authors' cooperation is gratefully acknowledged. After eliminating those who did not reply or who could not be located, the three foreign cities and one with insufficient data, a total of forty-four communities remained from the original group of fifty-five.¹⁰

Walton classified each community in his study according to type of power structure, using a slightly modified version of the scheme originated by Rossi (1960:398). The categories are (Walton, 1967:355):

- Pyramidal: A monolithic, monopolistic, single cohesive leadership group.
- Factional: At least two durable factions that compete for advantage.
- Coalitional: Leadership varies with issues and is made up of fluid coalitions of interested persons and groups.
- Amorphous: Absence of any persistent pattern of leadership or power exercised or the local level.

The classification of each city used in this test, by type of power structure, may be found in Walton (1966a:431-3).¹¹ For purposes of this analysis, the categories were treated as an ordinal scale measuring concentration of power. In the analysis which follows, the coalitional and amorphous categories are combined.

MPO scores¹² were computed for com-

¹¹ Unfortunately, the precise criteria for including a study in one or another of the categories is not made explicit in any of Walton's publications. The mechanics of the coding procedure are likewise unknown. As a check on the reliability of the coding, approximately one-third of the studies were independently classified by the author, and complete agreement noted in 75% of the cases. In addition, forty of the forty-four cities used were independently reclassified by Aiken (1970:517-19), and complete agreement reported for thirty-four (80%). In almost all cases, the coding difference was in the factional, coalitional and amorphous categories since some authors are not very explicit about the nature of factions or the degree of structure observed. Since recoding might introduce a bias, the Walton categories were used as given, on the grounds that they would provide a more severe test of the Hawley model, having been originally employed for another purpose.

¹² The census taken nearest the study date is used as the data source throughout the study. The MPO ratio is computed as follows:

$$\frac{\text{Total number of male managers, proprietors and officials}}{\text{Total number of employed males}} \times 100$$

There appears to be some mystery as to how Hawley computed the MPO ratios used in his study. In a footnote (1963:424), he suggests that due to the heterogeneity of the category, only managers, proprietors and officials "not elsewhere classified" be used. This severely limits the utility of the index, since the census did not report occupational data with this degree of detail for cities less than 250,000 in 1960 or 50,000 in 1950. Moreover, although it would have the merit of excluding some non policy-making categories, it would also exclude people in public administration, which would be unwise. Finally, Straits (1965:78) implies that Hawley uses all managers, proprietor and officials in computing the index for some of his tables. Hawley denies this in his Reply (1965

hypothesis, Aiken found that MPO ratios are higher in cities with concentrated power distributions (1970:503).

¹⁰ Communities eliminated included "Bakerville," "English City," Juarez, Mexico, Tiajuana, Mexico, "Service City," "Midway Co.," "River Co.," "Beach Co.," "Southern Co.," and "Farmdale."

munities in each category. The range of the MPO ratio for the fourteen pyramidal cities was 9.90–31.86; for the fifteen factional cities, 8.24–18.61; for the fifteen coalitional/amorphous cities, 5.53–14.94. The category means were 15.10 for the pyramidal, 13.76 for the factional, and 10.90 for the coalitional/amorphous. One way analysis of variance was used to test the null hypothesis that the means of the MPO scores do not differ between the three categories. The alternate hypothesis, derived from Hawley's model, is that MPO scores will be lowest for pyramidal cities and highest for coalitional/amorphous, with factional cities falling in the middle. Inspection of the category means reveals that this is not the case. Indeed, the relationship is the exact reverse of what Hawley would expect. Analysis of variance produced an F ratio of 3.22 which is just large enough (allowing for rounding and measurement error) to permit rejection of the null hypothesis¹³ at the .05 level of significance. Hawley's argument is clearly not supported by these data. Eta², used to

measure the strength of the relationship,¹⁴ revealed that only about 9% of the variation in the MPO is explained by the type of power structure. This is approximately the same as that reported by Aiken (1970:503), using different statistical procedures.¹⁵

Since several scholars (Wolfinger, 1962; Polsby, 1962; Freeman *et al.*, 1963; Walton, 1966b) have argued that the method employed to study power (e.g., reputational, decisional, positional or some combination of these) influences the type of structure uncovered,¹⁶ an attempt was made to control for method.¹⁷ The subset of twenty cities studied by the reputational method was treated separately to see if MPO scores of pyramidal power arrangements differed from those of factional, coalitional and amorphous, combined. The mean of the pyramidal category was 13.50; while the factional, coalitional/amorphous group had a mean of 12.26. Analysis of variance produced an F ratio of 1.16; not significant at the .05 level of confidence.

Even so, one can tentatively conclude that the MPO ratio does not measure community power concentration in the manner that Hawley postulated, at least for this particular group of forty-four cities.

CITIES STUDIED BY THE BONJEAN TWO-STEP REPUTATIONAL APPROACH

Bonjean and Olson (1964) and Bonjean (1971) have helped to clarify some of the

83), saying that all proprietors, managers and officials were used, except for some "technical specialists" who are left unidentified. Faced with this confusing array of statements, the method described above was selected since it can be used in communities of any size greater than 2,500. The use of males only is defended on the basis that in all community power studies, males are reported to play the dominant roles. MPO scores for all communities used in the analysis will be provided upon request.

¹³ Analysis of variance assumes independent random samples, from normally distributed populations having equal variances (Roscoe, 1969:236). Walton's cities are not a random sample of all U.S. cities, nor is the sample one of studies. In actuality, it is intended to be the universe of cities that meet his rather restrictive criteria (Walton, 1967:355). In fact, the only group of cities in the present study that meet the assumptions of random sampling is the Permanent Community Sample, discussed later in the article. Given this, the reader may wonder why tests of significance are made. Following Blalock (1960:270) one may reason that the cities are a random sample from a "hypothetically infinite 'universe of possibilities.'" Another justification is provided by Stinchcombe (1968:23–4) who defends the use of statistical inference even when the entire population has been measured, on the grounds that observed differences may be due to measurement error. No attempt is made to impute causality, and tests of significance are used through the paper to aid the reader in interpreting the findings.

¹⁴ Eta² is a measure of the degree to which categories are homogeneous compared to the total variation in the interval scale variable (Blalock, 1960:267). The formula is $\text{Eta}^2 = 1 - \frac{V_w}{V_t}$ where

V_w is the within categories variance estimate, and V_t is total variation in the interval scale variable that can be explained by the category variable.

¹⁵ Aiken used correlation analysis in determining the relationship between the MPO and degree of power concentration, treating the Walton categories as a 4 point scale. Aiken recognizes that this violates the interval scale assumption of the model, but justifies its use on the grounds that it "... (simplifies) the data presentation and ... (permits) control for the effect of third variables on relationships without a loss of cases" (1970:490).

¹⁶ But for evidence to the contrary, see Clark, *et al.*, 1968.

¹⁷ This is somewhat imprecise, as attempts to classify studies by method sometimes involve guesswork.

underlying variables of community power by isolating four dimensions.

These are:

1. *Legitimacy*—the extent to which leaders in the community occupy elected or appointed positions in local government or key voluntary associations.

2. *Visibility*—the extent to which leaders are recognized as such by the community at large.

3. *Scope of influence*—the degree to which sets of leaders specialize in issue areas, rather than all passing on every issue that comes before a community.

4. *Consensus*—the amount of agreement among leaders as to policy and general value orientations.¹⁸

These four dimensions can vary somewhat independently, and may be used to describe different types of power arrangements. For example, an "idealization" of a "covert elite," similar to that identified by Hunter (1953) would be low in visibility, low in legitimacy, but high on scope of influence (indicating that leaders exercise power over a great number of issues) and high on consensus. By contrast, a pluralistic system would be characterized by high visibility, high legitimacy, low scope of influence and low consensus (Bonjean, 1971:23-4). Intermediate types, based on Dahl's (1961: 184-9) "Patterns of Leadership" can also be described by these four variables (Bonjean, 1971:38-9).

The four dimensions can be related to concentration of power and the magnitude of the MPO ratio. If the dimensions adequately describe and contrast the salient distinctions between a covert elite and legitimate pluralism, and assuming that power is concentrated more highly in the former than the latter, Hawley's model would predict the following:

1. The higher the visibility, the higher the MPO ratio.
2. The higher the legitimacy, the higher the MPO ratio.

3. The higher the scope of influence, the lower the MPO ratio.

4. The higher the consensus, the lower the MPO ratio.

Seventeen U.S. communities have been studied using the two-step reputational method. Unfortunately, only scores for legitimacy and visibility are currently available.

The Bonjean technique yields three types of leaders: visible, concealed, and symbolic. A community's visibility score is simply the proportion of visible leaders among those designated as key leaders.

Legitimacy is measured by examining the formal political and para-political offices held by the key leaders. The former consist of the usual official positions in city and county government: mayor, city councilman, county judge, chief of police, etc. The latter are civic and voluntary association positions such as president of the local "good government" league, or the local chamber of commerce. The community's legitimacy score is the proportion of leaders holding at least one of these positions (Bonjean, 1971:23-4).

Seventeen communities in the U.S. have been studied using the two-step reputational method. These are shown in Table 1 along with their scores on legitimacy, visibility and MPO ratio.

Communities were divided into two groups at the median of the visibility scores (41.5) and the means of the MPO scores compared. Hawley's model would predict that MPO ratios are higher for cities with high visibility and legitimacy scores, and this is indeed the case. Communities with visibility scores above 41.5 have a mean MPO of 12.94, while those with visibility scores below the figure have a mean MPO of 10.98. While this is in the predicted direction, analysis of variance produces an F ratio of 3.5554, not significant at the .05 level.

The same procedure was used for legitimacy scores (median = .55). Again the relationship is in the predicted direction, as the high legitimacy group has a mean MPO of 12.79, and the low legitimacy group a mean of 10.73. This time the relationship is significant at the .05 level, as the obtained F ratio is 4.95. Eta² indicates about 21%

¹⁸ This dimension was termed "cohesiveness" in the Bonjean and Olson (1964) article, but the term "consensus" is used in the later version (Bonjean, 1971). The latter also contains suggestions concerning measurement of the variables.

Table 1. Legitimacy, Visibility, MPO Ratios and Size for Seventeen Communities Studied by the Bonjean Two-step Reputational Method

Community	Legitimacy	Visibility	MPO	Size (1960)
Tupelo, Miss. (Preston, 1969)	1.00	.88	17.54	17,221
Charlotte, N. C. (Bonjean and Carter, 1965)	.77	.82	14.85	201,564
Winston Salem, N. C. (Bonjean and Carter, 1965)	.75	.80	10.74	111,135
Andrews, Texas (Spiekerman, 1968)	.70	.80	11.39	11,135
Natchez, Miss. (Preston, 1969)	.69	.81	14.17	23,791
Victoria, Texas (Bonjean, 1971)	.63	.39	10.94	33,047
Burlington, N. C. (Bonjean, 1963)	.65	.29	11.28	33,199
High Point, N. C. (Bonjean and Carter, 1965)	.62	.38	11.41	62,063
Carlsbad, N. M. (Bonjean, 1971)	.48	.27	9.45	25,541
Edinburg, Texas (Bonjean, 1971)	.48	.40	10.94	18,706
Crystal City, Texas (Spiekerman, 1968)	.46	.85	8.41	9,101
Alice, Texas (Bonjean, 1971)	.38	.43	12.35	20,861
San Marcos, Texas (Bonjean, 1971)	.47	.34	11.35	12,713
Austin, Texas (Bonjean, 1971)	.36	.29	13.22	186,545
Belvidere, Ill. (French and Aiken, 1968)	.32	.12	11.31	11,223
Bloomington, Ind. (Miller and Dirksen, 1965)	.16	.32	8.78	31,357
Barbourville, Ky. (Sutton, 1970)	na	.57	14.06	3,211

of the total variation in the MPO is explained by its association with the visibility category.

These results, contrary to those obtained for the Walton data, provide weak empirical support for the Hawley thesis. A cautious interpretation is warranted, however, since the sample size is quite small, the cities were studied by seven different investigators and there were slight differences in the method of distinguishing key leaders.

THE NORC SAMPLE

The NORC Permanent Community Sample (PCS) is a probability sample of two hundred American cities of 50,000 or more (Rossi and Crain, 1968:262). The goal is to collect detailed data on decision-making, community action, demographic and ecological variables, voting and attitudes for a representative sample of American cities.

The first published report based on these

data is Clark's (1968b) study of community structure and community outputs. Fifty-one communities, sampled on the basis of region and population size, were investigated and their decision-making structures determined through the "ersatz decisional method" (1971:296-9).

For each community, a social scientist from a nearby college or university was selected to act in a liaison capacity. Each was politically knowledgeable about the community and had a special interest in community research. A panel of eleven informants was selected from each community, consisting of occupants of the following positions: the mayor, the chairmen of the Democratic and Republican parties, the president of the largest bank, the editor of the newspaper with the largest circulation, the president of the chamber of commerce, the president of the bar association, the head of the largest labor union, the health commissioner, the urban renewal director, and the director of the last major hospital fund drive (Clark, 1968b:579).

Each informant was interviewed by a NORC field worker regarding a specific set of community issues: urban renewal, election of the mayor, air pollution, and the poverty program. These particular issues were chosen in the belief that they tended to involve different segments of the community across different potential lines of cleavage, and in various interrelationships. The local influence structure was determined by seeking answers to the following questions (Clark, 1968b:580):

1. Who initiated action on the given issue?
2. Who supported action on the issue?
3. Who opposed action on the issue?
4. What was the nature of the bargaining process? Who negotiated with whom?
5. What was the outcome? Whose views prevailed?

An index of decentralization was constructed for each community by simply counting up the number of actors mentioned by the informants and dividing by the number of issue areas. For example, if a total of twenty actors was mentioned in response to the above questions, over four issue areas, the score for that community would be five (Clark, 1968b:580). One nomination from

any informant was sufficient to warrant inclusion.

This procedure has several problems associated with it. For one thing, the use of community statuses as referents instead of individuals led to the possibility of a person being counted twice if he were involved in different issues as an occupant of different statuses. Moreover, the index measures only participation, not outcome. Simple participation in decision making means little if the same interests always prevail.

These criticisms notwithstanding, the measure should provide a good test of the Hawley thesis. It has the additional merit of being quantitative, thus permitting the application of correlation analysis in addition to analysis of variance. Decentralization scores for the fifty cities can be found in Clark (1971:297). The range of these scores is 3.25 to 9.38, with a mean of 6.79. MPO scores for this set of cities ranged from 5.33 to 17.30, with a mean of 10.75.

The cities were divided at the median of the decentralization score (6.75) and the means of the MPO's compared for each group. The mean of MPO scores for cities with "low" decentralization scores was 11.88, while the mean for the "high" group was 9.18. This is directly contrary to the prediction that would be made from the Hawley model. Analysis of variance reveals that the null hypothesis can be safely rejected at the .01 level of confidence. The overall correlation coefficient between the decentralization score and the MPO ratio for this group of cities is $-.46$, significant at the .001 level.

Since this group of cities is larger than the ones previously tested, they constitute a sample of a finite universe; and since they were examined by a common methodology under direction of a well known research agency, one can have more confidence in the results. They strongly confirm the tendency for MPO ratios to be related inversely to power concentration in communities.

SUMMARY AND INTERPRETATION

The results of the foregoing tests present a cloudy picture. Although the magnitude of the relationships between the MPO and

various measures of power concentration increased with greater precision in method, the directions are bewildering. The MPO is inversely related to power concentration in the Walton group. Controlling for reputational method reduced the relationship, but this may be an artifact of grouping procedure. Among cities studied by the Bonjean two-step approach, MPO ratios and the measures of power concentration are directly related, but only legitimacy was statistically significant. Finally, the Clark decentralization measure for the fifty-one NORC cities varied inversely with the MPO; and the relationship is highly significant.

The obvious problem is that we are attempting to verify a measure of power concentration (MPO) by using other measures of power concentration as a standard when they themselves have not been examined for reliability and validity. It may well be that all the measures are faulty, and the associations with the MPO spurious. Possibly one or two of the measures are better indicators than a third. On the other hand, all three may be equally valid, but be tap-

ping different aspects of the phenomenon of power.

The overlap in coverage between the several groups of cities may aid in solving the puzzle. Table 2 shows cities that appear in more than one of the sample groups. It appears that the greatest amount of correspondence exists between the Clark and Walton groups. The two cities that Walton coded pyramidal (Amarillo and Waco) have very low decentralization scores, at the bottom of the distribution. Seattle and Atlanta, rated as coalitional by Walton, have decentralization scores which rank 18.5 and 30, respectively, in the fifty-one city sample. Contrariwise, the Bonjean and Walton groups, with the exception of Winston Salem and possibly Burlington, do not match well at all. Scores of Bloomington appear to be too low for a coalitional city. Although not included in the Walton sample, the investigators in Belvidere describe a power structure with two major factions (French and Aiken, 1968). Yet, it had the lowest visibility score in the entire group, and its legitimacy score was exceeded by all but Bloomington. Unfortunately, only Char-

Table 2. A Comparison of Power Concentration Measures for Cities Appearing in at Least Two of the Tests Reported

City	Walton Code*	Legitimacy	Visibility	Decentralization
Charlotte		.77	.82	6.25
Winston Salem	C/A	.75	.80	
Burlington	P	.65	.29	
Bloomington	C/A	.16	.32	
Amarillo	P			3.33
Atlanta	C/A**			6.50
Seattle	C/A, P***			7.50
Waco	P			3.25
Belvidere	F****	.32	.12	

* P, Pyramidal; F, Factional; C/A, Coalitional/Amorphous.

** This is the code from the Jennings (1964) study.

*** Gore and Peabody (1958) and Miller (1958) obtained conflicting results for this city.

**** Although not in the Walton study, it has been added to aid in interpretation. French and Aiken (1968) are quite specific about the presence of two durable factions.

lotte overlaps between the Bonjean and Clárk groups, but even here it is worth noting that its legitimacy score and visibility score are ranked two and three among the seventeen cities, while its decentralization score is near the lower middle of the Clark distribution, at 35.5.

The most persuasive explanation seems to be that the methods are tapping different dimensions of power whose relationships to each other are highly complex. This notion can be examined in terms of Bonjean and Olson's (1964) classification of power structure dimensions. The Clark decentralization measure, despite its faults, seems to come very close to their concept of scope of influence. Legitimacy and visibility can vary independently with scope of influence, and the latter is often very wide even in communities where leaders are both visible and legitimate. Mayor Daley's Chicago is a case in point. By the same token, the fact that leaders are concealed does not mean that they *must* have a wide scope of influence. Additional studies using the two-step technique will have to be completed before the relationships between the dimensions can be specified.

The findings in Table 2 are a strong argument against an oversimplification or a uni-dimensional conception of power. It is not surprising that Clark's measure and Walton's types are associated, if one of the major criteria used by the latter in classifying his studies was the notion of scope of influence. One may suspect that this is the case, since the notion of pyramidal communities almost necessarily implies wide scope of influence. By the same token, this dimension is closely bound up with the factional and particularly the coalitional categories.

CONCLUSION

This paper has been concerned with two objectives: (1) to examine the logical basis of Hawley's MPO as a measure of power concentration, and (2) to test the measure against field studies of local communities.

With regard to the first objective, it appears that there are serious flaws in Hawley's argument. First, there is no basis for

assuming that social systems of the same kind have equivalent amounts of power at their disposal. Second, the distinction between functional and derivative power is difficult to maintain in concrete situations. Every major decision of a powerful corporation will have consequences for other subsystems in a community. Would a decision to begin manufacturing a new product simply be an exercise of functional power? Families would be affected, as employment opportunities expanded. Schools would feel the effects of additional enrollments as new families moved into the community. City government would get some additional revenue, but would be required to provide additional services, etc. Finally, Hawley's use of the MPO to measure concentration of power in one or more subsystems is unjustified. Knowing the proportion of MPO's in the total community would not indicate anything about the distribution of MPO's in various sectors.

With regard to the second objective, the bulk of the evidence indicates that the MPO is inversely related to concentration of power. In retrospect, this is not surprising, given the nature of the measure and what is known about consequences of differential community composition. High MPO ratios are likely to be found in communities that are "middle class," with a high proportion of white collar workers and professionals. This is true for the fifty-one city sample, as a correlation of .65 was obtained between the MPO and "per cent white collar" as reported in the County and City Data Book (1962).¹⁹

Aiken (1970:501) found that cities with more white collar workers had more highly concentrated power structures. One of the reasons for this can be traced to the type of governmental structure. As Banfield and Wilson have argued (1963:41) the middle class is "public regarding," favoring a type of government that benefits the "community as a whole." This conception of government formed the basis for the reform movement in community politics, and led to adoption

¹⁹ Per cent white collar includes professionals, managers, officials, proprietors, sales and clerical workers.

of the council-manager form of government in many U.S. cities, accompanied by at-large representation and non-partisan elections.

The study by Aiken (1970:499-500) shows that these "reformed" communities have more concentrated power arrangements than communities with non-reformed governments. This is true as well for the fifty-one communities in the PCS, as Clark found a strong ($r = -.548$) inverse relationship between "reformism" and decentralization of the power structure (1968b: 585-6).

These findings lead one to suspect that the MPO ratio is nothing more than a weak measure of the middle class composition of a community. The higher the MPO, the greater the proportion of white collar workers and professionals, and the greater the chance that the community will have a "reformed" political structure, which is probably going to mean that the power structure is more concentrated than in a non-reformed community. This interpretation is supported by the direct relationship ($r = .56$) obtained between the MPO ratio and "reformism" for the fifty-one cities in the PCS.²⁰ Thus, the MPO appears to be an additional measure of tendencies toward reformism in city politics.

Although the above explanation is highly tentative, it is obvious that the MPO ratio explains only a small fraction of the total variation in community power concentration. All of the correlations reported are very low, leaving most of the variation unexplained. Following Jencks' "First Law of Human Relationships" (1968), may I now propose the following theorem: No *single* characteristic of a community will ever explain more than about 25% of the variation in the power structure of that community. It would follow that anyone interested in measuring power through the use of census data would do well to scrap the MPO, and devote effort to a different procedure involving several variables in a multidimensional framework. In line with this, a method of estimating power concen-

tration using Hadden and Borgatta's (1965) twelve profile variables in a multiple regression equation has been developed and will be reported in a future article.

REFERENCES

- Aiken, Michael
1970 "The distribution of community power: structural bases and social consequences." Pp. 487-525 in Michael Aiken and Paul E. Mott (eds.), *The Structure of Community Power: Readings*. New York: Random House.
- Aiken, Michael and Robert R. Alford
1970 "Community structure and innovation: the case of urban renewal." *American Sociological Review* 35 (August):650-65.
- Bachrach, Peter and M. Baratz
1962 "Two faces of power." *American Political Science Review* 51 (December):947-52.
- Banfield, Edward C. and James Q. Wilson
1963 *City Politics*. Cambridge: Harvard University Press and MIT Press.
- Blalock, Hubert M.
1960 *Social Statistics*. New York: McGraw-Hill.
- Bogue, Donald J.
1949 *The Structure of the Metropolitan Community*. Ann Arbor: Horace H. Rackham School of Graduate Studies, University of Michigan.
- Bonjean, Charles M.
1963 "Community leadership: a case study and conceptual refinement." *American Journal of Sociology* 68 (May):672-81.
1971 "Dimensions of power structure: some problems in conceptualization and measurement." Pp. 19-42 in Frederick M. Wirt (ed.), *Future Directions in Community Power Research: A Colloquium*. Berkeley: Institute for Governmental Studies.
- Bonjean, Charles M. and Lewis F. Carter
1965 "Legitimacy and Visibility: Leadership Structure Related to Four Community Systems." *Pacific Sociological Review* 8 (Spring):16-20.
- Bonjean, Charles M. and David M. Olson
1964 "Community leadership: directions of research." *Administrative Science Quarterly* 9 (December):278-300.
- Clark, Terry N.
1967 "The concept of power." *Social Science Quarterly* 48 (December):271-86.
1968a *Community Structure and Decision-Making: Comparative Analyses*. San Francisco: Chandler.
1968b "Community structure, decision-making, budget expenditures, and urban renewal in 51 American communities." *American Sociological Review* 33 (August):576-93. An expanded version of this article appears in Charles M. Bonjean, Terry N. Clark and Robert L. Lineberry (eds.)

²⁰ Although the index of reformism is the same that Clark (1968b:585) used, the analysis was done during the course of the present study.

- Community Politics: A Behavioral Approach. New York: The Free Press, 1971: 293-313.
- Clark, Terry, William Kornblum, Harold Bloom, and Susan Tobias
1968 "Discipline, method, community structure and decision-making: the role and limitations of the sociology of knowledge." *The American Sociologist* 3 (August):214-17.
- Dahl, Robert A.
1961 *Who Governs: Power and Democracy in an American City*. New Haven, Conn.: Yale University Press.
1968 "Power." Pp. 405-13, in David Sills (ed.), *International Encyclopedia of the Social Sciences*, v. 12. New York: The Macmillan Co. and The Free Press.
- Davies, J. Clarence
1966 *Neighborhood Groups and Urban Renewal*. New York: Columbia University Press.
- Freeman, Linton C., Thomas J. Fararo, Warner Bloomberg, Jr. and Morris H. Sunshine
1963 "Locating leaders in local communities: a comparison of some alternative approaches." *American Sociological Review* 28 (October):791-98.
- French, Robert Mills and Michael Aiken
1968 "Community power in Cornucopia: a replication in a small town of the Bonjean technique of identifying community leaders." *Sociological Quarterly* 9 (Spring): 261-70.
- Goldhamer, Herbert and Edward Shils
1939 "Types of power and status." *American Journal of Sociology* 45 (September): 171-82.
- Gore, William J. and Robert L. Peabody
1958 "The functions of the political campaign." *Western Political Quarterly* 11 (March): 55-70.
- Hadden, Jeffrey K. and Edgar F. Borgatta
1965 *American Cities: Their Social Characteristics*. Chicago: Rand McNally.
- Hawley, Amos
1963 "Community power and urban renewal success." *American Journal of Sociology* 68 (January):422-31.
1965 "Reply to Straits." *American Journal of Sociology* 71 (July):82-4.
- Hunter, Floyd
1953 *Community Power Structure: A Study of Decision Makers*. Chapel Hill: University of North Carolina.
- Jencks, Christopher
1968 "Who gets ahead." *New Republic* 158 (April 20):31-5.
- Jennings, M. Kent
1964 *Community Influentials: The Elites of Atlanta*. New York: The Free Press.
- Kaplan, Harold
1963 *Urban Renewal Politics: Slum Clearance in Newark*. New York: Columbia University Press.
- Miller, Delbert C.
1958 "Decision-making cliques in community power structures: a comparative study of an American and an English city." *American Journal of Sociology* 64 (November): 299-310.
- Miller, Delbert C. and James L. Dirksen
1965 "The identification of visible, concealed and symbolic leaders in a small Indiana city: a replication of the Bonjean Noland study of Burlington, North Carolina." *Social Forces* 43 (May):548-55.
- Mott, Paul E.
1970 "Configurations of power." Pp. 85-100 in Michael Aiken and Paul E. Mott (eds.), *The Structure of Community Power*. New York: Random House.
- Paulson, Wayne, Edgar W. Butler and Hallowell Pope
1969 "Community power and public welfare." *The American Journal of Economics and Sociology* 28 (January):17-27.
- Pellegrin, Roland J.
1967 "Selected bibliography on community power structure." *Southwestern Social Science Quarterly* 48 (December):451-65.
- Polsby, Nelson W.
1962 "Community power: some reflections on the recent literature." *American Sociological Review* 27 (December):838-41.
- Preston, James D.
1969 "The search for community leaders: a re-examination of the reputational technique." *Sociological Inquiry* 39 (Winter): 37-47.
- Roscoe, John T.
1969 *Fundamental Research Statistics for the Behavioral Sciences*. New York: Holt, Rinehart and Winston, Inc.
- Rossi, Peter
1960 "Power and community structure." *Midwest Journal of Political Science* 4 (November):390-401.
- Rossi, Peter H. and Robert Crain
1968 "The NORC permanent community sample." *Public Opinion Quarterly* 32 (Summer):261-72.
- Simon, Herbert
1953 "Notes on the observation and measurement of political power." *Journal of Politics* 15 (November):500-16.
- Spiekerman, Ruth Danette Hill
1968 *Identification of Community Power Structure Using the Reputational Approach: A Comparative Analysis of Two Texas Communities*. Unpublished M.S. Thesis, Texas A. and M. University.
- Stinchcombe, Arthur L.
1968 *Constructing Social Theories*. New York: Harcourt, Brace and World.
- Straits, Bruce C.
1965 "Community adoption and implementation of urban renewal." *American Journal of Sociology* 71 (July):77-82.
- Sutton, Willis
1970 "Visible, symbolic and concealed leaders in a Kentucky county: a replication, and

- comparisons with other communities." Unpublished Manuscript.
- U.S. Bureau of the Census
1962 County and City Data Book, 1962. Washington, D.C.: U.S. Government Printing Office.
- Walton, John
1966a "Substance and artifact: the current status of research on community power structure." *American Journal of Sociology* 71 (January):430-8.
- 1966b "Discipline, method and community power: a note on the sociology of knowledge." *American Sociological Review* 31 (October):684-99.
- 1967 "The vertical axis of community organization and the structure of power." *Southwestern Social Science Quarterly* 48 (December):353-68.
- Wolfinger, Raymond E.
1962 "Reputation and reality in the study of community power." *American Sociological Review* 25 (October):636-44.

SOME METHODOLOGICAL ISSUES IN COHORT ANALYSIS OF ARCHIVAL DATA *

KAREN OPPENHEIM MASON

Research Triangle Institute

WILLIAM M. MASON

Duke University

H. H. WINSBOROUGH

University of Wisconsin-Madison

W. KENNETH POOLE

Research Triangle Institute

American Sociological Review 1973, Vol. 38 (April):242-258

Cohort analyses in which the joint effects of aging, historical change and birth cohort membership are estimated for some dependent variable are often desirable on substantive grounds. Unless two of these three variables are viewed as indexing identical unmeasured causal factors, any analysis which makes estimates for only two of the three variables is subject to spurious results. But three-way cohort analysis is problematic because age, time period and birth cohort are linearly dependent on each other. Although this confounding makes estimation of some three-way cohort models impossible, this paper demonstrates that estimation is feasible in a number of such models. By exploring estimates derived for some of these models from hypothetical data for which the underlying effects are known, this paper also shows that meaningful three-way cohort analysis is difficult unless the researcher entertains relatively strong hypotheses about the nature of aging, period and cohort effects.

I. INTRODUCTION

THIS paper discusses the problem in cohort analysis that results from the logical relationship among age, historical period and birth cohort. These variables as usually measured are logically confounded with each other, and their joint use to predict a dependent variable is therefore problematic. In this paper we specify a general model for cohort analysis and indicate the

assumptions necessary to estimate its age, period and cohort parameters. This model is not the only one which can be specified for particular substantive analyses (see, e.g., Carlsson and Karlsson, 1970 for an alternative model), but it appears to embody assumptions that are common in many cohort problems (see, e.g., Baltes and Reiner, 1969; Cattell, 1970; Crittenden, 1962; Cutler, 1969-70; Evan, 1959 and 1968; Glenn and Grimes, 1968; Glenn and Hefner, 1972; Glenn and Zody, 1970; Greenberg, et al., 1950; Oppenheim, 1970; Schaie and Strother, 1968a and 1968b; Winsborough, 1972; Winsborough and Dickinson, 1972).

It has been suggested that cohort analysis can be performed meaningfully only if one of the three independent variables is ignored. We first argue in this paper that this "solution" to the problem of age-period-cohort

* Revision of a paper presented at the Annual Meetings of the American Sociological Association, New Orleans, August 28-31, 1972. This research was supported in part by Contract NIH-71-2212 with the National Institutes of Health, Department of Health, Education and Welfare. The authors thank Otis Dudley Duncan, Philip V. Piserchia, Norman B. Ryder and one of the referees for their comments on an earlier draft of this paper, and Francis Notzen and Barbara S. Schulz for their assistance with computer processing.

confounding is unsatisfactory in many sociological cohort problems. Because a distinct causal interpretation can often be applied to age, period and cohort, the failure to control for one of the three variables leaves results open to the possibility of spurious effects. For these cohort problems, a three-way analysis is desirable.

We next show that although the confounding of age, period and cohort makes certain three-way analyses impossible, it does not make all such analyses impossible. Using a multiple classification framework because it is general in functional form, we show that a small number of restrictions placed on the cohort model will suffice to estimate the independent effects of age, period and cohort.

The next section of the paper explores the behavior of such multiple classification models on three sets of hypothetical data. We show that there are pitfalls associated with three-way cohort analysis if prior knowledge about the behavior under study is limited, but also suggest ways in which these pitfalls can be avoided.

In the final section of the paper we note the relevance of our discussion for approaches to cohort analysis which differ from ours, and conclude by emphasizing the merits of rigorous, quantitative approaches to cohort analysis.

II. THE CONFOUNDING OF AGE, PERIOD AND COHORT

Cohort analysis normally starts by arraying some measure on the dependent variable according to age and period, cohort and period, or age and cohort. The third variable in these tables is then represented by the table diagonals.¹ In the case where age groups form the rows of the table and periods the columns, birth cohorts are represented by the diagonals that run from the upper left to the lower right. Thus, a three-way cohort analysis usually consists of considering how the main effects and the interactions between them representing cohorts determine the behavior under study.

¹This assumes that the size of each age group is equal to each interperiod interval, or that the years covered by each birth cohort are the same as the years covered by each age group, etc.

Although there are a number of methods designed to estimate interaction effects, Baltes (1968) has argued that detection of the interactions representing the third variable in a cohort analysis is impossible. Age, period and cohort are, he asserts, inherently confounded with each other, and an analysis in which behavior is explained by all three variables is thus not possible.

It is evident that such confounding does exist and can make certain parameters in some cohort models inestimable. For example, if age, period and cohort are treated as continuous variables, then it will be impossible to estimate all parameters in a model of the form:

$$Y = \alpha + \beta_1 A + \beta_2 P + \beta_3 C + \epsilon, \quad (1)$$

where Y is the dependent variable, α the intercept, the β_i the partial slopes associated with age, A , with period, P , and with cohort, C , and the ϵ a random error term. Because $C = P - A$ the above equation is redundant, and it is therefore impossible to attribute unique effects to all three independent variables.

The lack of independence among age, period and cohort can also be understood in the experimental framework (Blalock, 1967). In this framework, the effects of variables are estimated by holding constant the effects of all other variables and then manipulating the treatment of interest. Age, period and birth cohort are clearly variables that can be manipulated only in the statistical sense. Even in this sense, however, it is logically impossible to hold constant the effects of, say, both age and period and then vary birth cohort. Once we select a group of a particular age in a particular year, there is only *one* birth cohort whose behavior we can observe.

This confounding of age, period and cohort has led to the suggestion (Baltes, 1968; Baltes and Nesselroade, 1970) that cohort analyses be performed by ignoring one of the three independent variables. Although there may be situations in which this approach is justified, it will not prove satisfactory in many. In particular, if age, cohort and period have *distinct causal interpretations*, then an analysis which omits one of these variables is subject to spurious findings.

In Table 1 we present hypothetical data to illustrate a configuration which could lead to spurious findings if a variable were omitted. Because all cohorts and age groups in this table have identical rates in a given period, these data would usually be interpreted as showing only period effects (see, e.g., Evan, 1959; 1968). If a two-way analysis involving only age and cohort were performed on these data, however, the conclusions reached might be quite different. For example, Figure 1, which graphs these data by age and cohort, does not make clear that every cohort has an identical rate at a given point in history. The researcher analyzing his data through such a graph might therefore be led to conclusions we would generally consider to be false.

The appropriateness of a two- rather than a three-way cohort analysis rests on the question of whether we view age, period and birth cohort as causally distinct in relation to a given dependent variable. We cannot say categorically that these three variables are always causally distinct, but we suspect this possibility is strong for studies of archival social science data. Suppose, for example, that we are attempting to explain

men's earnings in the United States over time. Aging *per se* should, as we know from a number of past studies, affect men's earnings regardless of birth cohort or the periods in which we measure them. Because age indexes first the accumulation of occupational experience, but later the depreciation of skills that gradually become outdated, earnings tend to rise and then decline as men progress through the life cycle (Winsborough, 1972).

Secondly, for this example we can also see how period, regardless of men's ages or birth cohorts, would affect their earnings, insofar as the wage-price structure changes with time. Finally, there are also reasons why membership in particular birth cohorts might affect men's earnings, regardless of their age or the period in which we measure them. Ryder (1965:845) and Keyfitz (1972), for example, have suggested that men born in relatively small cohorts should have advantages in the educational system, the labor market and within bureaucratic structures relative to men born in larger cohorts. The small cohort men may, for example, experience less crowding in schools, greater job opportunities at the time they enter the labor market, and opportunities for more rapid advancement through bureaucracies where there will be less competition for higher level positions than there will be for men belonging to large cohorts. Regardless of men's ages or the historical period, then, certain cohorts may earn more than others because of the consequences of relative cohort size (Winsborough, 1972).

Even in problems where the size of birth cohorts is irrelevant, there may still be reasons to distinguish the effects of cohort membership from the effects of particular periods. Party identification in the United States, for example, is believed to be largely determined by early adulthood, although influenced as well by later short-term electoral influences (Oppenheim, 1970). This suggests that the political environment in which a particular birth cohort first enters the electorate may help determine the extent to which individuals in that cohort identify with a political party for the remainder of their lives. As that party experiences normal fluctuations in political fortunes, however, some members of the cohort may temporarily shift

Table 1: Data Constructed to Show Pure, Nonlinear Period Effects.

Ages	Time Period						
	1	2	3	4	5	6	7
1	50	47	43	48	51	53	50
2	50	47	43	48	51	53	50
3	50	47	43	48	51	53	50
4	50	47	43	48	51	53	50
5	50	47	43	48	51	53	50
6	50	47	43	48	51	53	50
7	50	47	43	48	51	53	50
8	50	47	43	48	51	53	50
9	50	47	43	48	51	53	50
10	50	47	43	48	51	53	50
11	50	47	43	48	51	53	50
12	50	47	43	48	51	53	50

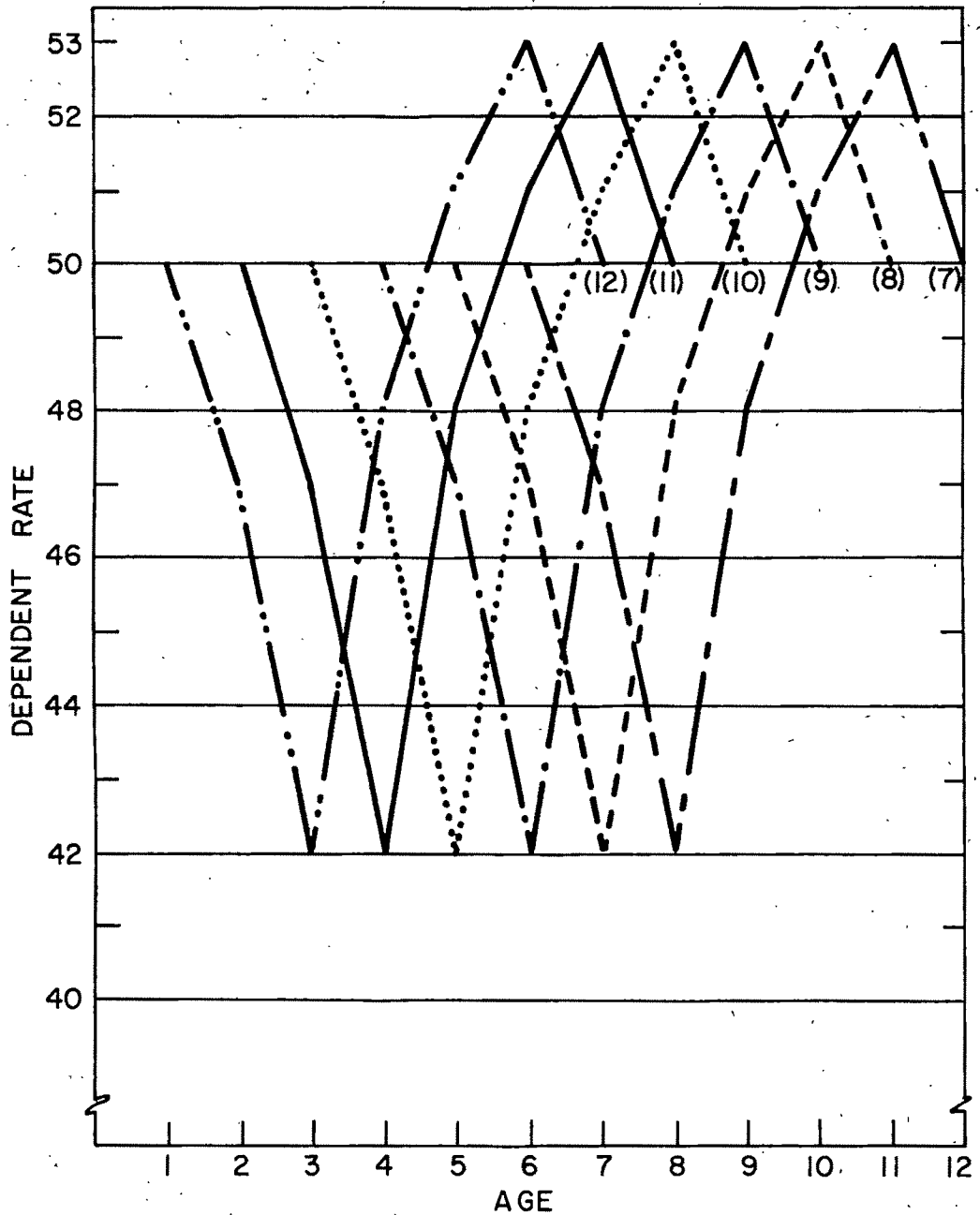


Figure 1: Dependent Rate by Age for Six Birth Cohorts (from Table 1).

their loyalties. Both cohort and short-term period effects can thus contribute to party identification. Since the aging process might also independently affect party identification (as persons become more "conservative" with age, for example, they may find the Republicans increasingly attractive), we have here

another example in which age, period and cohort conceptually have distinct causal impacts on the dependent variable.

There are, then, a number of substantive issues whose resolution requires that we consider the causal impact of all three variables involved in a cohort analysis.

Thus, we are led to examine the circumstances under which it is possible to perform these three-way analyses, given that age, period and cohort are confounded.

III. ESTIMABLE COHORT MODELS

Inspection of equation (1) suggests that its estimation is problematic because the relationships between age, period and cohort have the same functional form as the expected relationship of each of these independent variables to the dependent variable, Y . That is, we cannot estimate unique effects for age, period and cohort because we postulate each variable to be linearly related to Y and at the same time assume that A , P and C are linearly related to each other. This suggests one kind of model in which estimation of age, period and cohort effects is possible; namely, one in which the relationship of at least one of these variables to Y is constrained to be non-linear. For example, replacing A by A^2 would make equation (1) estimable. Thus, there clearly are some three-way cohort models in which we can estimate the unique contribution of aging, period change and birth cohort membership to the dependent variable.

That equation (1) can be modified to become estimable is not likely to be a helpful insight in practice, since analysts of social science archive data usually lack the rationale for specifying this model in the first place. Indeed, we suspect that in most cohort problems the available substantive knowledge is consistent with a large number of possible functional relationships between age, cohort and period and the dependent variable. This suggests consideration of the question of estimability in three-way cohort models which are relatively functional-free.

Multiple classification analysis provides a framework in which we can define a general model for three-way cohort analysis which is relatively functional-free.² Here,

we specify the dependent variable to be the result of effect parameters associated with particular levels of each independent variable:

$$Y_{ij} = \mu + \beta_i + \gamma_j \delta_k + \epsilon_{ij}, \quad (2)$$

$$(i = 1, \dots, r; j = 1, \dots, s; k = 1, \dots, r+s-1),$$

where the effect of the i -th age group is given by β_i , the effect of the j -th period by γ_j , the effect of the k -th cohort by δ_k ; where μ is the grand mean of the dependent variable and where ϵ_{ij} is the random disturbance. In the rest of this section we discuss what relationships may be estimated, using equation (2) as our starting point. In the Appendix we present formally, for a specific example, the argument given here.

As is well known (Graybill, 1961:227), it is impossible to obtain best linear unbiased estimates for the coefficients of models such as (2), which postulate unique effects for each category within each dimension and where each dimension is represented exhaustively by its categories. In these models it is possible to estimate only certain combinations of effects. However, these combinations often provide us with the information in which we are interested. For example, if we know the effects of membership in every age group relative to some other age group, we may surmise the functional form of the relationship between the dependent variable and age by plotting differences in coefficients as a single graph. Thus, for equation (2) we wish to know whether $(\beta_{i'} - \beta_i)$, $(\gamma_{j'} - \gamma_j)$ and $(\delta_{k'} - \delta_k)$ are estimable (for $i \neq i'$, $j \neq j'$, $k \neq k'$). That is, within any dimension, is the difference between any two effects estimable?

The answer to this question is no. This conclusion can be illustrated by attempting to manipulate the terms in Figure 2 to solve for differences among the β_i , γ_j , and δ_k parameters respectively. In Figure 2 each observed cell value in a cohort table is represented in terms of the unknowns from equation (2) for which we wish to solve. As subtraction of adjacent cells in the same row, the same column or the same diagonal will

² Other approaches should yield comparable results. For example, if one wished to treat age, period and cohort as continuous variables, one could estimate higher order polynomials involving all terms except for one of the three linear terms,

and then ascertain which slopes were significant. The significant slopes should then approximate functions similar in form to those estimated by a series of effect parameters for dummy variables.

show, we can at most solve for pairs of difference terms of the form:

$$Y_{1j'} - Y_{1j} = (\beta_{1'} - \beta_1) + (\gamma_{j'} - \gamma_j).$$

No amount of algebraic manipulation will lead to a solution for linear combinations of the form $(\beta_{1'} - \beta_1)$. Thus, under the assumption that all age groups, time periods and birth cohorts have unique effects on the dependent variable, it is impossible to estimate a difference between the effects of any two categories within a single dimension. In the Appendix we reach this conclusion with a formal proof for the five-age group, three-period case.

Having concluded that a model which postulates unique effects for each age group, period and cohort does not yield estimable differences between the coefficients of each classification, we next ask whether there is a respecification of the model which will yield estimates of these differences. One possible change in equation (2) that could be substantively attractive would be to assume that several age groups, cohorts or time periods have identical effects on the dependent variable. In a recent cohort analysis of party identification, for example, Oppenheim (1970) assumed that only three large blocks of birth cohorts would differ from

each other in party identification; those first eligible to vote before the "New Deal" period, those first eligible to vote in the New Deal era between 1928 and 1948, and those first eligible to vote after 1948. Greenberg, et al. (1950) and Klecka (1971) provide additional examples of research in which this kind of grouping assumption has been plausibly argued.

Under the assumption that any two ages, periods or cohorts have identical effect parameters, differences of the form $(\beta_{1'} - \beta_1)$ in equation (2) are now estimable. The Appendix demonstrates formally, for the five-age, three-period case, that all differences among the age effects, period effects, and cohort effects are now estimable. This conclusion can also be illustrated for the general case by reference to Figure 2, where we now "remove" the β_2 term from the second row of this figure (under the assumption that the first two age groups have identical effect parameters, both of which we arbitrarily set equal to zero). Subtraction of terms in adjacent cells falling in the same row, column or diagonal will now allow unique estimates of the differences between the coefficients which interest us. It is also possible to demonstrate that further algebraic manipulation will lead to solutions

AGES	PERIODS					
	1	2	3	...	s-1	s
1	$\mu + \gamma_1 + \delta_r$	$\mu + \gamma_2 + \delta_{r+1}$	$\mu + \gamma_3 + \delta_{r+2}$...	$\mu + \gamma_{s-1} + \delta_{r+s-2}$	μ
2	$\mu + \beta_2 + \gamma_1 + \delta_{r-1}$	$\mu + \beta_2 + \gamma_2 + \delta_r$	$\mu + \beta_2 + \gamma_3 + \delta_{r+1}$...	$\mu + \beta_2 + \gamma_{s-1} + \delta_{r+s-3}$	$\mu + \beta_2 + \delta_{r+s-2}$
3	$\mu + \beta_3 + \gamma_1 + \delta_{r-2}$	$\mu + \beta_3 + \gamma_2 + \delta_{r-1}$	$\mu + \beta_3 + \gamma_3 + \delta_r$...	$\mu + \beta_3 + \gamma_{s-1} + \delta_{r+s-4}$	$\mu + \beta_3 + \delta_{r+s-3}$
⋮	⋮	⋮	⋮	⋮	⋮	⋮
r-1	$\mu + \beta_{r-1} + \gamma_1 + \delta_2$	$\mu + \beta_{r-1} + \gamma_2 + \delta_3$	$\mu + \beta_{r-1} + \gamma_3 + \delta_4$...	$\mu + \beta_{r-1} + \gamma_{s-1} + \delta_s$	$\mu + \beta_{r-1} + \delta_{s+1}$
r	$\mu + \beta_r + \gamma_1 + \delta_1$	$\mu + \beta_r + \gamma_2 + \delta_2$	$\mu + \beta_r + \gamma_3 + \delta_3$...	$\mu + \beta_r + \gamma_{s-1} + \delta_{s-1}$	$\mu + \beta_r + \delta_s$

Figure 2: Multiple Classification Cohort Model (2) Represented in Tabular Form.

for all possible differences between coefficients: once these few have been found.

The assumption that two categories of a dimension have the same effect on the dependent variable is a sufficient constraint in the sense that additional assumptions about relationships between effects are unnecessary. To be sure, this does not rule out the possibility of alternative conditions of estimability in multiple classification cohort models. We shall, however, explore no further such conditions of estimability. The equality constraint is minimal, inasmuch as it involves only two categories. Moreover, applied judiciously, as between adjacent categories of a dimension, this constraint appears to be quite realistic in settings which might benefit from cohort analysis.

The main finding of the exercise just completed is that if the analyst is willing to assume that at least two age groups, time periods or birth cohorts have identical effects on the dependent variable, then three-way cohort analysis is feasible in the sense of yielding estimable differences between coefficients. This finding is consistent with our reasoning about the confounding of age, period and cohort. These variables are confounded, we have argued, because only one birth cohort exists for any given age and period. Assuming that at least two groups have identical effects on the dependent variable, however, breaks this deadlock. For example, suppose we assume that two age groups have identical effects. Now, when we hold constant the effects of period and cohort, there clearly will be two age groups, not just one, for which we can estimate the independent contribution of age to the dependent variable. Thus, relative to the groups for which we observe behavior, we will now have more than one on which to estimate the contribution of a particular dimension.

IV. STRATEGIES FOR EMPIRICAL ANALYSIS

Thus far we have shown that age, cohort and period effects are estimable under the assumption that two coefficients are equal within one of the three dimensions. We show next that the choice of which two coefficients are assumed equal can affect the estimates of the contributions of age, cohort and period to the dependent variable. We also show that the choice of equality constraint will not affect the model's fit of the dependent variable.

An understanding of these two additional results is crucial for situations in which strong a priori reasons are not available for assuming that two coefficients are equal and that all other pairs of coefficients are unequal. In these situations, analysts using different equality constraints to obtain the minimum conditions for estimability can reach different conclusions about the same data. Because the different estimated models will reproduce variation in the dependent variable identically, it will be difficult to judge which model is most appropriate.

To illustrate these points we perform multiple classification analyses using three sets of hypothetical data which from an intuitive point of view are pure in their effects.³ These data are shown in Tables 1-3. Table 1, discussed earlier, presents pure period effects data. Table 2 presents data with pure co-

³ These pure effects have deliberately been made nonlinear in form. That is, if the rates associated with periods, ages, or cohorts were plotted over these dimensions, the resulting functions would be nonlinear. We create our data in this way because perfectly linear pure effects are inherently ambiguous to interpret, and can be estimated equally well by the pure effect variable or by the remaining two variables in the cohort analysis (see equation (1)).

Table 2: Data Constructed to Show Pure Cohort Effects.

Ages	Time Period						
	1	2	3	4	5	6	7
1	37	40	42	43	45	47	50
2	33	37	40	42	43	45	47
3	31	33	37	40	42	43	45
4	27	31	33	37	40	42	43
5	29	27	31	33	37	40	42
6	30	29	27	31	33	37	40
7	34	30	29	27	31	33	37
8	37	34	30	29	27	31	33
9	41	37	34	30	29	27	31
10	44	41	37	34	30	29	27
11	47	44	41	37	34	30	29
12	50	47	44	41	37	34	30

Table 3: Data Constructed to Show Pure Aging Effects.

Ages	Time Period						
	1	2	3	4	5	6	7
1	35	35	35	35	35	35	35
2	37	37	37	37	37	37	37
3	39	39	39	39	39	39	39
4	44	44	44	44	44	44	44
5	47	47	47	47	47	47	47
6	49	49	49	49	49	49	49
7	46	46	46	46	46	46	46
8	43	43	43	43	43	43	43
9	41	41	41	41	41	41	41
10	38	38	38	38	38	38	38
11	37	37	37	37	37	37	37
12	34	34	34	34	34	34	34

hort effects, those in which the dependent rate is constant through time for a given cohort, differing only among cohorts. Finally, Table 3 presents data in which every age group has a distinct rate that remains constant through time, data that would normally be interpreted as showing effects due to the aging process only. For all three sets of data, we introduce random variation in the dependent rates via a table of random numbers before estimating the cohort models.

The first models we consider are those which incorporate only the minimal assumptions needed to achieve estimability; i.e., only two coefficients in one of the three dimensions are assumed equal. Table 4 presents estimates for three models with different equality constraints applied to each of the hypothetical data sets. The three models differ according to which dimension is chosen for the equality-of-parameters assumption. Model (1) assumes that two age groups have an equal effect (represented in the estimation process by excluding both of these age groups from the regression). Model (2) assumes that two time periods have equal effects, and model (3) assumes that two birth cohorts have equal effects. All estimates in Table 4 are obtained by per-

forming ordinary least squares, using the cells of the underlying cohort table as the units of observation.⁴

Because different equality constraints represent different assumptions of varying accuracy about given empirical configurations, we would expect the three models to yield different estimates of age, period and cohort parameters, regardless of the data set to which they are applied. We would also expect that for each data set the models which reproduce the constructed effect most faithfully would be those which impose no equality constraint on the dimension with the constructed effect. As inspection of Table 4 indicates, this is what we find.

The effect parameters estimated for each data set differ from one model to the next. With the pure cohort effects data, for example, the differences between the estimates for the first two cohorts are 4.06 for model (1), 3.55 for model (2) and 9.30 for model (3). Because the equality constraints for models (1) and (2) do not apply to cohorts, the estimates for these two models are generally more faithful than the model (1) estimates to the constructed cohort effects. In models (1) and (2) only the cohort coefficients are significant, but in model (3) the age and period coefficients are also significant. Similar observations, modified suitably, hold for the estimates involving the aging and period effects data. Thus, the particular model selected for estimation can not only affect the particular coefficients to be estimated, but can also affect which dimensions are significant.

Although we do not present figures to demonstrate this, it is also the case that which categories are chosen for the equality-of-effects assumption *within* a given dimension can affect the results of the estimation as well. Again, different equality-of-effects assumptions represent different models with distinct substantive implications, and the conclusions illustrated by Table 4 apply

⁴ Since our emphasis is on the logic of analysis, we do not take up the problem of serial correlation. We use ordinary least squares primarily for convenience. Also, in certain situations logit analysis (Theil, 1970) or the log-linear model (Goodman, 1971) might be used in preference to our approach. We believe we reach the same conclusions using ordinary least squares as we would using either of these techniques.

Table 4: Regression Estimates and R^2 's for Data Constructed to Have Either Cohort or Aging or Period Effects.

Regression Coefficients	Cohort Effects Data			Aging Effects Data			Period Effects Data		
	Model (1)	Model (2)	Model (3)	Model (1)	Model (2)	Model (3)	Model (1)	Model (2)	Model (3)
Period									
1	-1.01	2.05	-32.46*	-1.80	1.63	2.91	3.57	-20.17*	-5.30
2	-0.02	2.52	-26.25*	-1.99	0.87	1.94	0.60	-19.18*	-6.79
3	-1.13	0.90	-22.12*	-1.47	0.81	1.67	-4.28	-20.10*	-10.19
4	0.08	1.61	-15.65*	-0.34	1.38	2.02	-0.18	-12.05*	-4.62
5	0.31	1.33	-10.18*	-0.57	0.57	1.00	2.46	-5.45*	-0.50
6	-0.51	a	-5.76*	-0.57	a	0.21	3.96*	a	2.48
7	a	a	a	a	a	a	a	a	a
Age									
1	a	a	a	a	a	a	a	a	a
2	a	0.51	-5.24*	a	0.57	0.79	a	-3.96*	-1.48
3	-0.14	0.87	-10.64*	3.57*	4.71*	5.14	1.79	-6.12*	-1.16
4	-0.18	1.35	-15.92*	8.24*	9.95*	10.60*	2.13	-9.74*	-2.30
5	0.35	2.39	-20.63*	10.39*	12.67*	13.53*	2.53	-13.30*	-3.38
6	-0.85	1.69	-27.08*	12.75*	15.61*	16.68*	2.13	-17.65*	-5.26
7	-0.51	2.54	-31.98*	9.32*	12.75*	14.03	3.17	-20.56*	-5.70
8	-0.38	3.18	-37.10*	5.48	9.48*	10.98	4.14	-23.58*	-6.20
9	-0.40	3.67	-42.37*	3.24	7.81	9.52	5.15	-26.50*	-6.67
10	-0.54	4.04	-47.75*	0.56	5.70	7.63	6.34	-29.26*	-6.96
11	-0.20	4.89	-52.66*	-1.16	4.55	6.69	7.61	-31.94*	-7.16
12	-0.34	5.26	-58.04*	-3.84	2.44	4.80	6.26	-37.25*	-10.00
Cohort									
1	-0.66	-9.31	88.51*	4.65	-9.07	-8.71	-9.82	57.42*	13.30
2	-4.72	-12.86	79.21*	5.90	-3.24	-6.67	-9.02	54.27*	14.63
3	-7.25	-14.89	71.43*	6.57	-2.00	-5.22	-7.36	51.97*	14.80
4	-9.86	-16.99	63.58*	4.70	-3.30	-6.30	-8.01	47.36*	12.67
5	-13.07	-19.69*	55.12*	3.78	-3.65	-6.44	-7.53	43.89*	11.68
6	-17.89	-24.00*	45.06	4.86	-2.00	-4.57	-6.46	41.00*	11.27
7	-20.93*	-26.53*	36.77	5.20	-1.09	-3.44	-5.70	37.81*	10.55
8	-22.60*	-27.69*	29.86	2.89	-2.83	-4.98	-6.31	33.24*	8.46
9	-25.30*	-29.10*	23.90	1.32	-3.82	-6.40	-6.31	31.50*	8.10
10	-19.23*	-23.30*	22.74	3.82	-0.75	-2.46	-3.02	28.62*	8.60
11	-18.00*	-21.56*	18.72	3.00	-1.00	-2.50	-4.00	23.69*	6.34
12	-14.20*	-17.25*	17.28	2.93	-0.50	-1.79	-2.69	21.04*	6.17
13	-11.48*	-14.03*	14.74	1.83	-1.02	-2.10	-2.19	17.59*	5.20
14	-9.36*	-11.39*	11.63	2.75	0.47	-0.39	-1.48	14.34*	4.43
15	-7.89*	-9.42*	7.85	2.92	1.20	0.56	-1.54	10.33*	2.90
16	-5.55*	-6.87*	4.94	1.53	0.38	-0.05	-0.26	8.18*	3.20
17	-5.24*	-5.76*	a	0.79	0.21	a	-1.48	2.48	a
18	a	a	a	a	a	a	a	a	a
Intercept	52.00	52.00	52.00	34.00	34.00	34.00	49.00	49.00	49.00
R^2 's									
R^2 (Total)	.96438	.96438	.96438	.96435	.96435	.96435	.89640	.89640	.89640
R^2 (P+A)	.38353	.38330	.38972	.94579	.94602	.94610	.86150	.78312	.86280
R^2 (P+C)	.96290	.96258	.95810	.34392	.34255	.34369	.86194	.79104	.86096
R^2 (A+C)	.95972	.95973	.95450	.96052	.96067	.96064	.28345	.28345	.28326
R^2 (P)	.02164	.01522	.02164	.00393	.00385	.00393	.81845	.73876	.81845
R^2 (A)	.36188	.36808	.36808	.94185	.94216	.94216	.04306	.04436	.04436
R^2 (C)	.95771	.95771	.95231	.33617	.33617	.33609	.08077	.08077	.08050

*Significant at the .05 level.

aExcluded from the regression.

fully to the problem of choosing different equality constraints within a given dimension.

Table 4 reveals, for models which make only the minimum assumptions needed to achieve estimability, a striking pattern with regard to the multiple coefficients of determination and the predicted values on the dependent variable, \hat{Y}_{ij} . Regardless of the particular specification placed on these models, the coefficients of determination and predicted values for each are identical. Thus, for models of this form, we have no way to choose a best fitting model, even if we are willing to use such an empiricist criterion. Clearly, the researcher who approaches the data without strong a priori conceptions and who uses models of this form will face serious interpretative difficulties. Distinct models are likely to tell distinct "stories," but all will

do an equally good job of explaining the data.

That each of the three models fits the data equally well is the consequence of a general result. Graybill (1961:235-7) shows that alternative sets of minimal restrictions needed to achieve estimability will fit the data equally well, regardless of the multiple classification model under consideration. A way of overcoming the dilemma created by this outcome is to employ more restrictions than the minimum needed for estimability. In this case, alternative sets of restrictions will lead to distinct fits of the data. This suggests that a clearer picture of the "true" effects in a given set of cohort data might be obtained by comparing the results from several distinct models making more than the minimum assumptions needed for estimability.

Although such models may be created in several ways, we have chosen the following rule: Make the effects of two pairs of coefficients equal. In Table 5 we present estimates for three such models, again using the hypothetical data. Model (1) assumes two time periods and two age groups have equal effects (again, represented in the estimation by exclusion of all four dummy variables from the regression). Model (2) assumes two periods and two cohorts have equal effects, and model (3), two cohorts and two ages. In each model, a single category on the third dimension is also excluded from the regression.

As Table 5 indicates, the predicted values and coefficients of determination for these three models are now distinct, although differences in the coefficients of determination are for the most part small. Moreover, al-

though the differences between estimated coefficients for each data set are not the same across models, the results in terms of the significance of whole dimensions appear to be somewhat more stable than those for the original models presented in Table 4. Also, despite the small differences among the total R^2 's for the three models applied to a given data set, the model with the largest R^2 seems invariably to give estimates that are closest in form to the assumptions under which the data were created. In the cohort effects data, for example, the model which groups effects on age and period but leaves the effects of cohorts free to vary is both the one with the highest coefficient of determination and with the best picture of the "true" cohort effects used in creating the data.

Although the data we have used represent a minute subset of all the possible pat-

Table 5: Further Regression Models for Cohort Effects Data, Aging Effects Data, and Period Effects Data, with Associated R^2 's.

Regression Coefficients		Cohort Effects Data			Aging Effects Data			Period Effects Data		
		Model (1)	Model (2)	Model (3)	Model (1)	Model (2)	Model (3)	Model (1)	Model (2)	Model (3)
Period	1	1.07	0.46	-4.06	0.52	1.68	-1.35	-12.53*	-19.48*	2.71
	2	1.72	1.22	-2.58	-0.03	0.92	-1.60	-12.93*	-18.62*	-0.13
	3	0.28	-0.11	-3.21	0.11	0.85	-1.16	-15.23*	-19.67*	-4.87
	4	1.16	0.89	-1.50	0.86	1.41	-0.10	-8.56*	-11.74*	-0.63
	5	1.06	0.89	-0.79	0.27	0.59	-0.41	-3.35*	-5.26*	2.15
	6	a	a	a	a	a	a	a	a	a
	7	a	a	a	a	a	a	a	a	a
Age	1	a	a	a	a	a	a	a	a	a
	2	a	-0.31	a	a	0.60	a	a	-3.60*	a
	3	0.34	0.02	-0.86	4.11*	4.74*	3.67*	-1.99	-5.75*	1.59
	4	0.84	0.20	-1.38	9.16*	10.00*	8.42*	-4.22*	-9.24*	1.79
	5	1.50	0.95	-1.34	11.68*	12.73*	10.54*	-6.39*	-12.68*	2.05
	6	0.62	-0.03	-3.03	14.41*	15.67*	13.08*	-9.36*	-16.91*	1.52
	7	1.30	0.54	-3.18	11.35*	12.82*	9.72*	-10.91*	-19.70*	2.42
	8	1.76	0.89	-3.54	7.89*	9.57*	5.96	-12.51*	-22.56*	3.26
	9	2.07	1.09	-4.05	6.02	7.91	3.78	-14.11*	-25.39*	4.12
	10	2.27	1.18	-4.68	3.72	5.81	1.18	-15.50*	-28.03*	5.17
	11	2.94	1.74	-4.83	2.36	4.67	-0.47	-16.80*	-30.58*	6.31
	12	3.14	1.83	-5.45	0.05	2.57	-3.08	-20.73*	-35.77*	4.82
Cohort	1	-6.20	-0.73	10.75	-1.58	-5.39	2.94	33.26*	53.72*	-6.61
	2	-9.93	-4.57	6.20	0.05	-3.55	4.26	31.50*	50.70*	-5.94
	3	-12.14	-6.88	3.18	1.09	-2.30	5.01	30.57*	48.52*	-4.42
	4	-14.41	-9.26	0.09	-0.41	-3.59	3.21	27.35*	44.04*	-5.21
	5	-17.29*	-12.26	-3.62	-0.96	-3.93	2.36	25.25*	40.69*	-4.87
	6	-21.79*	-16.88*	-8.90	0.48	-2.26	3.51	23.86*	37.93*	-3.93
	7	-24.48*	-19.67*	-12.44	1.21	-1.34	3.93	21.93*	34.85*	-3.31
	8	-25.92*	-21.11*	-14.60	-0.74	-3.08	1.68	18.74*	30.41*	-4.06
	9	-28.21*	-23.60*	-17.81*	0.58	-1.55	2.70	17.37*	27.79*	-2.98
	10	-21.79*	-17.30*	-12.21	0.95	-0.97	2.77	16.87*	26.04*	-1.04
	11	-20.21*	-15.81*	-11.51	0.52	-1.22	2.03	13.14*	21.21*	-2.17
	12	-16.10*	-11.83*	-8.15	0.79	-0.70	2.02	12.07*	18.70*	-0.99
	13	-13.05*	-8.89*	-5.93	0.07	-1.22	1.00	10.00*	15.38*	-0.62
	14	-10.60*	-6.54*	-4.26	1.36	0.29	1.99	8.15*	12.25*	-0.05
	15	-8.80*	-4.86*	-3.30	1.90	1.03	2.23	5.53*	8.36*	-0.25
	16	-6.13*	-2.30	-1.45	0.87	0.22	0.91	4.78*	6.34*	1.42
	17	-5.50*	a	a	0.50	a	a	0.50	a	a
	18	a	a	a	a	a	a	a	a	a
Intercept		52.00	48.44	48.75	34.00	34.13	34.49	49.00	50.53	48.08
R^2 's										
R^2 (Total)		.96423*	.95890*	.95882*	.96401*	.96434*	.96417*	.85784*	.89208*	.89486*
R^2 (P+A)		.37711*	.38330*	.38353*	.94570*	.94602*	.94579*	.78182*	.78312*	.86150*
R^2 (P+C)		.96258*	.95718*	.95810*	.34255	.32427	.34369	.79104*	.79085*	.86096*
R^2 (A+C)		.95972*	.95450*	.95432*	.96052*	.96064*	.96043*	.28345	.28326	.28326
R^2 (P)		.01522	.01522	.02164	.00385	.00385	.00393	.73876*	.73876*	.81845*
R^2 (A)		.36188*	.36808*	.36188*	.94185*	.94216*	.94185*	.04306	.04436	.04306
R^2 (C)		.95771*	.95231*	.95231*	.33617*	.33609*	.33609*	.08077	.08058	.08058

* Significant at the .05 level.

a Excluded from the regression.

terns to be found in cohort tables, the results seen for these data nonetheless suggest some procedures that might provide clues about the nature of aging, cohort and period effects for the analyst unable or unwilling to make a priori constraints on the cohort model. First, the analyst in this position can estimate a variety of models for a given set of data, where these models make more than the minimum number of assumptions needed for estimability. If the general nature of estimates is similar for all models (e.g., which dimensions are significant remains constant over all), then it is probably safe to interpret these findings in substantive terms. Second, if there is some variation in the estimates among models, then the coefficients of determination may be used to select the best-fitting model.

A third approach to three-way cohort analysis may be helpful in situations for which a priori information is scarce. A step-wise procedure in which whole dimensions are added or excluded from any given model may provide additional information about the ability of these dimensions to explain variance in the dependent variable. Certainly, if the data contain a single, pure effect, as they do in our hypothetical sets, such step-wise changes will tend to tell the "true" story. Tables 4 and 5 present the coefficients of determination for the different steps, and in both tables the various increments and decrements in R^2 show that the dimension whose effects were constructed into the data was the largest component of the explained variance. Thus, with the pure cohort effects data, for example, the coefficient associated with the regression on cohorts alone [$R^2(C)$] is much larger than those associated with regression on ages [$R^2(A)$] or on period [$R^2(P)$] alone. Similarly, the difference between any total coefficient [$R^2(\text{Total})$] and R^2 's for equations excluding a single dimension is largest when the cohort dimension is excluded.⁵

⁵ Comparison of R^2 's across dimensions, however, is complicated by the fact that the number of categories in a particular dimension will also affect the degree of fit. There is a correction for the effects of this variation from one dimension to the next in the number of categories (Goldberger, 1964:217). Yet another complication is that increments in the (corrected) R^2 convey a much more confusing message if the "pure" effects in the data

Thus, in situations for which a priori information is limited, there are nonetheless strategies which may be helpful. These are (1) to compare results across a number of more-than-minimally restricted models. Similarity of results across models helps justify interpretation of the general thrust of the results. (2) If the results across these models lead to different interpretations of the data, then the best-fitting model can be selected in the absence of reasons for not doing so. (3) Examine the step-wise changes in variance explained to obtain additional clues about the nature of the underlying effects. These strategies will not necessarily yield unambiguous results; but given the circumstances in which they might be applied, they are relatively rigorous ways to attempt three-way analyses where these are conceptually desirable.

V. ADDITIONAL CONSIDERATIONS

Estimability and Alternative Specifications

The three-way cohort model considered in this paper is flexible with respect to functional form and general in that it may be applied to widely different substantive problems. It is, however, only one approach the researcher might consider in cohort analysis. Ryder (1965; 1968), for example, suggests an approach (which has been given one formalization by Carlsson and Karlsson, 1970), in which the effects of membership in particular cohorts are variable through time, rather than fixed as is assumed in the model considered in this paper. The estimability of models using this approach is thus far unexplored. We do not examine the estimability of other models here. However, the researcher who finds Ryder's, or any other, approach substantively attractive should do so before performing data analysis. As this

are linear rather than nonlinear. Here, as would be expected from equation (1), the R^2 for the "pure" variable alone is about the same as the R^2 for a regression on the remaining variables together. Although it may be unusual to find linear effects in real cohort data, the possibility nonetheless exists. Thus, in cases where the underlying effects are linear, the increments in variance explained by whole classifications will provide a more ambiguous picture of the data than is the case when the underlying effects are nonlinear—largely, however, because interpretation of linear cohort, age or period effects is logically less clear-cut.

paper has illustrated, not all cohort models are estimable, and empirical results of cohort analysis can be misleading if this is not understood.

Cohort Analysis and Quantitative Techniques

As in other areas of research, there are distinct advantages of doing cohort analysis with the kinds of rigorous quantitative estimation techniques illustrated by the use of multiple classification in this paper. One important feature of this methodology, as opposed to a cross-tabular approach, is that it requires an explicit and complete specification of the model. This in turn makes interpretation of cohort data more clear than is otherwise the case. The ambiguities that can arise when specification is incomplete are illustrated by a recent controversy over the nature of party identification in the United States. An early researcher in this area (Crittenden, 1962) has been criticized by Cutler (1969-70) for ignoring possible cohort effects on party identification, and Cutler in turn has been criticized for misspecifying the nature of period effects (Glenn and Hefner, 1972). All these authors have used only cross-tabulations, and have thereby avoided making a complete and explicit specification. At least some of the controversy over their results might have been avoided had a quantitative model been posited and estimated.

Another important feature of the quantitative approach illustrated in this paper is the ease with which additional variables can be incorporated into the analysis. This ability is important for two reasons. First, it is often desirable to insure that cohorts are consistent in social composition over time before making substantive inferences about aging, period or cohort effects, and in such cases there is an obvious need to control for variables other than age, period and cohort alone. As Glenn has noted in several past articles (e.g., 1970), the need to standardize cohorts arises in Gallup Poll time series because of sample biases that have changed over time. Moreover, in any cohort analysis, the need to control for compositional changes can arise from the normal effects of social differentials in mortality. Such controls might

be achieved in several ways, but quantitative methods such as multiple classification make this a relatively simple task.

Secondly, the ability to add variables to the basic cohort model is also important when the analyst is concerned with incorporating cohort analysis into the more general framework of causal modeling. Here, especially, the quantitative approach typified by multiple classifications offers the greatest advantages over other methodologies. Although it is possible to control for additional variables through standardization techniques (those favored by Glenn and his coworkers; see, e.g., Glenn and Grimes, 1968; Glenn and Zody, 1970), these techniques do not readily yield estimates of the effects of these other variables. Clearly, when the interest is in building a more general causal model, part of which employs period, age and cohort, there will be interest not only in controlling for additional variables, but in estimating their impact as well. For this purpose, a technique such as multiple classification analysis offers great advantages in simplicity and rigor over standardization techniques.

VI. SUMMARY

This paper has made three points. First, we have argued that cohort analyses which ignore one of the three dimensions of age, birth cohort and time period are often unsatisfactory on substantive grounds. Because age, cohort membership and time period often index distinct sets of causal factors (not directly measurable), an analysis that attempts to describe behavior in terms of only two of these dimensions is subject to spurious findings.

Secondly, we have shown that the confounding of age, period and cohort observed by past users of cohort analysis is real, and makes estimation of all age, period and cohort effects impossible. Models in which at least two age groups, two periods or two birth cohorts are assumed to have identical effect parameters, however, *are* estimable.

Finally, we have shown that cohort analyses performed without prior knowledge or strong theoretical preconceptions about which parameters are identical are subject to errors of interpretation. The estimates derived from different cohort models can be quite distinct,

and, as we have seen for cases in which the underlying effects in the data are known, can produce misleading results. However, incorporating an additional equality constraint on a second dimension into the estimation of several alternative cohort models, along with estimating changes in the coefficient of determination when classifications are added to and removed from the total model, may provide some clues, even when little prior knowledge exists about the behavior under study. A carefully applied three-way cohort analysis, then, can yield information about cohort, aging and period effects.

APPENDIX

We state a result useful in determining the estimability of any multiple classification model and then apply this result to a particular example of a cohort model. For a general discussion of estimable functions, see Graybill (1961: 223-53).

Consider

$$(A-1) \quad E\{Y\} = X\beta,$$

where Y is of dimension $N \times 1$, where X is of dimension $N \times p$, is fixed and of rank $r < p$, and where β is of dimension $p \times 1$. E denotes the expectation operator. For this model we will be able to find a $p \times m$ matrix, K , of linear constraints, which when applied to the normal

equations will give a set of estimates for β^* , related to the subset of freely-varying parameters of β (call these β°) as follows:

$$(A-2) \quad \beta^* = K\beta^\circ.$$

Suppose now that we are interested in estimating a series of linear combinations among the original elements of β , specified by a matrix L . The combination $L\beta$ is estimable if and only if there is some other set of linear combinations, L_1 , such that

$$(A-3) \quad E\{L_1\hat{\beta}^\circ\} = L\beta.$$

From this, it can be shown that $L\beta$ is estimable if and only if

$$(A-4) \quad LK(K'X'XK)^{-1}K'X'X = L.$$

This equation provides a simple means by which to assess the estimability of any multiple classification model. Note that any set, K , of legitimate (i.e., nonestimable) linear constraints may be used in (A-4) since estimability is independent of restrictions used in solving the normal equations. Also, once we have found some K which provides solutions to the normal equations, estimability of $L\beta$ can be proved using only that K .

The Full Cohort Model. The model given as equation (2) in the text of this paper can be represented by a design matrix, X , of the following form, for the case where we have five age groups, three time periods and seven cohorts:

$X =$
 15×16

μ	β_1	β_2	β_3	β_4	β_5	γ_1	γ_2	γ_3	δ_1	δ_2	δ_3	δ_4	δ_5	δ_6	δ_7
1	1	0	0	0	0	1	0	0	0	0	0	0	1	0	0
1	1	0	0	0	0	0	1	0	0	0	0	0	0	1	0
1	1	0	0	0	0	0	0	1	0	0	0	0	0	0	1
1	0	1	0	0	0	1	0	0	0	0	0	1	0	0	0
1	0	1	0	0	0	0	1	0	0	0	0	0	1	0	0
1	0	1	0	0	0	0	0	1	0	0	0	0	0	1	0
1	0	0	1	0	0	1	0	0	0	0	1	0	0	0	0
1	0	0	1	0	0	0	1	0	0	0	0	1	0	0	0
1	0	0	1	0	0	0	0	1	0	0	0	0	1	0	0
1	0	0	0	1	0	1	0	0	0	1	0	0	0	0	0
1	0	0	0	1	0	0	1	0	0	0	1	0	0	0	0
1	0	0	0	1	0	0	0	1	0	0	0	1	0	0	0
1	0	0	0	0	1	1	0	0	1	0	0	0	0	0	0
1	0	0	0	0	1	0	1	0	0	1	0	0	0	0	0
1	0	0	0	0	1	0	0	1	0	0	1	0	0	0	0

For this model, we would usually want to estimate the contrasts specified by:

$$L = \begin{matrix} 11 \times 16 \\ \begin{bmatrix} \mu & \beta_1 & \beta_2 & \beta_3 & \beta_4 & \beta_5 & \gamma_1 & \gamma_2 & \gamma_3 & \delta_1 & \delta_2 & \delta_3 & \delta_4 & \delta_5 & \delta_6 & \delta_7 \\ 0 & 1 & 0 & 0 & 0 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 1 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & -1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & -0.5 & -0.5 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & -0.5 & -0.5 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & -0.5 & -0.5 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & -0.5 & -0.5 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & -0.5 & -0.5 \end{bmatrix} \end{matrix}$$

We can now specify any constraint matrix, K , and check if $L\beta$ for the unconstrained cohort model is estimable using equation (A-4).

Consider the following matrix which imposes the conditions $\beta_3 = \gamma_2 = \delta_4 = \delta_7 = 0$ and permits us to solve the normal equations:

$$K = 16 \times 12$$

$$\begin{bmatrix} 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \end{bmatrix}$$

Using APL, an interactive programming language that operates on arrays, we determine that $LK(K'X'XK)^{-1}K'X'X \neq L$, and hence conclude that $L\beta$ is not estimable, given the model specified by X in this Appendix. This result is not a consequence of the dimensions of X . If X is increased in size so that the number of observations is greater than the number

of parameters to be estimated, the combination $L\beta$ is still inestimable. It is the structure of the model specified by X , not its size, that determines this result.

A Constrained Cohort Model. Suppose that we change the original model, constraining to have only six cohort effects. Now,

$$\mathbb{K} = 15 \times 12$$

This \bar{K} satisfies (A-4) with the design matrix \bar{X} and hence $L\beta$ is estimable for this constrained cohort model.

We again note that the constraints which led to estimability are in the model, which is designated by the design matrix, \bar{X} , and not in \bar{K} , which states the side conditions imposed to solve the normal equations. Having found that the constrained cohort model permits estimability of the relevant contrasts of parameters, we can go on to specify additional constraints on the design matrix. In particular, we can add the constraint that two age groups have the same effect. This model will also permit estimability of the important contrasts.

REFERENCES

- Baltes, Paul B.
1968 "Longitudinal and cross-sectional sequences in the study of age and generation effects." *Human Development* 11 (3):145-71.
- Baltes, Paul B. and John R. Nesselroade
1970 "Multivariate longitudinal and cross-sectional sequences for analyzing ontogenetic and generational change: a methodological note." *Developmental Psychology* 2 (September):163-68.
- Baltes, Paul B. and Guenther Reinert
1969 "Cohort effects in cognitive development of children as revealed by cross-sectional sequences." *Developmental Psychology* 1 (September):169-77.
- Blalock, H. M., Jr.
1967 "Status inconsistency, social mobility, status integration and structural effects." *American Sociological Review* 32 (October):790-801.
- Carlsson, Gosta and Katarina Karlsson
1970 "Age, cohorts and the generation of generations." *American Sociological Review* 35 (August):710-18.
- Cattell, Raymond B.
1970 "Separating endogenous, exogenous, epigenetic and epogenic component curves in developmental data." *Developmental Psychology* 3 (September):151-62.
- Crittenden, John
1962 "Aging and party affiliation." *Public Opinion Quarterly* 26 (Winter):648-57.
- Cutler, Neal E.
1969-70 "Generation, maturation, and party affiliation: a cohort analysis." *Public Opinion Quarterly* 33 (Winter):583-91.
- Evan, William M.
1959 "Cohort analysis of survey data: a procedure for studying long-term opinion change." *Public Opinion Quarterly* 23 (Spring):63-72.
1968 "Cohort analysis of attitude data." Pp. 158-82 in James M. Beshers (ed.), *Computer Methods in the Analysis of Large-Scale Systems*, Revised Edition. Cambridge: M.I.T. Press.
- Glenn, Norval D.
1970 "Problems of comparability in trend studies with opinion poll data." *Public Opinion Quarterly* 34 (Spring):82-91.
- Glenn, Norval D. and Michael Grimes
1968 "Aging, voting, and political interest." *American Sociological Review* 33 (August):563-75.
- Glenn, Norval D. and Ted Hefner
1972 "Further evidence on aging and party identification." *Public Opinion Quarterly* 36 (Spring):31-47.
- Glenn, Norval D. and Richard E. Zody
1970 "Cohort analysis with national survey data." *The Gerontologist* 10 (Autumn):233-40.
- Goldberger, Arthur S.
1964 *Econometric Theory*. New York: Wiley.
- Goodman, Leo A.
1970 "The multivariate analysis of qualitative data: interactions among multiple classifications." *Journal of the American Statistical Association* 65 (March):226-56.
- Graybill, Franklin A.
1961 *An Introduction to Linear Statistical Models*, Volume I. New York: McGraw-Hill.
- Greenberg, B. G., John J. Wright and Cecil G. Sheple
1950 "A technique for analyzing some factors affecting the incidence of syphilis." *Journal of the American Statistical Association* 45 (September):373-99.
- Keyfitz, Nathan
1972 "Oscillations in a demographic-economic model." Madison, Wisconsin: Unpublished paper presented at the Conference on Population Dynamics, Sponsored by the Mathematics Research Center, University of Wisconsin, July, 1972.
- Klecka, William R.
1971 "Applying political generations to the study of political behavior: a cohort analysis." *Public Opinion Quarterly* 35 (Fall):358-73.
- Oppenheim, Karen
1970 "Voting in recent American presidential elections." Chicago: University of Chicago unpublished Ph.D. dissertation.
- Ryder, Norman B.
1965 "The cohort as a concept in the study of social change." *American Sociological Review* 30 (December):843-61.
1968 "Cohort analysis." Pp. 546-50 in David L. Sills (ed.), *International Encyclopedia of the Social Sciences*, Volume 2. New York: The Macmillan Company & The Free Press.
- Schaie, K. Warner
1965 "A general model for the study of developmental problems." *Psychological Bulletin* 64 (August):92-107.
1967 "Age changes and age differences." *The Gerontologist* 7 (June):128-32.
- Schaie, K. Warner and Charles R. Strother
1968a "A cross-sequential study of age changes in cognitive behavior." *Psychological Bulletin* 70 (December):671-80.

- 1968b "The effect of time and cohort differences on the interpretation of age changes in cognitive behavior." *Multivariate Behavioral Research* 3 (July):259-93.
- Theil, Henri
1970 "On the estimation of relationships involving qualitative variables." *American Journal of Sociology* 76 (July):103-54.
- Winsborough, H. H.
1972 "Age, period, cohort and education effects on earnings by race—an experiment with a sequence of cross-sectional surveys." Madison, Wisconsin: University of Wisconsin, Center for Demography and Ecology, unpublished paper presented at the July, 1972 Russell Sage Foundation Conference on Social Indicator Models.
- Winsborough, H. H. and Peter Dickinson
1972 "Age, period, and cohort components of U.S. fertility." Madison, Wisconsin: University of Wisconsin, Center for Demography and Ecology, Working Paper 72-6 (June).

PSYCHOLOGICAL REDUCTIONISM, METHODOLOGICAL INDIVIDUALISM, AND LARGE-SCALE PROBLEMS

MURRAY WEBSTER, JR.

The Johns Hopkins University

American Sociological Review 1973, Vol. 38 (April):258-273

I examine prospects for reducing sociology to psychology, distinguishing "theoretical reduction" from "empirical reduction," and applying minimum conditions for producing a general reduction. Reductionist claims frequently arise from a confusion with the more general orientation "methodological individualism," and are related to issues of holism and emergent properties. Difficulties entailed by the formal requirements of successful reduction are shown to preclude its accomplishment at this time, and some implications of the analysis for sociological theory construction are considered.

SOCIOLOGISTS generally seem less than enthusiastic about the idea of *reductionism*, a situation which is understandable in view of the connotations of the word. Taken to its extreme, a reductionist view seeks to make sociologists obsolete: if social structures and sociological theories can best be explained psychologically, then sociological explanations are unnecessarily complex. Even superficially, to speak of "reducing" sociology to psychology clearly implies the diminished importance of sociological phenomena, and suggests that they are simply special cases of something more "fundamental."

Close study of just what is meant by reductionism reveals that, at the present state of knowledge, psychological reduction of sociological phenomena or sociological theories has not been accomplished. Moreover, there is no good reason to believe that *in principle* a general reduction could be carried out.

Publication in 1958 of Homans' "Social Behavior as Exchange" provided the foundation for a reorientation of American sociol-

ogy.¹ The family of exchange theories which followed this article constitute the first generally accepted theoretical rationale for studying "small-scale" problems, and an accepted way of interpreting both small-scale and large-scale phenomena on the individual level.²

Any successful analysis of large-scale phenomena in individual terms raises the issue

¹ Prior to this time, the dominant—virtually the only—theoretical orientation in general use was functionalism, which does not lend itself easily to analyzing individual action. Balance theories had been proposed by Newcomb (1953) and as early as 1944 by Heider, and are individualistic in orientation. However, since balance theories were primarily of interest to psychologists before about 1960, it seems reasonable to credit Homans' 1958 article with providing the first focus of sociological interest in what were called "micro-level" processes.

² Questions of the adequacy of exchange theories on other grounds will not be considered here (one study is Maris, 1970, 1971). From our point of view, the significance of the exchange point of view is that it provides a generally accepted theoretical framework to legitimate sociological interest in individual actors.

of psychological reduction: Could all phenomena be interpreted in terms of individuals? If so, would knowledge of psychology give a complete understanding of sociology? Are psychological explanations in some sense more basic or more general than sociological explanations? These and related issues appear frequently in sociological literature.³

The central question is whether adequate explanations of sociological phenomena (social facts) and sociological propositions ("laws," or generalizations) may be constructed using psychological propositions. In order to explain something—either a social fact or a sociological proposition—it must be shown that some combination of propositions, along with appropriate situational "given" conditions, enables its derivation using standard rules of logic. Thus "derivation" and "deduction" both are equivalent to "logical implication," which must be shown for explanation: To explain something means to deduce it. A theory of something is an interrelated set of general propositions sufficient to explain it: A theory of something is an explanation of it.⁴

³ The volume of related literature precludes any comprehensive review, or even an adequate summary of these statements. Blain and Cioffi (1971), Burgess and Bushell (1969), and Borger (1970) contain good bibliographies. Homans provides unusually clear, non-technical, persuasive, and influential articulation of his reductionist claims (1958, 1961, 1964, 1967, 1971), though without such an extensive bibliography.

⁴ These meanings of "theory" and "explanation" are standard in most sociological literature, and are consistent either with formal axiomatic explanation or with informal "logical" explanation. They are also developed by Homans in one of his reductionist statements (see Homans, 1967:21-31). Other usages of the term "explanation"—interpretation, describing subjective meaning, transforming the unfamiliar to the familiar, making a mechanical "model" of assumed processes, reasoning by analogy—do exist, and, particularly in psychology, some have attracted the serious attention of a minority of investigators. For purposes of this analysis what is significant is that the standard usage of terms is the only one for which it is clear what reduction would entail, and it seems very likely to be far easier to perform a reduction using the "deductive explanation" interpretation than using any of the other meanings. In showing why psychological reduction is highly unlikely, therefore, it seems wise to present the analysis in terms of the "strongest" case.

Two sorts of explanations may be distinguished: (1) explaining something about phenomena—that is, being able to deduce a given (social) fact from general (psychological) propositions and conditions; and (2) explaining propositions—that is, showing that any proposed (sociological) "laws" may be deduced from more general (psychological) propositions. An extreme reductionist position asserts that all sociological phenomena or social facts, and all sociological propositions or "laws" can only be explained by using psychological propositions or "laws."

Methodological Individualism and Psychological Reductionism

A position sharing some elements with psychological reductionism is methodological individualism; and perhaps because of the similarity, the two are often confused. Therefore, clearly distinguishing the terms is crucial. Methodological individualism is a strategy of theory construction which seeks to explain any social institution or phenomenon using individuals as the basic unit of analysis (see Brodbeck, 1968).⁵ Thus, a theory constructed by a methodological individualist would necessarily include statements about individuals and how they behave. It might, of course, also include statements about collectives, but at base, these statements would be derivable from statements explicit in the theory about the behavior of individuals. Beyond this general strategic orientation, there is little agreement among theorists of methodological individualistic persuasion. Sociologists with interests as varied as J. Berger, Blau, Coleman, J. Davis, Homans, Newcomb, Stinchcombe, and Zetterberg all apply the strategy of methodological individualism in some or all of their work.

⁵ Some sociologists object to the idea of following the directives of philosophers of science in our theoretical activities. Without entering this argument here, we merely note that this part of the discussion is not prescriptive. No suggestion that we follow Brodbeck (or any other philosopher) is made in this paper. Rather, we are simply relying on the analytic abilities of a philosopher for a convenient and simple description of what sociologists and other empirical scientists have already done, and in fact are still doing.

Methodological individualists deny, by the types of theories they construct, that "the whole is more than the sum of its parts." Methodological individualists do not deny the occurrence of group characteristics; they do deny that any group characteristic is undefinable in terms of the individuals comprising the group. Thus a sociologist who adheres to methodological individualism would not build a theory including the concept "mob mind" or "national mood" unless at the same time he defined the mob mind or the national mood in such a way that the other propositions predicated on individuals in his theory could define it and explain the way it operated. For a methodological individualist, the phenomena of interest are explained by propositions predicated on individuals and their behavior, and the concepts of the theory are concepts which inhere in individuals, not irreducibly in groups.

The opposing doctrine, that there *are* properties of groups which are undefinable in terms of the individuals constituting the groups, is usually called methodological holism.⁶ If we consider "mob mind" to be in principle inexplicable in terms of the wishes and interrelations of individuals, or if we assert that the "mood of the country" is not explainable in terms of the composite moods of various individuals, then we adopt the view of methodological holism. Many contemporary sociologists, necessarily including psychological reductionists, reject the holistic approach; however by no means all sociologists are anti-holists.

This discussion is germane to a special sort of objection which is sometimes raised to the idea of reductionism, one based on ideas of holism. Mandelbaum (1955) argues

⁶ Mandelbaum (1957) argues that one may deny methodological individualism without accepting methodological holism. A theory of "historical trends" such as Marx's is given as an example, for such concepts as "historical era" may be undefinable in terms of any collection of individuals. Others (especially Popper, 1957) argue that Marxian theories are holistic. The discussion of "emergent properties" in this paper may be related to this issue, since what appear to be irreducible concepts at one time may not appear irreducible with the benefit of later knowledge. In any case, for purposes of delineating methodological individualism, it is helpful to contrast it to holism.

against reductionism using the example of a transaction between a teller and a customer in the bank (1955:308-9). The exposition unfolds by describing a social fact (or phenomenon) of their interaction situation, and asking a hypothetical reductionist to explain it in psychological terms. If he can do this, another feature of the interaction is presented, and explanation in psychological terms again is demanded. This process is repeated, with the purpose of showing that an infinite number of psychological explanations would be required for such sociological concepts as "role" and "status": that is, in order fully to explain the situational meaning of these terms in psychological terms, one would need to have explained an infinite number of previous interactions of both participants—clearly an impossible task.

Yet Mandelbaum's argument appears specious, for what are generally required to be explained are given facts, observations, or findings; not situations. A fact or a finding is a statement about a situation, and an infinite number of statements (most of them uninteresting) could in principle be made about any situation.⁷ No scientific theory, psychological or sociological, could ever hope to explain a situation fully. The only fair task is to isolate ahead of time the facts, findings, or phenomena of interest, and then see whether a given theory can be used to explain these.

If this summary reflects his intent, Mandelbaum (1955) argues both for the irreducibility of certain sociological concepts, and for a holistic total explanation of a situation. While the former position is scientifically tenable—that is, scientific theory construction can take place using "emergent" group concepts—the latter requirement is too difficult for any sort of theory. Here, the two are combined: the argument for irreducibility seems to be based on demonstrating the impossibility of total explanation. Still, as others have noted, not every statement of every theory predicted on individuals is psychological (see Berger, 1969). Sociologists who are methodological individualists have been praised by reductionists for "really do-

⁷ Consider for example: "The participants are both less than forty years old; they are both less than forty-one years old. . . ."

ing psychology" and criticized by other sociologists for "merely doing psychology"; neither assertion is necessarily justified. Psychological reductionism does entail methodological individualism, but the implication does not work the other way. One can think of several different sorts of sentences predicated on individuals which could not be accepted as psychological: "Humans have forty-six chromosomes"; "An individual who smokes is more likely to get cancer than one who does not." Other examples could be cited from religion, physiology, sub-specialities of medicine, free will doctrines of behavior, etc. It seems doubtful that even an ardent reductionist would want to defend a claim that *any* statement predicated on individuals is a psychological assertion.

General Requirements for Performing Reduction

In order to reduce any field to any other—sociology to psychology, psychology to physiology, chemistry to physics—certain general requirements must be met. By examining these requirements and applying them to the specific case of psychological reductionism, it is possible to determine just what activities would be required for a reduction, and thus, the prospects for a successful reduction of sociology to psychology.

We need to distinguish two sorts of sentences, *t* and *o*. Theoretical assertions (propositions) of any field will be designated by t_x for each assertion, and the entire set by T_x . We refer to the body of sociological theory as T_2 and the psychological propositions needed for the reduction as T_1 ; in this case, we talk of reducing T_2 to T_1 . Second, observation statements, "findings" of a field or "facts," will be designated by o_x for each statement, and the entire set by O_x . Then all the findings of sociology are designated by O_2 , and the set of findings of behavioral psychology by O_1 .⁸ We assume that the proposi-

tions of sociology T_2 are adequate to explain the findings of sociology O_2 ; and the propositions of psychology T_1 are adequate to explain the findings of psychology O_1 . If we demonstrate that T_2 are reduced to T_1 , then we must grant that T_1 are also adequate to explain all the findings of O_2 .

What might appear the most direct method of reduction is to deduce T_2 from T_1 ; that is, to work only with the theoretical propositions of the two fields, and to demonstrate that the propositions of sociology follow from (are deducible from) those of behavioral psychology. Such a demonstration, if successful, would constitute a "theoretical reduction"; sociological theory would be reduced to psychology. In fact, this method is hopelessly difficult. The propositions of both fields—of all fields—are not fixed, either in quantity or in their precise formulations. Concepts change subtly as more empirical information becomes available, and refinements of the explanatory assertions relating the concepts are constantly introduced as the result of empirical research. As more findings, o_x 's, become available, any field alters its general explanatory principles, T_x , of those findings.

Yet it would be a mistake to conclude from this that any reduction is impossible; there are examples in other fields where it is generally recognized to have been more or less completely accomplished: chemistry to physics, thermodynamics to statistical mechanics, some parts of biology to biophysics. What is referred to here may be termed "empirical reduction." If all known (and future) findings may be explained by the reducing theory as well as by the one to be reduced, then the reduction has been demonstrated satisfactorily.⁹ If the empirical knowledge of sociology may be explained by the propositions of psychology, even if such

statement or finding considered worth explaining by practitioners in the field under consideration will qualify as an o_x .

⁹ Of course this would not necessarily make the reduced theory obsolete; for some purposes it might continue to provide the clearest and most direct sorts of explanation. Here we are discussing the possibility, not the desirability, of reduction. Sociologists would be willing to adopt psychological explanations if they were demonstrably simpler or more complete than available sociological explanations of the same phenomena.

⁸ Fortunately, it is not necessary to discuss any precise, philosophical meaning of the various *t*'s and *o*'s. Any sentence which could reasonably be considered a proposition of some abstraction within either field qualifies as a t_x , and the exact boundaries of the set of "acceptable" propositions for T need not be specified. Likewise, any observation

psychological explanations be quite involved, then we would accept that the former field has been reduced to the latter.¹⁰

The historian of science Kuhn points out (1970:98-102) that reduction of the theoretical sort is rarely, if ever, accomplished in empirical fields, for this would entail demonstrating a correspondence between very different sorts of concepts in the two theories.¹¹ For example, Newtonian physical theories are not reducible to Einsteinian physical theories under conditions of velocity \ll the speed of light, since the concepts (even when the same words are used) have very different meanings: space is curved to an Einsteinian and empty to a Newtonian, matter is convertible into energy to an Einsteinian and absolute in quantity to a Newtonian, time varies with velocity to an Einsteinian and is a stable measure to a Newtonian, etc. What is usually meant by saying that this reduction has been accomplished in physics is that any observation sentence which can be explained by Newtonian theories can also be explained by Einsteinian theories (and that Einsteinian theories can explain some observations not explainable using Newtonian theories).

Thus the question, of reduction really turns upon the observation statements: can T_1 , the reducing theory, satisfactorily explain O_2 , the findings of the field to be reduced? We also require that no time parameter be attached to O_2 ; that is, we speak of general reduction, not of reduction with respect to currently available empirical knowledge (cf. Nagel, 1949).

¹⁰ Lachenmeyer (1970) concludes that the reduction of sociological propositions to psychological propositions is impossible, at least for the present, due to the lack of explicit logically interrelated theories with determinate concepts in both fields. His discussion omits consideration of the second sort, "empirical reduction," in which sociological findings are explained by psychological propositions. The general possibility of either a "theoretical reduction" or an "empirical reduction," given the lack of rigorous codification of either propositions or findings in both fields is discussed here.

¹¹ For non-empirical fields, concepts seem to undergo less change in their meanings, and thus theoretical reduction is a possibility. Russell and Whitehead's claim in *Principia Mathematica* (1925) that logic is "fundamental" to mathematics is based on their demonstration that Peano's axioms for the real number system can be derived from the rules of logic.

The philosophers Kemeny and Oppenheim (1970) demonstrate that the following definition of reduction meets the criteria discussed above: Reduction has been accomplished if (1) the reducing theory contains terms (concepts) not in the vocabulary of the theory to be reduced, (2) every finding implied (explained) by the theory to be reduced is also explained by the reducing theory, and (3) the logical rigor and integration with other theories of the reducing theory are at least as great as those of the reduced theory. Stated precisely: ¹²

Theory T_2 has been reduced to theory T_1 if and only if:

- (1) The vocabulary of T_1 contains terms not in the vocabulary of T_2 ;
- (2) Every o_2 implied by T_2 is also implied by T_1 ;
- (3) T_1 is at least as well systematized as T_2 .

We may now apply this definition to consider just what reducing sociology to behavioral psychology would entail.

Requirements for Reducing Sociology to Psychology

The above definition of reduction is considerably less stringent than the sort of operation usually implied in discussions of psychological reductionism. In particular, it does not require the deduction of propositions of sociology from those of behavioral psychology, but only that the findings of sociology be deducible from psychological propositions. Nor does it deny the possibility or the existence of sociological laws. Therefore, reduction in this sense might appear to be

¹² Condition (1) simply asserts that the two theories are not identical, nor simply "models" of each other. Condition (2), the crucial one for our discussion, represents formally the "empirical reduction" described in the text. Condition (3) is necessary because without it, T_2 would be able to make some predictions which are too precise for T_1 ; or T_1 might make some bizarre predictions which are avoided by T_2 .

The proof of this theorem is complex, and the reader is referred to the original article. The final step in the proof is:

- (1) T explains $O \equiv T \cdot O_1 \rightarrow O_2$.
- (2) if $T_2 \cdot O_1 \rightarrow O_2$, then $T_1 \cdot O_1 \rightarrow O_2$. (This is analytically true if the set O_1 is empty.) Thus condition (2) of the definition is necessary.
- (3) if $T_2 \cdot O_1 \rightarrow O_2$, then $T_2 \rightarrow (O_1 \rightarrow O_2)$. Hence, $T_1 \rightarrow (O_1 \rightarrow O_2)$, hence $T_1 \cdot O_1 \rightarrow O_2$. Thus condition (2) of the definition is sufficient.

a straightforward and relatively simple matter. However, it is not; for in order for the reductionist claim to have any meaning it is necessary either to demonstrate a successful reduction, or at least a reason to believe that reduction would not be too difficult. Close examination of the definition of the term indicates a number of difficulties with both performing the reduction and with demonstrating its feasibility in principle.

Two requirements of the definition for any reduction are fundamental. First, before performing any reduction we require the existence of two bodies of theory, T_1 and T_2 , along with their definite observation statements O_1 and O_2 . Both T_1 and T_2 must meet high standards of closure and completeness, which we define below. Second, in order for T_1 (the psychological theory) to be able to explain O_2 (the observation statements of sociology), it is necessary to specify an interpretation of the psychological terms in T_1 for the sociological terms in O_2 . We first consider the implications of these requirements in the general case of psychological reductionism, and then illustrate them with a simple example.

The first requirement, existence of two specifiable bodies of theory and two specifiable sets of observation statements could prove an immediate block to reduction. For either field, sociology or psychology, one might ask "What theory?" Neither field meets the requirement of actually having a theory, in the sense of an explicit body of propositions possessing a high degree of logical consistency and empirical confirmation. While there may be some agreement among behavioral psychologists as to the explanatory propositions of their field—and even this is clearly lacking among sociologists—a strict construal of the requirements of reduction would mean that the two fields would need to reflect the degree of axiomatization which Peano performed on the theory of numbers.

But since this criterion cannot be met with psychology and sociology (indeed it would be difficult to assert it for any science except certain branches of atomic and mechanical physics), we may relax it a bit and ask whether even with a generously loose interpretation of the terms T_1 and T_2

it is reasonable to expect that a reduction could be performed.

A well developed theory, in addition to logical consistency, demonstrates a high degree of *closure* and *completeness*. Closure of a theory means that, within its intended scope of application, all relevant variables are specified. The usual example of closure is Newton's $F = MA$; force equals mass times acceleration. It is a closed system in the sense that all variables necessary to determine force are specified; appropriate measures of any two of the variables enables perfect prediction of an appropriate measure of the third. No variable which is not contained in the theory affects any variable which is within the theory. Thus for a completely closed system, it is not necessary to add the cautionary words "all other things being equal," for these imply that something not within the theory (unknown, or at least unspecified) *does* affect the values of theoretical variables.

Completeness means that the precise relations between the variables of the theory are known and specified. The only errors of prediction made by a complete theory are those due to faulty measurements—measurement error, or poor operationalizations. There are no areas where predictions are indeterminate; that is, where the theory is unclear precisely what prediction should be made. Also, there are no areas of *anomaly*; replicated findings which are not exactly what the theory says they should be.

It is obvious that neither sociological nor psychological theory at this time can meet stringent requirements of either completeness or closure, a fact partly recognized by sociologists when we add the famous *ceteris paribus* clause to our propositions. Just how weak the theories of both fields are, however, is not immediately apparent without examining an instance of theoretical explanation. Taking any available example, we may ask whether a proposed explanation will account satisfactorily for a finding by a loose requirement of completeness and closure.

A commonly used measure reflecting these criteria is variance in some finding which may be accounted for by a proposed explanation; for example, how much of the variance in some social behavior of interest may be

accounted for by a proposition such as "The greater the reward, the more likely the activity which elicits the reward?" In particular, as a minimal criterion of closure and completeness, we may ask whether the variance accounted for by the proposition is greater than half the total variance described in the observation. Assuming no measurement error, can the proposed theoretical principle explain even half the observed variance in o_2 ? Of interest also is whether some psychological principle is more successful at explanation by this criterion than some sociological principle. If the principle asserts an association of two concepts (such as reward value and frequency of activity), because explained variance is equal to the square of measures of association, we need an association within the principle of .7 or higher to explain half the observed variance in this manner.

Most psychological and sociological principles are stated vaguely enough that we cannot determine exactly the strength of association claimed, but there are two reasons to believe that in most cases it probably is not very high. First, the implicit *ceteris paribus* clause in these propositions indicates lack of closure; we know that "other things" are never equal. Second, the probabilistic form of nearly all propositions indicates lack of completeness. In order to explain half the variance, the level of the relationship in the entire proposition must be .7 or higher, including both of these sources of indeterminacy. Being unable to explain more than half the variance does not by itself indicate inadequacy of explanation, but it does indicate less than perfect completeness and closure of the particular explanation being proposed. And as we shall see later, even theoretical propositions which assert an overall probability of .7 or higher will not permit explanation in any but the most simple case where only one theoretical principle is used.

The criterion of completeness asks how precisely accurate is a proposed theoretical explanation, a question better applied to predictions than to *ad hoc* explanations. For the overwhelming majority of sociological and psychological theories, only ordinal predictions of cases are possible, in-

dicating a rather low level of completeness in both sets of theories. Cases where numerical predictions are made (such as those from mathematical models) sometimes do provide reasonably good fit to observations; but the conditions under which these are possible are severely limited, and thus indicate major areas of indeterminacy in the theories.

In addition, use of stochastic (probabilistic) theories increases the difficulty of assessing the completeness of any theory. If the proposition t_1 states that "Individuals will *tend to* maximize perceived profit," it is impossible to falsify the proposition with any finite set of observation statements.¹³

Finally, any combination rules or "emergent" properties, to be discussed in a separate section below, would further increase the difficulty of constructing psychological explanations for sociological findings.

The other requirement for reduction is that we find some acceptable way of interpreting the concepts in sociological observation statements O_2 so that psychological propositions T_1 can explain them. Typically, the terms used in sociology are not the sort which can be completely defined by reference to a single individual; for example, *status structure* of a small group cannot be defined without several members of the group, *power* usually refers to some observable or potentially observable feature of interaction between two or more individuals, *interaction* refers to things undefinable when only one individual is present. In order to demonstrate a psychological reduction, some way must be found to interpret sociological terms so that they can be stated in psychological terms. In other words, a satisfactory interpretation must be found so that terms like *status structure* can be defined in terms of properties of various individuals.¹⁴

¹³ Nor is this problem alleviated by specifying a probability: "Individuals will, with $p = .70$, maximize perceived profit." We might always be sampling the "null set," where individuals *do not* maximize.

¹⁴ This is not to argue that such definition is impossible, a point taken up in the following section. All that is asserted is that the psychological reduction of sociology requires that terms commonly used in sociological observation sentences must be

Specifying a psychological interpretation for sociological terms requires what are usually called rules of correspondence; these tell, most simply, what psychological term, or combination of terms, corresponds to each sociological term. They are the rules of translation of the special vocabulary of one field to that of another, similar in most ways to the rules which govern translation of Latin words to English.

Since we do not have a book of these rules, we may ask at least what they would look like, so that we can decide whether such a book could reasonably be compiled. The sociological terms are given in the set of observation sentences, so we need not worry about them; the question is what the relevant psychological terms will look like. Clearly they will be terms predicated on individuals; that is, terms which are used in propositions relating to individuals. But this is not sufficient unless we accept *any* term related to individuals as psychological, a position already rejected.

In the ideal case, the terms would be prominent in the set of explicit, accepted psychological theories; that is, the set of explicit propositions which all or most practicing psychologists accept. Even assuming that psychologists have some shared agreements on theoretical propositions, clearly psychology does not have a set of universally accepted, explicit axioms.

So in the imperfect case, it is reasonable to rely on some consensus among psychologists as to what constitutes the concepts of the field: we would accept as a psychological term any term which appears occasionally in the psychological literature. We need psychological propositions which could be used to derive the sociological observation sentences about status structures, power, and interaction. Perhaps a psychological interpretation of every sociological concept can be compiled. Until the attempt actually is made we cannot finally evaluate the claim that success is possible, but the effort required just on the rules of correspondence would be considerable.

The use of theoretical constructs (such as "diffuse status characteristic") or disposi-

tion terms (such as "racial prejudice") increases further the difficulty of establishing acceptable rules of correspondence between them and psychological concepts. Theoretical constructs are defined only implicitly by the set of propositions in which they exist—that is, we specify the effects of diffuse status characteristics, and how the concept relates to other theoretical concepts, but never explicitly define the term. Disposition concepts are defined only incompletely in situations of interest—for example, we say a person is racially prejudiced *if* when he meets a minority group member he is rude; *if* he refuses to sit next to a minority group member; *if* he would not want his sister to marry one, etc.

Thus the nature of both theoretical constructs and disposition terms is that no finite set of measurements will suffice to define them fully; only partial definition is possible. Explaining sociological observations containing a theoretical construct or a disposition concept completely by psychological propositions would require specifying a rule of correspondence between the relevant psychological term and every *possible* sociological observation sentence containing the construct. Since to do so is impossible, we would probably be satisfied if a rule of correspondence could be produced for every situation "of interest," but even this criterion is difficult to attain since we may find new situations of interest in the future. Such a change may not make the reduction impossible, but it does introduce added uncertainty about whether any proposed set of correspondence rules will continue to be satisfactory.¹⁵

¹⁵ Requiring that all abstract terms first be eliminated from the sociological observation sentences O_x would make it possible to recognize a satisfactory set of correspondence rules between the observations and a proposed psychological explanation. But this requirement would have two other undesirable consequences: it would greatly increase the total number of sociological observations, and it would probably mean having to eliminate some observations of potential importance or interest from the body of sociological facts O_x . It seems to be a characteristic of conceptual maturity in empirical sciences to develop and use theoretical constructs, and return to strictly operationalized observations in sociology would represent a backward step in our understanding of social phenomena.

able to be stated in psychological terms. Otherwise sociological observations cannot be explained by psychological principles.

Assuming we had explained a sociological observation with propositions predicated on individuals, and that we also had a complete set of rules of correspondence for every sociological concept used in the explanation, then (assuming the concepts were actually used by some psychologists), we would have performed a psychological reduction—of that one sociological finding. This of course would not demonstrate a general reduction of sociology, nor even the possibility of such a general reduction. But demonstrating rules of correspondence between sociological and psychological concepts is a major requirement distinguishing a psychological reductionist position from the general strategy of methodological individualism. In the latter, one assumes as an article of faith that group properties can be explained on the individual level; in the former, one asserts that this can actually be (or has been) done, and that the explanation in fact constitutes part of the theories of psychology.

As the illustration below will indicate, because of the lack of completeness and closure, the set of explanatory propositions for any given phenomenon will have to be considerably more complex and more precise than may appear necessary just to explain that phenomenon. Some of the explanations, therefore, may well be complex enough to make the effort unappealing.

An Example of Explanation

The preceding general points may be illustrated by considering a simple hypo-

thetical attempt to demonstrate a psychological explanation of a sociological finding. Questions of whether the explanation is empirically or logically "true" are not central to this illustration, and will be omitted.

Let us begin by accepting a sociological finding o_2 , and offering a sociological proposition t_2 for explanation. Then we will propose individually oriented propositions in the attempt to produce a reduction.

o_2 : Group A took more time to solve a set of problems than group B.

This may be explained by derivation from the conjunction of two sentences, a sociological proposition t_2 , and an antecedent condition AC1 which asserts that the condition of the proposition is met. These are presented in the second and third lines of Chart 1. A complete explanation would require a rule of correspondence asserting that "electing leaders" is an operationalization of "developing internal stratification," and that "efficient" means "took less time"; but these are simple, and we omit them. We also omit explicit statement of rules of correspondence for translating sociological concepts into psychological concepts, but they involve relatively small inferences and should be clear from the statements.

We shall attempt a reduction by showing how the sociological finding o_2 may be derived from a psychological proposition t_1 . The explanation constructed will use the proposition t_2 as one step in the deductive chain; thus, it will constitute a theoretical

Chart 1

- o_2 : Group A took more time to solve a set of problems than Group B.
- AC1: Group B elected leaders and developed internal stratification: Group A did not.
- t_2 : The greater the stratification of a problem-solving group, the greater its efficiency.
- t_1 : The greater the task orientation of group members, the greater the stratification of the group.
- t_1' : The greater the perceived group reward for successful problem solution, the greater the task orientation of the members.
- AC2: The reward for successful problem solution offered the members of Group B was greater than the reward offered the members of Group A.
- AC3: High task orientation is known to be instrumental to successful problem solution by the members of both groups.
- t_1 : The greater the perceived reward, the more likely the activity related to gaining the reward.

reduction as well as an empirical reduction. What is of interest is explaining the finding, not the proposition, though for purposes of illustration it is convenient to perform both types of reduction simultaneously in this case. Thus we accept that Group B's relative success is related to its stratification, and will attempt to produce an explanation in psychological terms of the group concept "stratification." It should be noted that a successful demonstration of this reduction would not constitute a general reduction of sociology to psychology; it would merely show that *one* observation o_2 and *one* proposition t_2 have been reduced.

Thus, though the illustration is more than the minimum necessary to demonstrate reduction (since it is a theoretical reduction as well as an empirical one), it is far from sufficient for demonstrating the general reduction of sociology. It is also inadequate for demonstrating the *possibility* of general reduction of sociology.

An additional proposition, t'_2 , gets the first half of t_2 to the individual level.

We have not defined "task orientation" (e.g., "concern with problem solving instead of with being friendly") since it is not central to the demonstration. However t'_2 does attempt to explain the fact of "stratification" of t_2 in individual terms. Now the goal is to explain task orientation in terms and propositions readily recognizable as belonging to psychology.

We add t''_2 and antecedent conditions AC2 and AC3. Finally, we propose a psychological proposition t_1 . With appropriate interpretation of terms such as "activity related to gaining the reward" (e.g., "developing high task-orientation") and "instrumental" (e.g., "increasing the probability of"), a deductive chain could be constructed from the psychological proposition t_1 to the sociological proposition t_2 and the sociological observation o_2 . This is illustrated in the flow chart (Chart 1), with arrows from statements indicating the logical connective "implies." The question now is whether this demonstration constitutes a theoretical reduction (t_2 to t_1 and, more importantly, an empirical reduction (o_2 to t_1).

Most sociologists definitely would not accept this set of sentences as an adequate explanation, on grounds of closure. At best,

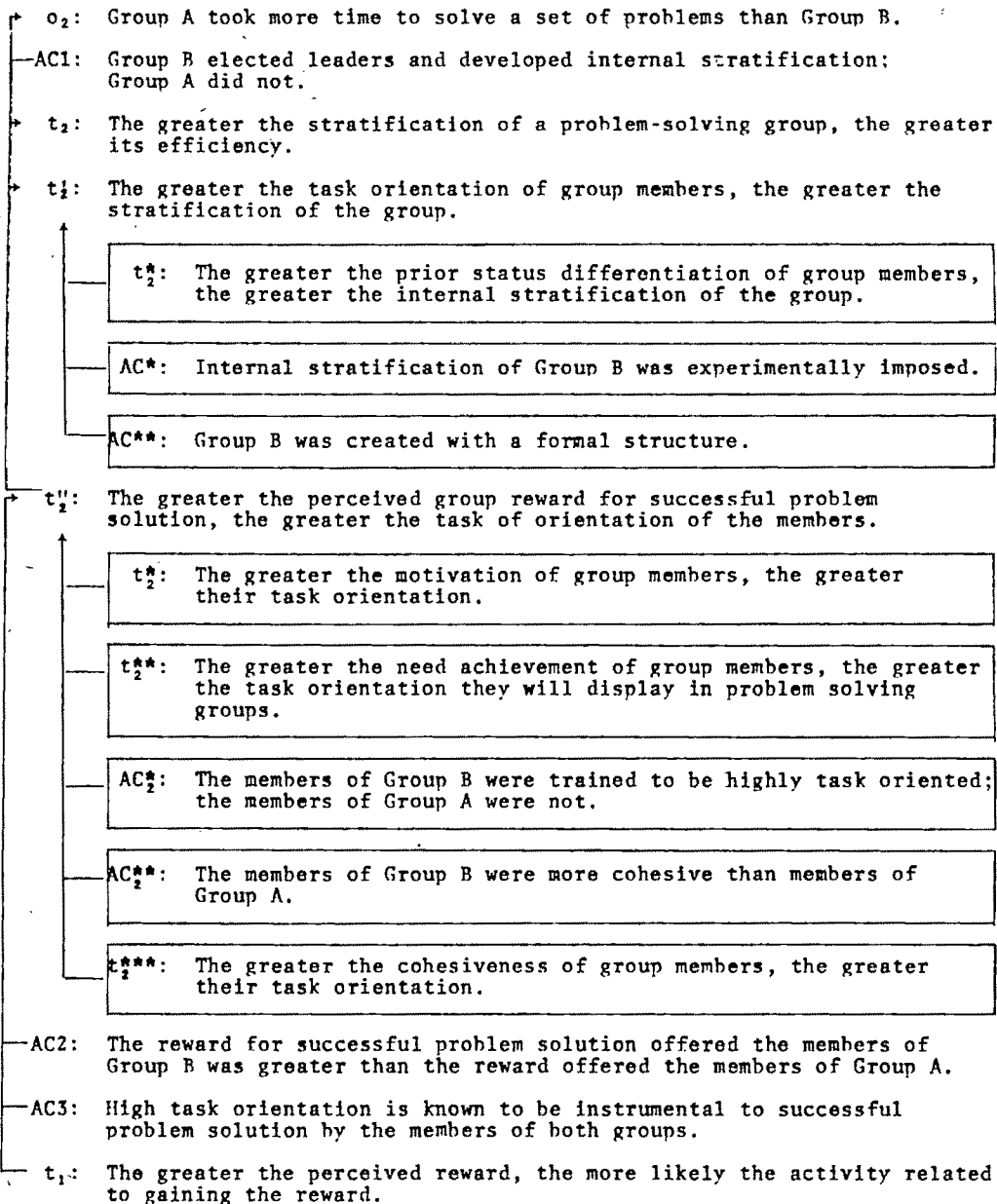
it is a partial explanation, for we have specified only some of the sufficient conditions at each stage of the explanation (represented by the arrows), and none of the stages provides a necessary condition. For example, we may ask whether increasing task-orientation (t'_2) is the only way to increase stratification of the group; of course it is not. Stratification of the group could be increased in a variety of other ways; for example, by diffuse status characteristics of the individuals, by manipulation of the experimental setting, by imposing a formal organization on the group from the start, etc. Similarly, one may ask at the preceding step of the explanation whether increasing the size of the reward is the only way to increase task orientation of the members, and again the answer is obviously no. Such factors as motivational differences, "need achievement," previous training, interpersonal attraction, and individual differences in susceptibility to rewards affect the degree of task orientation of the individuals.

These factors (and anyone could construct a plausible longer list of them) are incorporated in the boxes in the diagram in Chart 2. The point is that all these factors, both those specified in our propositions and those added on *ad hoc* bases, affect variance in the eventual dependent variable degree of stratification.

We have already noted that unless a proposed explanatory proposition asserts an association of about .7 or higher, it cannot be said to explain even half the observed variance. When two propositions must be conjoined to effect the reduction—for example, our t_1 and t''_2 —the explanation becomes much more difficult. Costner and Leik (1964) demonstrate that, except for a closed system (which we do not have) where the propositions assert asymmetric causality (rather than, for example, empirical covariance), very high levels of association are required for even the directionality of variance to be predictable. Thus unless $r \geq .7$ for both propositions (or, for example, .25 for one and .90 for the second), even ordinal predictions of outcome cannot be made.¹⁰

¹⁰ How prediction behaves when the explanatory chain contains more than two propositions is not completely known, but the required levels of as-

Chart 2



sumed association will be at least as high as for cases of only two propositions. Since in general we expect psychological explanations to involve longer deductive chains than sociological explanations—that is, more propositions usually will be required to explain sociological observations from psychological “laws” than from sociological “laws”—this means that the relationship assumed at each stage of the explanation must necessarily be higher if the explanation is psychological.

As our example suggests, frequently in sociology and psychology, it cannot be claimed that at every stage more than half the variance is explained. Thus by even the weakest criterion, neither type of theory permits reduction on grounds of closure. Failure to produce a closed theory means that even the observed *ordering* of cases (Group B > Group A) cannot be explained.

For completeness, we have already noted the major problem: most currently available theories do not permit precise enough predictions even to compare them in terms of completeness. In the typical case, propositions permit the ordering of two conditions of a situation; and this is inadequate to decide which provides the better prediction. Our example o_2 is this sort of ordinal prediction, as are any possible new derivations from the deductive chain. We have not even attempted, for example, to explain the observation, "Group A took 2 hours, 10 minutes, and Group B took 1 hour, 45 minutes." In addition, any probabilistically formulated proposition is impossible to disconfirm empirically, which makes it impossible to know which of several alternative explanations has been confirmed more frequently.

Emergent Properties and Combination Rules

Properties of a phenomenon which were not apparent from knowledge of its components, or properties of an aggregate which were not expected from information about the individuals composing the aggregate are said to be *emergent*. They arise, or become apparent, only at the macro-level or in the collectivity. Thus a study of molecular properties of water would not ordinarily prepare the student for the fact that a glass of water is *transparent*; nothing normally studied on the molecular level would lead one to expect to find that a large number of water molecules transmits light waves. (Nor, in fact, would it lead one to expect that water would be opaque. Molecular theories say nothing about molar properties.) Similarly, if we accept the idea that there exists a "mob mind" or a "national will," or any other such collective representation in sociology, this idea would be emergent with respect to psychology—the usual study of psychology would not say anything about the fact that people in a group of sufficient size are willing to do things (for instance, lynching) which most of them would not condone individually.

Accepting the idea of emergent properties might seem to obviate acceptance of either psychological reductionism or methodological individualism. While it may make the reductionist position more difficult—especially if that position is construed in an

extreme fashion—it can be shown to be compatible with methodological individualism. To begin the analysis of why this is so, note that accepting the idea of emergent properties does not mean that these properties must be considered *really* to exist, in a phenomenological sense. Sociological theorists may use the ideas "national will," "role," "group mean" simply because they are convenient concepts in statements—either observational or propositional—they wish to make regarding a given culture.

It is important to differentiate acceptance of emergent properties from methodological holism, a distinction similar to that drawn between methodological individualism and psychological reductionism. The holist view is that an object or a situation must be studied in all its aspects, and some of the aspects will be aggregate properties; thus, holism does entail acceptance of emergent properties. However, one may accept the idea of emergent or aggregate properties without accepting the idea that all aspects of a situation must be studied. The difference is that the holist would say that emergent properties are in principle undefinable at the individual level. This is an extreme statement, worded more strongly than the definition of emergent properties above, and stronger than would be considered necessary by a sociologist who used emergent concepts. All we said is that the emergent property is unpredictable from ordinary or normal study of the reducing field; an emergent property in sociology, then, would not be predictable from theories of individual behavior ordinarily studied by the psychologist.

But there is no reason in principle why, when psychologists study individual behavior, they could not include some propositions dealing with changes in behavior when the group gets large; for example, "A person, if he is a member of a group of over twenty people, all of them highly emotionally aroused over the same issue, is likely to display a lower level of ego controls than he typically displays in his individual volitional behavior." If the psychologist were interested in how individuals' behavior changes in large groups, he might formulate propositions about individuals which explain the emergent property mob mind. This is a *combination rule*; it tells how units at the

individual level are combined in order to explain the collective phenomenon. Since the hypothetical proposition is predicated on an individual, it is within the province of methodological individualism; since it was formulated by a psychologist it might also fit as part of a psychological reduction. What usually prevents us from saying that a psychological reduction has been performed is that, in actual practice, psychologists seem not to be very much interested in this sort of proposition; consequently, these propositions do not often appear among the (loosely construed) set which constitute psychological propositions, or psychological knowledge. In any case, instances are infrequent enough in psychology that only rarely does there exist any potential reduction of emergent sociological concepts.

On the other hand, one of the tasks of the methodological individualist is to show how emergent group properties may be explained as consequences of individual behavior. The task is to formulate appropriate combination rules so that group phenomena can be explained at the individual level.

An example from social psychology can help to describe the nature and function of combination rules. Within the past few years, several investigators have studied the 'risky shift' phenomenon: individuals in groups tend to accept worse odds at several hypothetical decision situations than the same (or similar) individuals say they would accept at the same (or similar) situations when alone. The riskier group decision thus is an emergent property, not apparent from what was known at the individual level; one recent review article (Dion, *et al.*, 1970:311) has thus concluded that it may "represent a *true* group effect" (emphasis in original).

To explain the risky shift, what would be required are propositions about individual behavior (such as those which could explain the decisions made by individuals alone), along with some combination rules which specify differences to be expected when the individual finds himself making a collective decision in a group. For example, it may be the case that individuals perceive "diffusion of responsibility" in group situations; thus, a combination rule asserting this would be combined with other propositions which predict decision making as a function of re-

wards and degree of responsibility (e.g., Camilleri and Berger, 1967). Another possible explanation is that it is those individuals who make the most risky choices who become influential leaders in groups; thus, others are influenced by these naturally risk-prone individuals in the group situation. In this case, the combination rules would have to explain why risk-prone individuals are likely to become leaders, and then to specify the influence process by which others are induced to accept more risk in their group decisions. (For a review and assessment of several current competing explanations, see Cartwright, 1971, and Pruitt, 1971.)

From our perspective, each alternative explanation constitutes a set of combination rules, which, when explicitly stated and empirically supported, would enable prediction of the emergent property from what is known about individual behavior. Two facts may be noted. First, though at present no single set of combination rules for explaining this phenomenon has received definitive support, at least the direction in which the explanation will be constructed appears clear. The group property will be explained using propositions about individual behavior, combined with appropriate combination rules. The individual behavior propositions are not being seriously questioned nor is the strategy of methodological individualism; what is presently problematic is deciding just which set of combination rules is best. Second, any observation statement o_2 describing the emergent property is clearly sociological, not psychological: the concepts in the sentence are group properties. This means that at least one of the statements in the set of combination rules will have to contain a sociological concept also. In order to perform an empirical reduction of o_2 , it will be necessary to specify rules of correspondence which enable derivation of both o_2 and the rules of correspondence used to explain o_2 from psychological propositions. As this example illustrates, many sociological observations make use of group concepts, and any proposed reduction of them must include not only rules of correspondence for the group concepts, but also for whatever combination rules have been adduced to enable individualistic explanation. In principle it

may be possible to construct such a set (bearing in mind the restriction that it probably would contain some concepts not particularly interesting to most psychologists), but it clearly would have to be more extensive than may be apparent to many reductionists.

For larger scale phenomena, the field to be reduced may contain observation sentences and properties which require enormous effort to explain using either psychological or even individually-predicated propositions. The economist Olson (1971) has shown that the behavior of individuals regarding collective goods (such things as roads, hospitals, sewer systems) differs markedly by size of group. In small groups, especially when characterized by great disparity in wealth among members, collective goods are likely to be provided by individual, voluntary action. In large groups, especially where relative differences in wealth are less noticeable, provision of collective goods will not occur unless coerced by some means such as taxation.

It might appear that the change in willingness of people in large groups to pursue their own welfare is thus an emergent property, one which becomes apparent not in small groups, but only in large ones. This aggregate property of unwillingness to provide collective goods is emergent with respect to the economic assumption of individual maximization of benefits.

However what is most interesting about Olson's analysis is that he demonstrates that the fact is a logical consequence of the assumption that individuals seek to maximize their individual benefit, and that the popular earlier belief (which was accepted, for example, by Aristotle, Laski, and Marx; Olson, 1971:1,6) that groups seek collective benefits for their members is *not* a logical consequence of the assumption of individual maximization. The reason, he argues, is that the implicit combination rules had earlier been misunderstood.

Olson's analysis, conducted entirely within the framework of methodological individualism, indicates that the apparently anomalous fact of the collective in actuality is a logical consequence when the antecedent conditions describing the nature of large groups are adequately specified. The explana-

tion involves some propositions and observations constructed entirely for the purpose of explaining features of the group. Statements which have nothing to do with individuals or individual behavior are introduced. It is, therefore, a theory of groups, not a theory of individual behavior in groups. Performing a psychological reduction on such an argument would be an exceedingly difficult task, and even the outlines of such a reduction are not clear at this time.

An important feature illustrated by these examples is that emergence is related to the current state of knowledge in a field. When sociological concepts are emergent, it is because no generally recognized individual propositions are adequate to explain them—now. In the future, these propositions may be available. In any individual instance it may be extremely difficult to construct an individual explanation for a collective phenomenon. Imagine, for instance, the large number of antecedent conditions (combination rules) about molecular sizes, weights, and positions; and the propositions about the nature of light and polarization which are required to explain water's transparency on the basis of knowledge of molecules. In principle, a good physical chemist probably could do it, but in practice, he would probably not consider it worth the time it would require.

With the state of knowledge in less well developed fields, such explanation often is currently impossible. To ask a biologist to explain the emergent (psychological) property *cognitive processes* on the basis of his knowledge of brain cells is currently to ask more than he can do. This is often the case: the effort required would be enormous, the payoff would be small, and in any case, practitioners in the potential reducing science have little interest in the phenomenon.¹⁷ The central point is that when a property is

¹⁷ Explaining emergent properties has sometimes been crucial in determining the acceptance of a proposed reduction. The reduction of thermodynamics to statistical mechanics was generally considered successful only after observations previously explained by propositions about "entropy" (unavailable energy in a closed thermal system) were shown to be derivable from probabilistic assumptions about the behavior of molecules (Nagel, 1961:343). Note that this is an instance of what is here called empirical reduction.

called emergent, the expression is elliptical; what is meant is "emergent with respect to the current state of knowledge."¹⁸

Conclusion

The availability of psychological explanations for social phenomena is undemonstrated; any claim that a general reduction exists is absurd. The current state of empirical knowledge and propositional theory building in both sociology and psychology are clearly inadequate; moreover, they do not give any indication that the reduction will ever be accomplished. Methodological individualism, which is not so clearly a specific viewpoint as an article of faith which may be adopted in theory construction—the faith that any phenomenon of interest can be explained by propositions about individuals—seems to be gaining adherents among sociological theorists. However, articles of faith, or meta-theoretical assumptions which must be adopted before any theory can be constructed, are not in themselves subject to verification or falsification. What we ask is how good are the theories themselves, and only very indirectly whether the meta-theory is a good one.

REFERENCES

- Berger, J.
1969 "Elements of a sociological self-image." Pp. 177-92 in I. L. Horowitz (ed.), *Sociological Self-Images: A Collective Portrait*, Beverly Hills, California: Sage Publications.
- Blain, R.
1971 "On Homans' psychological reductionism." *Sociological Inquiry* 41 (Winter):3-25.
- Borger, R. and F. Cioffi
1970 *Explanation in the Behavioural Sciences*. Cambridge: Cambridge University Press.
- Brodbeck, M.
1968 "Methodological individualisms: definition and reduction." Pp. 280-303 in *Readings in the Philosophy of the Social Sciences*. New York: The Macmillan Co.
- Burgess, R. and D. Bushell
1969 *Behavioral Sociology: The Experimental Analysis of Social Process*. New York: Columbia University Press.
- Camilleri, S. F. and J. Berger
1967 "Decision-making and social influence: a model and an experimental test." *Sociometry* 30 (December):365-78.
- Cartwright, D.
1971 "Risk taking by individuals and groups: an assessment of research employing choice dilemmas." *Journal of Personality and Social Psychology* 20 (December):361-78.
- Costner, H. and R. Leik
1964 "Deductions from axiomatic theory." *American Sociological Review* 29 (December):819-35.
- Dion, K., R. Baron and N. Miller
1970 "Why do groups make riskier decisions than individuals?" Pp. 306-77 in L. Berkowitz (ed.), *Advances in Experimental Social Psychology*, Vol. 5. New York: Academic Press.
- Heider, F.
1944 "Social perception and phenomenal causality." *Psychological Review* 51 (November):358-74.
- Homans, G.
1958 "Social behavior as exchange." *American Journal of Sociology* 62 (May):597-606.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace and World, Inc.
1964 "Bringing men back in." *American Sociological Review* 29 (December): 808-18.
1967 *The Nature of Social Science*. New York: Harcourt, Brace and World, Inc.
1971 "Reply to Blain." *Sociological Inquiry* 41 (Winter):19-24.
- Kemeny, J. and P. Oppenheim
1970 "On reduction." Pp. 307-18 in B. Brody (ed.), *Readings in the Philosophy of Science*, Englewood Cliffs: Prentice-Hall, Inc.
- Kuhn, T.
1970 *The Structure of Scientific Revolutions* (second edition). Chicago: The University of Chicago Press.
- Lachenmeyer, C.
1970 "Reduction in sociology: a pseudo-problem." *Pacific Sociological Review* 13 (fall): 211-17.
- Mandelbaum, M.
1957 "Societal facts." *British Journal of Sociology* 6 (December):305-17.
1957 "Societal laws." *British Journal of the Philosophy of Science* 8 (November):211-24.
- Maris, R.
1970 "The logical status of Homans' social theory." *American Sociological Review* 35 (December):1069-81.
1971 "Notes and comments." *American Sociological Review* 36 (August):706-15.
- Nagel, E.
1949 "The meaning of reduction in the natural sciences." In R. C. Stauffer (ed.), *Science*

¹⁸ The existence of some observation statements of interest to psychologists (e.g., regarding the "risky shift," learning problems, or effects of status on interaction) which are currently unexplained, raises an intriguing possibility: perhaps sociologists, especially those methodological individualists among us, will be able to produce adequate explanations for these psychological findings using sociological propositions. This would, of course, constitute a partial reduction of psychology to sociology.

- and Civilization, Madison: University of Wisconsin Press.
- 1961 The Structure of Science. New York: Harcourt, Brace and World, Inc.
- Newcomb, T.
1953 "An approach to the study of communicative acts." *Psychological Review* 60 (November): 293-404.
- Olson, M.
1971 The Logic of Collective Action (revised edition). New York: Schocken Books.
- Popper, K.
1957 The Poverty of Historicism. New York: Harper & Row, Publishers.
- Pruitt, D.
1971 "Toward an understanding of choice shifts in group discussion." *Journal of Personality and Social Psychology* 20 (December): 495-510.
- Russell, B. and A. N. Whitehead
1925 Principia Mathematica (second edition). Cambridge: Cambridge University Press.

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Fellows and Active and Student members of the Association may submit manuscripts of 100 to 300 typed pages for publication in the *ASA Arnold and Caroline Rose Monograph Series in Sociology* to the Series Editor, Sheldon Stryker, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

SYMBOLIC INTERACTION AS A PRAGMATIC PERSPECTIVE: THE BIAS OF EMERGENT THEORY

JOAN HUBER *

University of Illinois, Urbana-Champaign

American Sociological Review 1973, Vol. 38 (April):274-284

The epistemology of symbolic interaction derives from the pragmatic model of Dewey and Mead, close associates. This paper argues that the methodology of pragmatism and symbolic interaction permits the perspectives of the researcher and the people in interactive situations to bias the research. Influenced by Hegel, Dewey and Mead adopted an evolutionary, holistic view of reality; progress was inevitable and people were naturally rational. In addition, their notion of the logico-theoretic component in science deviated sharply from the more common hypothetico-deductive model. In both the pragmatic and symbolic interaction approaches, the theoretic component is ambiguous; to formulate theory prior to research is thought to be risky for it may bias the research. Instead, theory emerges from the research process; the participants in an interactive situation contribute to it. Thus the social givens of the researcher and the participants serve as a theoretical framework, giving the research a bias which reflects the social perspective of the researcher and the distribution of power in the interactive setting. That many researchers in this tradition are liberal humanitarians obscures the problem of bias inherent in this approach.

As a socially-based approach to the relation of the individual and society, symbolic interaction (SI) has always been an important perspective in American sociology. Kuhn (1970:83) suggested that, unlike psychoanalysis, field theory, and learning theory, SI is logically consistent with basic social science propositions. As a research tradition, SI has produced insightful accounts of human interaction in natural settings. Yet even its adherents are doubtful about its methodology and the status of its findings. Blumer (1969:1) pointed out that the position of SI had never been clearly formulated and no reasoned statement of the methodological approach existed.

The SI tradition is related to a number of other approaches and techniques such as labelling theory, sociological phenomenology and existentialism, participant observation, qualitative sociology, and naturalism. No attempt to deal with the differences and similarities of these approaches to the SI tradition will be made in this paper, which is concerned, rather, with the overall his-

torical drift. Yet their relationship to SI is important because even if, as Ehrlich (1972) asserts, what was scientifically most useful in the SI approach has already been absorbed into the mainstream of social psychology, many of the problems of SI methods still plague these other approaches.¹

Events in the last decade have thrown the problems of SI methods into even greater relief. Many young sociologists are unhappy with the direction of the discipline, particularly with what they see as an overemphasis on quantification. They feel that social science has failed to come to grips with the real world (Blumer, 1966: vii).² Such concern has led to increasing criticism of standard methodology; the procedures which methodologists found so intriguing have not had the desired impact on research because the methodologists failed to communicate the substantive rele-

¹ For example, Becker (1958:653) points out that the observational researcher faces the problem of convincing others of the validity of his findings; Bruyn (1966:174) says that there is currently no method to do this. Lofland (1971:vii) notes that, strangely, few instructions are available to show how qualitative observation and analysis is done.

² Filstead (1970:1, 8) cites Blumer, Clinard, Bruyn, Deutscher, Becker, Gouldner, and Horowitz as showing a concern with the current direction of the field.

* An earlier draft of this paper was presented at the ASA meetings, August 1972. I am deeply grateful to J. David Lewis, Clark McPhail, and an anonymous reader, but especially to William Form for their comments on one or more successive drafts. All errors are my own.

vance of the tools they admired so much (Hill, 1970:18, 19). Apparently the SI tradition and similar approaches answer a need in the discipline; and, whatever their inadequacies, they will probably be around for some time.

The main thesis of this paper is that the SI tradition shares with the philosophy of pragmatism,³ from which it originates, an epistemology which makes it reflect the social biases of the researcher and of the people whose behavior is observed. In a benignly liberal climate of opinion this outcome tends to go unnoticed; but in the long run, this kind of methodology is sensitive to the forces of social control. So far as I have been able to discover, SI methods have not been criticized from this point of view. The explication of this thesis begins by showing the relationship of SI and pragmatism.

The progenitors of the SI tradition include Dewey, Cooley, Baldwin, and Znaniecki, among others; but the chief architect was George Herbert Mead (Manis and Meltzer, 1967:1; Kuhn, 1970:71). Most of the published materials on which knowledge of his position is based were not originally intended for publication (Meltzer, 1959:27). His books, based on student notes, were published posthumously (Stevens, 1967:553). His articles were scattered in journals and out-of-print books until Reck's (1964:v) selection appeared. Mead was not a systematic writer. He found extemporaneous speaking to be his best medium and felt that men do their best thinking in conversation (Lee, 1945:v). Dewey (1932:xl) observed that at about the time of his death Mead was beginning to get a command of his ideas which made communication to others easier and more effective. SI thus began with an oral tradition which tended to

persist. By the early sixties neither Faris, Thomas, nor Blumer had presented a rounded theoretical conception; hence much time was devoted to casuistical debating over questions of orthodoxy (Kuhn, 1970:71-2).

Mead was one of the leading figures of pragmatism (Shibutani, 1968:83; Gallie, 1966:31). Dewey, its main expositor, became a close friend of Mead's at the University of Michigan and their intellectual exchange continued at the University of Chicago where Mead taught from 1893-1931. Dewey provided the range and vision, Mead, the analytical depth and precision (Morris, 1934:xi). Dewey's daughter reported that the influence of Mead on Dewey, from the nineties on, ranked with that of James (Mills, 1966:296). Yet Dewey is usually ignored as a major influence on American sociology (Petras, 1968:18). Likewise, Mead tends to be ignored by philosophers (Mills, 1966:464). Possibly one reason for their neglect is that Mead's ideas were not readily accessible in his lifetime (Shibutani, 1968:83). However, American sociologists typically take little interest in philosophy and philosophers show little interest in the output of sociologists.⁴

Pragmatism, as Durkheim (1960:386) observed, was a reaction to the ideas of traditional rationalism.⁵ Dewey and Mead, like Marx before them, thought that traditional philosophy was arid, formal, and useless. Moreover, the dominant social view held that customs derived from a fixed human nature which was, in turn, derived from an immutable god. Such ideas supported a rigid legal system which shored up a privileged social order. To use material and social factors to explain human arrange-

³ The paper refers mainly to Dewey's version of pragmatic philosophy. Peirce's conception is different (Lewis, forthcoming). He thought that other pragmatists misunderstood his theories and his dominant attitude toward them was one of contempt. From Kant's *pragmatisch* Peirce adopted the name pragmatism, which became popular. This so irritated Peirce that he referred to his own formulation as "pragmaticism," a name which he thought was so ugly that no one would use it (Wennerberg, 1962:14-15). In this opinion he was correct.

⁴ Mills' (1966) doctoral dissertation, a rare exception, is a sociological analysis of pragmatism rather than an assessment of the impact of pragmatic philosophy on sociology. Such an analysis is yet to be made. Mills (1966:464) omitted a detailed consideration of Mead's work but felt that the omission was intellectually unwarranted.

⁵ Durkheim lectured on pragmatism at the Sorbonne, 1913-14. He wanted to find a formula that would preserve the essentials of rationalism but at the same time answer the valid criticisms that pragmatism had made. Stone and Farberman (1970:100-12) see Durkheim as more sympathetic to pragmatism than I do.

ments unveiled the conservative bias of traditional ideas. Ironically, pragmatism had a similar bias, although Dewey and Mead and their followers were not aware of it. Nevertheless, all sociologists owe a debt to the pragmatists (and to Marx) for what now appears to be common sense: men make their own social world.

But pragmatism went far beyond this general assumption. According to Kaplan (1964:36, 42), pragmatism is a variant of semantic empiricism which, in turn, was a development of epistemic empiricism. From Locke through Kant, epistemic empiricism was the doctrine which held that experience was a necessary condition of knowledge. Semantic empiricism, developed in the last hundred years, holds that not only knowledge but also meaning must necessarily include an experiential component. Two of the three major variants of semantic empiricism, logical positivism and operationism, ask the same question of any scientific assertion: Can its meaning be established and, if so, how? That is, can sense data be used for verification and, if so, what kind of rules govern such use? Pragmatism asks what difference it would make if a statement were true. The meaning of objects is the effect they produce (Dewey, 1916:309). That is, whether a belief is good or bad depends upon whether the activities which it inspires in the organism entertaining the belief have consequences which are satisfactory or unsatisfactory to it (Russell, 1945:825). What counts is not the origin of a proposition but its outcome, not the connections with experience antecedently given but with those to be instituted. Truth is thus dependent on human action. Those who feel that human beings are not always rational see pragmatism as a step on the road to madness, an intoxication with power (Russell, 1945:828) or as an all-out assault on human reason (Durkheim, 1960:363). SI shares this stress on the outcome of human action as a criterion of scientific truth.

Why should the stress on the outcome of an event as a criterion of truth render knowledge susceptible to social control? Briefly, because the future, unlike the past, is subject to manipulation by those who currently have power. "The past cannot be

affected by what we do, and therefore, if truth is determined by what has happened, it is independent of present or future volitions; it represents, in logical form, the limitations on human power. But if truth, or rather "warranted assertibility," depends upon the future, then, in so far as it is in our power to alter the future, it is in our power to alter what should be asserted" (Russell, 1945:826).

In order to clarify the line of argument, I shall outline it here. In the SI approach, as in the pragmatic, formal logic has an ambiguous status. The place of the rational (logico-theoretic) component in validation is never spelled out clearly. When the place of theory is unclear, when the theoretical expectations are not explicated, then the social givens of the present serve as an implicit theoretical formulation. In pragmatic doctrine, scientific truth is defined as whatever works best in a given situation, as judged by the investigator who observes the emergent outcome. A belief is judged by its effects; if the effects are good, then the belief is true, or has warranted assertibility. Later formulations held that truth is the emerging consensus of the participants in an interactive situation. All of these formulations have a status quo bias for, when no theoretical expectations are specified, and when truth is expected to emerge from interaction, then what is taken to be true tends to reflect the distribution of social power among the participants.⁶ This assertion is supported by theory and research in group ranking and conformity (Zajonc, 1968:253-60). Dewey and Mead avoided confronting the power implications of the pragmatic model because they thought that the world was evolving from worse to better. Therefore, whatever worked was bound to be right, at least in the long run. However,

⁶ The claim that SI methods have a status quo bias does not imply that scholars in this tradition are more conservative than other sociologists. I am arguing only that the model is especially susceptible to influence. Whether scholars in this tradition have a distinctive political viewpoint is a question I have not examined. I have no reason to suppose that they are anything but humanitarian liberals. Dewey was a kindly and admirable man. But this fact has no bearing whatsoever on the susceptibility of these methods to social bias.

the views of the latterday adherents of SI and related approaches are not explicitly evolutionary, nor is the implication of defining truth as an emergent social consensus of participants systematically confronted. I shall now discuss the main threads of this argument in detail, first, the relevant views of Dewey and Mead.

THE PRAGMATISM OF DEWEY AND MEAD

An aspect of pragmatism and SI basic to their sensitivity to social control is the ambiguous status of formal logic (Huber and Loomis, 1970). Philosophers of science distinguish between the logically necessary and the logically contingent, i.e., between mathematics-theory-logic and empirical observations; both are necessary for scientific knowledge (Braithwaite, 1963). A common sense translation of this statement says that facts do not speak for themselves. But the status of the logico-theoretic component is not clear in SI and pragmatism. The difficulty derives from the influence of Hegel's dialectic.

Hegel claimed that the dialectic was a new logic, but this claim is false if one uses the usual definition of the word 'logic' (Mills, 1962:130). Hegel wanted to know if history had any meaning and posited the dialectic as a formal device to enable him to explain social change. In dialectical form, knowledge moves in stages from thesis, to antithesis, to synthesis; history obligingly repeats these stages empirically. "Process" and "emergent" are key words. To understand the result, one must understand the entire process because each stage contains the earlier stages in solution, so to speak; all have their place in the final whole. Because only the Whole is Reality, nothing partial can be quite true. Hence truth and falsehood are not sharply defined opposites. In Aristotelian logic, an entity can be defined as A or non-A. That is, entities can be analytically and empirically distinguished from one another. In the dialectic, entities simply merge into one another. Reality is one great Whole. One ought not study the eye of John Jones for Jones is an organic whole and to study his eye alone is pointless. Described in this fashion, the dialectic may strike some observers as nonsense.

Most of the writers who use the word today leave it undefined, which is probably just as well.

In his early days Dewey was a Hegelian (Mead, 1936:151) and Hegel remained the chief source of Dewey's logic (Mills, 1966:357). The contradictions between thesis and antithesis became conflicting elements in a problematic situation (White, 1943:152). Dewey confused logic and empiricism; logic was thought to be both empirical and normative (Dewey, 1920:137), and ultimately derived from the acts performed (Dewey, 1929:163). But Dewey's view of what was good differed from that of William James in a way that was important for sociology. James had equated truth with what was good for the individual. Dewey saw the difficulties with this idea and declared that truth was public. A belief was to be judged by the consequences it had for many persons, not for just one person. This aspect of pragmatism may be what led Stone and Farberman (1970:15) to comment that pragmatism carried social psychology away from the psychologistic fallacy. From the standpoint of the present critique, it matters little whether the warranted assertibility of a proposition is based on the way it works out for one person or many.

Unlike Dewey, Mead was well aware of contemporary developments in symbolic logic (1943:202; 1936:Ch. 15; 1964:199 ff). Although he once claimed that the theory of the intelligent act fell within the realm of Hegel's logic (1964:8), and passages in his later work still indicate a strong dialectical aroma (1964:189), he finally concluded that the dialectic was a scientifically useless device that could be used to prove anything (1936:143). Yet his thought shows the influence of Hegel in a number of ways. His idea of sociality is basically holistic:

The principle of sociality is that in the present within which emergent change takes place, the emergent object belongs to different systems in its passage from the old to the new because of its systematic relationship with other structures, and possesses the characters it has because of its membership in these different systems (Mead, 1932:65).

With this concept Mead attempts to avoid

some of the difficulties of Aristotelian logic. Perhaps a clearer statement of the concept is the assertion that sociality is the capacity of being several things at once. "The animal traverses the ground in pursuit of his prey and is at once a part of the system of distribution of energies which makes his locomotion possible and a part of the jungle system which is a part of the life system on the surface of the inanimate globe" (Mead, 1932:49).

In addition to glossing over the distinctions between entities which characterize Aristotelian logic, Mead tended to ignore the logico-theoretic component in his notion of scientific methodology. His prescriptions for the practice of science sound very like the methods that Willer (1970:19) has described as empirical or magical thinking, when events A and B are connected only at the observational level. Mead's example of science indicates that he is discussing empirical thinking:

A child's explanation of the conduct of others and the savage's appeal to magic are uncritical uses of a method which requires only analysis and recognition of the implications of its technique to become scientific (Mead, 1938:91).

The basic difficulty is that Mead fails to make a sufficient distinction between a hypothesis and a theory, with the result that his work fails to describe the theoretical component adequately. Facts define themselves in scientific problems (Mead, 1964:260). Like Dewey, Mead sees science as beginning with an immediate problem, with an exception that conflicts with a law and leads to the appearance in the mind of the scientist of a hypothesis that will solve his problem (Mead, 1936:136). The test of an hypothesis is that the conduct that was going on can be continued. "It is the same sort of test which the animal finds. If it finds itself in a difficult situation and sees escape, it rushes off in that direction and gets away. This is a fair test of what we call a hypothesis" (Mead, 1936:349). An animal and a scientist do the same thing when they face a problem. They select some element in the situation to carry the act through to its completion. "The only test the animal can bring to such a reconstruction of its habits is the ongoing of its activ-

ity. This is the experimental test; can it continue in action? And that is exactly the situation found in science" (Mead, 1936:346). The test of truth is the ability to continue a process which has been inhibited (1936:350; 1964:328). If an hypothesis works, it becomes an accepted theory (Mead, 1936:353). The test of truth is ongoing conduct and truth is synonymous with the solution of a problem (Mead, 1964:328).

His view of the relationship of a scientific law to an hypothesis is much the same:

You are undertaking to set up another law in place of the one which has been overthrown. The new law is tentatively set up as a hypothesis. You test it. When you have tested it, it becomes a working hypothesis. And if others test it and it works, it becomes an accepted theory (Mead, 1936:825).

To illustrate the scientific method, almost all of Mead's examples refer to the natural or physical sciences where consensus on goals is high and the solutions are technological. In a rare discussion of a social problem, Mead (1964:261-2) says that various cult values—which are incommensurable—will prevent a solution; hence the scientist must learn to state, as far as possible, our social customs in terms of their functions. What the scientist is supposed to do when this much is accomplished Mead does not say. Because his works are cast in a phylogenetic frame of reference (Petras, forthcoming) and because, like Dewey, Mead assumes that evolutionary processes will make the world better, the problem is minimized. Indeed, Mead often expressed the view that history was on the side of progress; inevitably the brotherhood of men on earth would emerge (Lee, 1945:75; Shibutani, 1968:87). If this assumption were true, then consensus on goals would trouble neither scientist nor citizen alike.

The scientific method, Mead thought, was only the evolutionary process grown self-conscious; scientific technique is simply doing "consciously" what takes place naturally in the evolution of forms (Mead, 1936:371). As an example, Mead mentions the food problem, by which he means digesting materials that have cellulose coverings. Humans had to work out a means to get rid of the covering, i.e., milling. But

Mead never mentions the problem of distribution. His position tends to be a purely analytical scheme which lacks content (Meltzer, 1959:29).

In addition to assuming that evolution was progressive, Mead and Dewey both assumed that men were naturally rational. In making inquiries, men "naturally" test and improve the operations in the course of what they are doing (Dewey, 1929:124). Mead (1934:379) thought that men were rational, capable of logical thinking, because they were social. Ethical judgments can be universal because the voice of all is the universal voice; that is, everyone who can rationally appreciate the situation will agree. Social reconstruction would presuppose a basis of common social interests, by all of those whose minds bring about the reconstruction:

And the way in which any such social reconstruction is actually effected by the minds of the individuals involved is by a more or less abstract intellectual extension of the boundaries of the given society to which these individuals all belong, and which is undergoing the reconstruction—the extension resulting in a larger social whole in terms of which the social conflicts that necessitate the reconstruction of the given society are harmonized or reconciled, and by reference to which accordingly, these conflicts can be solved or eliminated (Mead, 1934:308–9).

The Hegelian influence appears in this passage. The conflicts are somehow going to dissolve in the larger social whole that is created. Mead simply did not allow for irreconcilable conflict. He felt that every interest involved would be considered, but added that "you cannot lay down in advance fixed rules as to just what should be done" (Mead, 1934:388). Indeed, were all men rational and evolution progressive, rules would not be very important. As Dewey put it, the educative process is all one with the moral process, since the latter is a continuous passage of experience from worse to better (Dewey, 1920:183). Science, reason, and progress were isomorphic and inevitable.

The problems of the pragmatic model would be less obvious in a small community with a high level of education and homogeneity of the values that people often squabble over. If people can agree on the

way things are supposed to work, it is not so difficult for them to agree on whether things really do work out that way. On technological matters the degree of agreement is often high. It is on social matters that fights occur.

LATER FORMULATIONS ⁷

Of the subsequent attempts to provide a methodological basis for SI, Blumer's (1969) is the most sophisticated. But he shows a Meadian ambiguity when he describes the theoretic component. He generally uses words from the vocabulary of the hard-science methodologist, such as "theory" and "concept," but he gives them different meanings. Blumer (1969:24) says that a "prior picture" of the empirical world is an unavoidable prerequisite for its study. The four customary means for empirical validation (proper research design, replication, hypothesis testing, and operational procedures) are claimed to be inadequate because they can give no assurance that premises, problems, data relations, and so forth are empirically valid.⁸ Since these four usual means will not do, Blumer concludes that the only way to get this assurance is to go directly to the empirical social world (Blumer, 1969:35). Blumer does not mean that the investigator should consult census data or surveys but rather that he or she should directly inspect group life. Direct familiarity is necessary, he says, because most sociologists hold their theoretical positions tenaciously; and they gratuitously accept concepts and beliefs as inherently true. These images shape inquiry and become a substitute for direct experience. But Blumer does not explain how scientists can approach reality with blank minds nor does he offer evidence to show that persons who follow the SI tradition hold to their concepts and positions any less tenaciously than those who do not. Nor is his conclusion at

⁷ In recent years the two foremost exponents of the SI point of view have been Herbert Blumer and the late Manford Kuhn (Manis and Meltzer, 1967:vi), and four major varieties of the tradition have been identified (Petras and Meltzer, forthcoming).

⁸ Blumer's criticism of the discipline is extraordinarily persuasive. As a critic, he is without peer.

this point consistent with his earlier statement that a prior picture is necessary in order to study the world.

What the investigator should do, Blumer says, is to conduct an exploratory study, seeking acute, well-informed observers. The investigator should also aim to cast the problem in a theoretic form for analysis. But the usual theoretical procedures used in sociology will not do. What is needed is "inspection," an intensive focused examination of the empirical content of whatever elements are used for analysis, and the same kind of examination of the empirical nature of the relations between such elements (Blumer, 1969:43). The prototype of inspection is represented by the handling of a strange physical object. "We may pick it up, look at it closely, and test it in one way or another. Inspection is not preset, routinized; it is free and flexible, the anti-thesis of inquiry as outlined in current methodology" (Blumer, 1969:44).

The crucial deficiency of social theory, Blumer (1954:5) says, is the ambiguous nature of its concepts.⁹ There are two ways to solve the problem. First, to develop precise procedures that will yield a definitive empirical content, relying on standardized techniques and mathematical categories. This way will not establish genuine concepts related to the natural world. The other way is to accept sociological concepts as sensitizing rather than definitive. This approach is spared the logical problems confronting the first, but it forfeits the achievement of definitive concepts with specific objective bench marks. It depends on faithful reportorial depiction and analytical probing; and it remains in close and continuous relation with the natural social world (Blumer, 1954:9-10).

Thus theory is seen to emerge from direct observation, with little specification of the rules of logic or procedure to be used. The

investigator is urged to use well-informed observers. But if the observers fail to agree among themselves, on what grounds does the investigator choose one view rather than another? Do the observers ultimately shape the theory that emerges? What ensures the objectivity or reliability of the investigator? Many scholars currently involved in SI and related research styles ignore some of these questions and disagree on others. Let us examine some responses to these questions.

The Meadian tradition requires the researcher to maintain both his own and the actor's perspective. Unless he addresses this problem, he cannot warrant his findings on scientific grounds and will be open to the charge that they are no different from those of a lay actor (Cicourel:1964:52). But those who do sociology in this style do not always confront this issue clearly. For example, researchers are advised to cultivate close relationships with those they study because such persons can check on the emerging theory (Denzin, 1971:168). Equal weight cannot be given to informants because their motives for aiding the observer shape the character of their information. But the reader is not told how the researcher knows which informants to drop. Becker (1970) avoids the problem of separating his views from those of the participants by opting for the side of the underdog. Because values are said to be an implicit part of any scientific enterprise, nothing is gained by not frankly taking sides. This solution fails to distinguish the findings of sociologists from the findings of anyone else, and hence is unsatisfactory to those who hold that sociologists can claim special competence. Gouldner (1968:105) notes that the problem of identifying an underdog is ignored, as well as the problem of knowing which dog to side with in a hierarchy of stratified dogs.

The phenomenologists encourage a complete merging of the views of the researcher and the interactive situation, for fear that a scientific hypothesis will create the very reality that the scientist defines in his design. As Bruyn (1966:271, 273) notes, the traditional empiricist sets up preconceived realities which he seeks to verify; the phenomenologist wants to keep his precon-

⁹ Willer and Webster (1970) also conclude that the amount of theory in sociology is small because sociologists conceptualize incorrectly. Their prescription is unlike Blumer's. Rather than using the concepts of everyday experience, observables, or descriptive terms, sociologists should define constructs expressing abstract properties of entities for use in theories.

ceptions to a minimum and avoid anticipating causal relationships. Research interests are to be guided by the subject as given. The ambiguous view of theory which characterized the Meadian tradition is thus made consistent by eliminating the need for theory.

The objectivity of the scientist is also a source of difficulty. Dewey's (1939:775) criterion of truth was a method, to be used by intelligent men who would have a sympathetic regard for persons of differing views. The guarantee of objectivity was the social sensitivity of the observer to the needs of others (Dewey, 1920:147). To judge the credibility of grounded theory, Glaser and Strauss (1967:230) use a criterion based on the feelings of empathy aroused in the observer: if the reader is so caught up in the description that he feels as if he were in the field, he is more likely to be convinced of the accuracy of an account than if the description were flat and unconvincing. The judgment is also based on the assessment of how the researcher came to his conclusions, whom he interviewed, and how he might have appeared to those he studied. Douglas (1970:13 ff) advised that a "review" of the experience taken for granted by the natural, or everyday-life stance, will enable the researcher to understand what is going on. What the review should consist of and how the researcher knows that he has done it properly is not explained.

Summarizing earlier efforts, Denzin (1970:26) concluded that no single method will ever meet the requirements for the validation of interaction theory and therefore recommended multiple methods. The proper strategy cannot be derived solely from principles in research manuals because it is an "emergent" process, contingent on the investigator, the research setting, and his theoretical perspective (Denzin, 1970:310). The formulation appears to be a mixture of standard methods added to a derivative of the research prescription advanced by Dewey and Mead.

DISCUSSION

A major legacy of pragmatism to the Meadian tradition is the ambiguity toward the logico-theoretic component in scientific

research. When the theoretical formulation is primitive, when it "emerges" from the research, or when it is absent, then investigators will tend to use implicitly their own social givens as a theory. When the subjects studied by the sociologist participate in the formulation of emerging theory, then their own givens are added to the emerging theory. The subjects studied by sociologists in the SI tradition have made such problems less obvious. Much of the research has focused on people who have little social power or influence: little children, skid row bums, drug addicts, mental patients, immigrants, delinquents, and assorted deviants.¹⁰ The investigators are typically persons who are deeply sympathetic to and understanding of the underdogs they study, but the fact remains that any lack of consensus among the participants in such situations can be settled by the researcher with little backtalk from the participants.

The problem of scientific objectivity raised by lack of a prior theoretical formulation, by the absence of clear-cut criteria for selecting credible informants would be highlighted were the researcher to inspect a group of topdogs, say, the executives of a major corporation. In this situation, the researcher's colleagues might be uneasy if the researcher could not distinguish between theoretical concepts and observed behavior, if the hierarchy of credibility of the informants were arbitrary, and if other such judgmental procedures could hardly be replicated. Which of the participants in an interactive setting is to have most influence in determining the shape of an "emerging" theory is a question that the SI model has not confronted.

The most important way to improve the practice of SI sociology would be for its

¹⁰ Meadian sociology has been accused of ignoring the reality of social stratification. The labelling theory of deviance, a derivative of the SI approach, is an apparent exception. Deviance occurs because some powerful groups can impose their rules on subordinates (Becker, 1963:17). The focus of research shifts from types of deviance to the processes by which people become deviant (Kitsuse, 1964:87). This development shows great promise but thus far a detailed analysis of the institutional arrangements which enforce definitions has not been made. That is, the theoretical approach has not been well developed.

adherents to confront the problems raised by their ambiguity toward the logico-theoretic component in their work. Their use of the customary vocabulary of methodology illustrates the ambiguity. Often such words as theory, hypothesis, concept are given meanings which are quite different from those they have when used by conventional methodologists. In this situation, the rational discussion of important issues in the discipline is difficult. Every group, of course, has a right to define words any way it chooses; but when words already have a consensual definition, to give them a different one obfuscates communication.

Furthermore, nothing prevents a detailed observational account from being informed with notions from a stratification theory or any other theory. For the researcher to spell out in advance and in detail what is expected and why it is expected is more work than transcribing events with the atheoretical simplicity of a blank mind. But such preliminary spadework would help to integrate the findings into a larger body of work, hence make them more meaningful. To be sure, the production of theory in sociology is beset with many unsolved problems. Nevertheless, the prior construction of logically-related propositions is important in science because it gives the researcher a chance to lose the game. A theoretical formulation forces researchers to bet on a particular outcome and to explain why they bet the way they do. In the absence of such a formulation, the researcher always wins, for any outcome is permissible.

This paper has criticized the SI tradition, one of the most important approaches in the discipline of sociology. Along with ethnomethodology and other styles in the holistic tradition, however, SI has retained a freshness in its approach to data which is often lost when aggregate data emerge from the bowels of the computer. Blumer's injunction to look at real people makes good sense. The detailed accounts of the way people behave make good reading. Some of these rich reports may well survive studies awash with mathematical formulations.¹¹ But the

practitioners of SI remain nervous lest their reports be confused with mere journalism. Their fear is justified. What is needed is a frank confrontation with a major legacy of pragmatism. In the absence of theory, the social givens of the researcher and the participants serve as a theoretical framework, giving the research a bias which reflects the unstated assumptions of the researcher, the climate of opinion in the discipline, and the distribution of power in the interactive setting. Much SI research reflects a kindly concern for the people who are studied because SI methods attract those who delight in observing the nuances of human behavior. Nevertheless, when the criterion for truth is what people do, when theory emerges from practice, then the biases of those who do and act are embedded in the theory.

REFERENCES

- Becker, Howard S.
1958 "Problems of inference and proof in participant observation." *American Sociological Review* 23 (December):625-60.
1963 *Outsiders: Studies in the Sociology of Deviance*. New York: Free Press.
1970 "Whose side are we on?" Pp. 15-25 in William H. Fikstead (ed.), *Qualitative Methodology: First Hand Involvement with the Social World*. Chicago: Markham.
- Blumer, Herbert
1954 "What is wrong with social theory." *American Sociological Review* 19 (February):3-10.
1956 "Foreword." Pp. iii-vii in Severyn T. Bruyn (ed.), *The Human Perspective in Sociology*. Englewood Cliffs, N.J.: Prentice-Hall.
1969 *Symbolic Interactionism: Perspective and Method*. Englewood Cliffs, N.J.: Prentice-Hall.
- Braithwaite, R. B.
1963 *Scientific Explanation*. New York: Cambridge University.
- Bruyn, Severyn T.
1966 *The Human Perspective in Sociology*. Englewood Cliffs, N.J.: Prentice-Hall.
- Cicourel, Aaron V.
1964 *Method and Measurement in Sociology*. New York: Free Press of Glencoe.
- Curtis, James E. and John W. Petras
1970 *The Sociology of Knowledge: A Reader*. New York: Praeger.
- Denzin, Norman K.
1970 *The Research Act: A Theoretical Introduction to Sociological Methods*. Chicago: Aldine.
1971 "The logic of naturalistic inquiry." *Social Forces* 50 (December):166-82.

¹¹ Many of the studies using the latest techniques of quantification are also examples of empirical thinking (Willer, 1970), and are relatively atheoretical.

- Denzin, Norman K. (ed.)
1970 *Sociological Methods: A Sourcebook*. Chicago: Aldine.
- Dewey, John
1916 *Essays in Experimental Logic*. Chicago: University of Chicago.
1920 *Reconstruction in Philosophy*. New York: Henry Holt.
1929 *The Quest for Certainty*. New York: Minton, Balch.
1932 "Prefatory remarks." Pp. xxxvi-xl in George Herbert Mead, *The Philosophy of the Present*. Chicago: Open Court.
1939 "Experimentation in moral theory." Pp. 775-8 in Joseph Ratner (ed.), *Intelligence in the Modern World: John Dewey's Philosophy*. New York: Modern Library.
- Dewey, John, A. M. Moore, H. C. Brown, G. H. Mead et al.
1917 *Creative Intelligence*. New York: Henry Holt.
- Douglas, Jack D. (ed.)
1970 *Understanding Everyday Life: Toward the Reconstruction of Sociological Knowledge*. Chicago: Aldine.
- Durkheim, Emile
1960 "Pragmatism and sociology." Pp. 386-436 in Kurt H. Wolff (ed.), *Emile Durkheim, 1858-1917: A Collection of Essays with Translations and Bibliography*. Columbus: Ohio State University.
- Ehrlich, Howard
1972 *Personal Communication*.
- Filstead, William J. (ed.)
1970 *Qualitative Methodology: Firsthand Involvement with the Social World*. Chicago: Markham.
- Gallie, W. B.
1966 *Peirce and Pragmatism*. New York: Dover.
- Glaser, Barney G. and Anselm L. Strauss
1967 *The Discovery of Grounded Theory: Strategies for Qualitative Research*. Chicago: Aldine.
- Gouldner, Alvin W.
1968 "The sociologist as partisan: sociology and the welfare state." *The American Sociologist* 3 (May):103-16.
- Hill, Richard J.
1970 "On the relevance of methodology." Pp. 12-19 in Norman K. Denzin (ed.), *Sociological Methods: A Sourcebook*. Chicago: Aldine.
- Huber (Rytina), Joan and Charles Loomis
1970 "Marxist dialectic and pragmatism: power as knowledge." *American Sociological Review* 35 (April):308-18.
- Kaplan, Abraham
1964 *The Conduct of Inquiry: Methodology for Behavioral Science*. San Francisco: Chandler.
- Kitsuse, John I.
1964 "Societal reactions to deviant behavior: problems of theory and method." Pp. 87-102 in Howard S. Becker (ed.), *The Other Side: Perspectives on Deviance*. New York: Free Press.
- Kuhn, Manfred
1970 "Major trends in symbolic interaction theory in the past twenty-five years." Pp. 70-87 Gregory P. Stone and Harvey A. Farberman (eds.), *Social Psychology through Symbolic Interaction*. Waltham, Mass.: Xerox.
- Lee, Grace Chln
1945 *George Herbert Mead: Philosopher of the Social Individual*. New York: King's Crown.
- Lewis, J. David
Forth- "Peirce, Mead, and the objectivity of coming meaning." *Kansas Journal of Sociology*.
- Lofland, John
1971 *Analyzing Social Settings: A Guide to Qualitative Observation and Analysis*. Belmont, Cal.: Wadsworth.
- Manis, Jerome G. and Bernard M. Meltzer
1967 *Symbolic Interaction: A Reader in Social Psychology*. Boston: Allyn and Bacon.
- Mead, George Herbert
1932 *The Philosophy of the Present*. Edited by Arthur E. Murphy with prefatory remarks by John Dewey. Chicago: Open Court.
1934 *Mind, Self & Society*. Edited, with introduction, by Charles W. Morris. Chicago: University of Chicago.
1936 *Movements of Thought in the Nineteenth Century*. Edited by Merritt H. Moore. Chicago: University of Chicago.
1938 *The Philosophy of the Act*. Edited, with introduction by Charles W. Morris in collaboration with John M. Brewster, Albert M. Dunham, and David L. Miller. Chicago: University of Chicago.
1964 *Selected Writings*. Edited, with introduction, by Andrew J. Reck. Indianapolis: Bobbs-Merrill.
- Meltzer, Bernard N.
1959 *The Social Psychology of George Herbert Mead*. Kalamazoo, Mich.: Western Michigan University.
- Mills, C. Wright
1962 *The Marxists*. New York: Dell.
1966 *Sociology and Pragmatism: The Higher Learning in America*. New York: Oxford University.
- Morris, Charles W.
1934 "Introduction." Pp. ix-xxxv in George H. Mead, *Mind, Self & Society*. Chicago: University of Chicago.
- Petrus, John W.
1968 "John Dewey and the rise of interactionism in American social theory." *Journal of the History of the Behavioral Sciences* 4 (January):18-27.
- Forth- "George Herbert Mead's theory of self: coming a study in the origin and convergence of ideas." *The Canadian Review of Sociology and Anthropology*.
- Petrus, John W. and Bernard N. Meltzer
Forth- "Theoretical and ideological variations in coming contemporary interactionism." *Catalyst*.
- Reck, Andrew J.
1964 "Preface" and "Editor's Introduction."

- Pp. v-xxxii in George Herbert Mead, *Selected Writings*. Edited, with introduction by Andrew J. Reck. Indianapolis: Bobbs-Merrill.
- Russell, Bertrand
1945 *A History of Western Philosophy*. New York: Simon and Schuster.
- Shibutani, Tamotsu
1968 "Mead, George Herbert." Pp. 83-7 in David L. Sills (ed.), *International Encyclopedia of the Social Sciences* 10. New York: Macmillan and Free Press.
- Stevens, Edward
1967 "Biographical Note: G. H. Mead," *American Journal of Sociology* 72 (March): 551-7.
- Stone, Gregory P. and Harvey A. Farberman (eds.)
1970 *Social Psychology through Symbolic Interaction*. Waltham, Mass.: Xerox.
- Wennerberg, Hjalmar
1962 *The Pragmatism of C. S. Peirce: An Analytical Study*. Lund, Sweden: CWK Gleerup.
- White, Morton C.
1943 *The Origin of Dewey's Instrumentalism*. New York: Columbia.
- Willer, David and Murray Webster, Jr.
1970 "Theoretical Constructs and Observables." *American Sociological Review* 35 (August): 748-57.
- Willer, David and Judith Willer
1972 *Systematic Empiricism: Critique of a Pseudo-Science*. Englewood Cliffs, N.J.: Prentice-Hall.
- Willer, Judith
1970 *The Social Determination of Knowledge*. Englewood Cliffs, N.J.: Prentice-Hall.
- Zajonc, Robert B.
1968 "Conformity." Pp. 253-60 in David L. Sills (ed.), *International Encyclopedia of the Social Sciences* 3. New York: Macmillan and Free Press.

ITEMS (*Continued*)

■ **Karen Oppenheim Mason** is a Senior Sociologist at the Center for Population Research and Services of the Research Triangle Institute, North Carolina. Currently investigating the impact of women's labor force experience on fertility, she is also interested in contemporary sex role definitions and in fertility control programs. **William M. Mason** is Assistant Professor of Sociology at Duke University. He is studying various aspects of the achievement process. Particular attention is directed to the consequences of errors in students' reports of parental socioeconomic status for models of that process, and to questions of ability-education interactions, and nonlinearities in the effects of ability and education on socioeconomic attainment. **H. H. Winsborough** is Professor of Sociology at the University of Wisconsin, Madison. **W. Kenneth Poole** is a Senior Statistician and Head of the Methodology Department at the Research Triangle Institute. His major interests are multivariate and time series analysis, and his most recent applied work has been in the areas of analysis of dental caries data and development of fertility measures.

■ **Murray Webster** is Assistant Professor of Social Relations at Johns Hopkins University, where he teaches courses in theory construction and social psychology. His current research (with Doris Entwisle) involves applying a formal theory to produce practical improvements in children's performance expectations, and conducting laboratory tests of information processing models in sociological theory.

■ **Joan Huber** is Assistant Professor of Sociology at the University of Illinois, Urbana. She has recently completed a monograph on income and ideology with William Form, and a text-reader on the sociology of poverty with Paul Chalfant, and she was special editor for the *American Journal of Sociology's* recent issue on women. J.F.S.

ERRATUM

The Editor is embarrassed to report that the "case study" cited by James S. Coleman ("Loss of Power," ASR, February, 1973, page 14) was misplaced in the body of the text rather than following the introductory remarks in footnote 12. Our apologies to Professor Coleman and to our readership.



AMERICAN SOCIOLOGICAL REVIEW
Volume 38

Number 3

Anderson	Casual Models and Social Indicators	Cartwright Schwartz	The Invocation of Legal Norms
Blau Folbre	Trends in Occupational Mobility	Heap Roth	Phenomenological Sociology
Glazer	On Urban Alienations and Anomie	Hewitt Hall	Social Problems, Situations, and Theories
Jackman	Education, Prejudice and Response-Set	Nelsen et al	Ministerial Roles and Social Action

Comments

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit three copies, and retain a copy for your own files.* Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—doublespaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated within reference. ["... have claimed that this is so (but see Jones, 1952:99 for conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. ["... as many have noted (Johnson, 1942; Perry, 1947; Linquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29 (October):345–66.

1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32 (February):5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Stanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

* Manuscripts will not be returned unless accompanied by a self-addressed, stamped envelope.

CAUSAL MODELS AND SOCIAL INDICATORS:
TOWARD THE DEVELOPMENT OF SOCIAL SYSTEMS
MODELS¹

JAMES G. ANDERSON

Purdue University

American Sociological Review 1973, Vol. 38 (June):285-301

To be meaningful social indicators must be components of some social systems model so that changes in the values of these social statistics over time tell us something about the functioning of the social system. A necessary next step in developing social indicators is constructing models involving interrelated sets of social indicators in each major institutional area of society.

This paper reports an initial effort to derive a set of social indicators for the area of health care. A structural equation model has been constructed for the health care system serving the state of New Mexico. The model includes a network that specifies the causal relationships hypothesized as existing among a set of social, demographic, and economic variables related to the availability and use of health services and to health status; a set of structural equations that indicate the direct effect of variables in the model on each endogenous variable; a set of reduced form equations that indicate the combined direct and indirect effect of each predetermined variable on each endogenous variable included in the model.

The model can be used to provide monitoring information pertaining to the effect of a change in a particular variable on all other variables comprising the health care system. Also it provides explanatory information regarding the differences in the availability of health care services, their use, and the health-status of the population in various counties. Finally, predictions of the effects of alternative health care policies that would affect the supply, the organization of care, or patterns of use of health services can be made based on the model.

DESPITE widespread interest in the use of social indicators for policy formation and program evaluation (Bauer, 1966; National Commission on Technology, Automation and Economic Progress, 1966; U.S. Department of Health, Education, and Welfare, 1969) and a burgeoning literature on the subject (Wilcox, et al., 1972), progress to date has been disappointing. The authors of the one national social report that has been prepared define a social indicator to be:

... a direct measure of welfare ... subject to the interpretation that if it changes in the "right" direction, while other things remain

equal, things have gotten better, or people are "better off" (U.S. Department of Health, Education, and Welfare, 1969:97).

This definition leads to largely descriptive reporting of social conditions. In fact the major stated purpose of the report was to "... give social problems more visibility and thus make possible more informed judgments about national priorities" (U.S. Department of Health, Education, and Welfare, 1969: xii).

Land (1971) and Wilcox and Brooks (1971) criticize this approach, pointing out that the success of economic indicators results from the fact that such indicators are interrelated components of an economic model in which variation in the values of indicators tells us something about the functioning of the economic system. Without

¹ This paper is a revised version of a paper presented at the annual meeting of the Rural Sociological Society, Baton Rouge, Louisiana, August 25-29, 1972.

theoretical models of social systems specifying the nature of relationships among indicators of states of the system, such indicators are inadequate for policy planning and program evaluation. The mere accumulation of time series data on selected social indicators provides little more than descriptive data concerning societal conditions, and does not permit separation of the effects of public policies and programs from the impact of social processes such as migration, urbanization, and industrialization (Wilcox and Brooks, 1971; Sheldon and Freeman, 1970).

If we accept the modified definition of social indicators proposed by Land (1971) and Wilcox and Brooks (1971) that views social indicators as components of a social system that describes the functioning of that system, then the next step is one of inductive model building in each of the major societal institutional areas. Causal models are needed to relate the social impact of major social processes and public policies to indicators of quality of life. Land (1971) suggests that such a focus on models that describe specific social processes may permit us to develop social models comparable to economic models that have been used successfully for public policy formation for some time. The construction of such models, however, requires the empirical verification of their conceptual basis (Perle, 1970). To date the failure of research in the area of social indicators to go much beyond the collection of individual indicators of quality of life that lend themselves to little more than description has reflected the lack of a prerequisite methodology (Wilcox and Brooks, 1971).

Causal modelling procedures that have been developed in biology (Wright, 1934, 1954, 1960; Li, 1956), economics (Wold and Jureen, 1953; Wold, 1954; Goldberger, 1964; Christ, 1956), and the social sciences (Blalock, 1964, 1971; Duncan, 1966; Land, 1969) potentially provide the necessary methodology. These approaches permit articulation among the verbal language used to state social theories, the operational language that specifies how concepts are to be measured, and the mathematical language that permits the construction of simultaneous equation models that lead to the empirical verification of the theory (Blalock, 1964, 1969).

THEORETICAL MODEL

The purpose of this paper is to construct a preliminary model for the health care sector. Census data, vital statistics, hospital statistics, and health manpower data have been used to construct a model of the health care system serving the state of New Mexico. The model relates basic demographic processes to indicators of the health care delivery system which in turn are related to indicators of the health-status of the population. In the following sections of this paper the construction and analysis of this model will be described.

Health-Status Indicators

The performance of a health care system and the evaluation of public policies affecting the delivery of care ultimately must be appraised in terms of their ability to improve the health of a target population. In the past health-status indices based on mortality have been used as measures of the level of health (U.S. Center for Health Statistics, 1966a). Recently several measures based on mortality and morbidity (U.S. National Center for Health Statistics, 1965) and on disability (Fanshel and Bush, 1970; Sullivan, 1971) have been proposed. These latter indices have the disadvantage that data are not routinely collected on morbidity and disability at the state, county, or metropolitan area level.

In this study the infant mortality rate has been taken as a measure of a county's health-status since it is a sensitive index of the level of health existing in an area and is responsive to a host of social and economic factors (Wrong, 1956; Anderson, 1958). While this rate has stabilized nationally in recent years, among minority groups and in rural areas of the United States it remains high (U.S. National Center for Health Statistics, 1966b). For example, while the national infant mortality rate in 1968 was 21.8 infant deaths per thousand live births, the rate for New Mexico was 23.9. County rates varied from a high of 91.0 to a low of 10.6 with nine of the thirty-two county rates exceeding thirty infant deaths per thousand live births. Consequently, this measure appears to be a good social indica-

tor of the health-status of the population in various New Mexico counties.

Indicators of Health Care Delivery

Direct attempts to affect health-status involve changes in the system that delivers health care. At the present time this system is comprised of a collection of largely autonomous private physicians, public and private clinics and hospitals, both short-term and long-term, nursing homes, medical laboratories, and pharmacies. Enlightened policy formation requires an understanding of the aggregate behavior of these independent components of the health care delivery system. Presently the federal government subsidizes the construction of hospital facilities through the Hill-Burton Act, health insurance for aged and indigent through Medicare and Medicaid, and the operation of neighborhood health centers through the Office of Economic Opportunity. Present bills pending in Congress, if enacted, would affect the nature of health insurance for the population as a whole, the organization of care through the formation of Health Maintenance Organizations, and the training and use of physician's assistants and other allied health personnel. The affects of each of these policies on indicators of health-status would result both directly and indirectly from changes in the availability of health manpower and facilities and in modifications to the current patterns of use of health care. Choices among these policies then, should be made in terms of their effects on the health-status of the population (Feldstein, 1967; Anderson, 1972a, 1972b).

Consequently, the proposed model incorporates several indicators of the aggregate behavior of the delivery system. First the number of general practitioners as well as the number of medical specialists available per 100,000 population have been used as indicators of the supply of health manpower. Similarly the number of short-term general hospital beds available per thousand population has been incorporated into the model as an indicator of the supply of facilities. Finally, the proportion of births that occurred in the hospital has been used as an indicator of current use patterns.

Social and Demographic Factors Affecting Health-Status and Health Care Delivery

Social and demographic factors and the processes that change them affect health-status, the supply of health manpower and facilities, and the use of health care services. For example, income is highly related to infant mortality as an early study of infant mortality rates in eight cities in the United States revealed (Woodbury, 1925). A later study in Cleveland found the same relationship even when income was divided into deciles (Green, 1939). This finding is further substantiated by a study of 973 cities in the United States during the period 1939-1940 (Altenderfer and Crowther, 1949) as well as by studies in Holland (Collins, 1927), Stockholm (Titmuss, 1943), and England and Wales (Morris and Heady, 1955). More recent evidence from Providence, Rhode Island and Boston, Massachusetts, however, suggests that the relationship between infant mortality and income may be changing. Data from these two studies reveal that neonatal mortality appears to be associated with biological factors rather than with socioeconomic characteristics. At the same time deaths during the postneonatal period were found to be inversely related to socioeconomic factors such as income (Stockwell, 1962, 1963; Donabedian, et al., 1965).

Urbanization is another significant factor that affects the health-status of a population. National statistics indicate significant rural-urban differences in infant mortality (U.S. National Center for Health Statistics, 1966b). Indirectly health may be affected by changes in the structure of the family (Adams, 1968; Gore, 1968) and the deleterious effect of segregated neighborhoods upon child bearing and rearing (Yankauer, 1950; Yankauer and Allaway, 1958) that result from increased urbanization. A more direct effect is due to the increased availability of health care services to urban populations.

Moreover, the presence of large minority racial and ethnic groups in a county has a profound effect on social and economic characteristics of its population as well as upon health-status. Woodbury's (1925) study of eight American cities revealed differential infant mortality rates among sev-

eral groups of foreign born whites. Native born nonwhites were found to have the highest rate; white foreign born Jews were found to have the lowest rate. Anderson (1958) in reviewing the findings of this study cites cultural factors unique to this ethnic group as an explanation for this unanticipated result. More recent statistics reveal differences in infant mortality rates for whites and nonwhites in the United States as a whole (U.S. National Center for Health Statistics, 1966b, 1971).

Income also affects use of health care services, especially hospitalization. The results of three national surveys have demonstrated a relationship between hospital admission rates and family income (Andersen and Anderson, 1967). Richardson (1969) found an even more pronounced relationship between use and income in analyzing hospital data for 1967. Moreover, Rosenthal (1965) and Feldstein and German (1965) found income to be an important predictor of hospital use rates in the several states.

Education also appears to be related to hospital utilization. Rosenthal (1965) found education, one of five components extracted from a principal components analysis of twelve population characteristics, to be a predictor of hospital admission rates and average length of stay. He interpreted this finding as indicative of increased awareness among highly educated persons of the value and importance of seeking medical care. Moreover, the kind of jobs open to persons with little education may require continuous work with no provisions for sick leave. This would provide an incentive, independent of cost, for poorly educated persons to avoid hospitalization if at all possible.

Differences in hospital use rates are also apparent between rural and urban areas with the lowest hospital utilization rates occurring among urban residents, even when differences in the age distribution of the two populations are taken into account (Andersen and Anderson, 1967). Also prediction models developed by Feldstein and German (1965), by Rosenthal (1965), and by Feldstein (1967) all indicate that percentage urbanization is an important predictor of hospital usage.

The ethnic composition of the population

also has a major impact on hospital use. Data indicate that nonwhites do not use health services as often as whites do (Richardson, 1969). This differential use reflects differences in age, education, income, and the availability of health services among others.

The supply of health manpower also appears to be significantly affected by differences in the composition of the population served and in the medical environment. Marden's (1966) study of the influence of five demographic and ecological variables on the distribution of physicians within metropolitan areas of the United States revealed significant differences between the factors that affect the distribution of general practitioners and those that affect the distribution of medical specialists. Age and race were found to be the most important factors in accounting for differences in the supply of general practitioners. For specialists, the educational level of the population and the general medical environment of the area, in particular the number of short-term general hospital beds available, were found to be important determinants of supply.

THE SIMULTANEOUS EQUATION MODEL

The model used to describe the complex interrelationships that obtain between components of the health care system and its target population consists of a system of simultaneous equations. Such a set of equations can be used to determine the response of health-status indicators to changes in components of the health care system, such as physician supply, or in population characteristics.

In this section a simultaneous equation model of the health care system serving the state of New Mexico is developed. Data on the thirty-two New Mexico counties from the U.S. Bureau of the Census (1971), annual guide issues of *Hospitals*, the Journal of the American Hospital Association (1971), physician distribution statistics published annually by the American Medical Association (1967, 1971), and Vital Statistics of the United States (U.S. National Center for Health Statistics, 1970, 1971) have been used to estimate the parameters of this model.

Endogenous Variables

The model accounts for variation in a set of endogenous variables that characterize the health care system and the population it serves. The social and demographic variables are age (AGE) as measured by the percentage of persons aged sixty-five and over, education (ED) as measured by the proportion of the population twenty-five years of age or over who have completed twelve or more years of education and income (INCOME) as measured by the proportion of the population with income less than \$3,000. The number of general practitioners (GP) and medical specialists (SP) per 100,000 population, the number of short-term general hospital beds (BEDS) available per thousand population, and the percentage of births that occurred in the hospital (BRNHOSP) have been used to characterize the health care delivery system.

Infant mortality (INFMORT), or the number of infant deaths per 1,000 live births, has been used as a health-status indicator. Rates for 1968 have been used since this is the latest year for which county statistics have been published. Since the principal causes of mortality vary greatly with the age of the infant, the infant mortality rate has been broken down into a neonatal mortality rate (NEONTL), or the number of infants who die during the first twenty-seven days per thousand live births, and a postneonatal mortality rate (PNEONTL), or the rate of infant deaths occurring between the ages of one month and one year. In the first instance, adverse conditions present during the fetal period and surrounding the birth of the child are important causes of neonatal deaths. During the postneonatal period, poor nutrition and sanitation and lack of proper preventive care play a large role in infant deaths (Stockwell, 1962; Donabedian, et al., 1965; U.S. National Center for Health Statistics, 1966b).

Predetermined Variables

Predetermined variables consist of exogenous variables and lagged values of the endogenous variables. The exogenous variables include the proportions of nonwhites (NW) and Spanish-Americans (SA) residing in the county, and the proportion of

persons who lived in urban areas (URBAN) in 1970. 1964 levels of general practitioners (GP₁₉₆₄) and medical specialists (SP₁₉₆₄), the first year that these data were published by county, were used as additional predetermined variables in the model. Table 1 shows the intercorrelations among the two sets of variables.

The structural model is graphically represented in Figure 1.

Identification of the Model

Before estimating the parameters of the structural equations, it is necessary to determine whether or not a sufficient number of constraints have been imposed on the set of equations so that the parameters can be identified. Otherwise there will be an infinite number of sets of parameter values that are compatible with both the data and the restrictions, and the structure of the model can not be determined (Christ, 1966). In the type of model presented here, where the only constraints imposed are that certain variables do not appear in certain equations used to predict values of the endogenous variables, Fisher (1966) provides a means of determining whether or not each equation is identified. An equation is identified if a number of variables equal to one less than the total number of equations is excluded from that equation. In the present model an equation is identified if eight variables are excluded from the equation. Inspection of equations (1) through (9) that appear below indicates that all of these equations are identified.

Estimation of Model Parameters

A two-stage least squares technique has been used to estimate parameters of the model since ordinary least squares would provide parameter estimates that are both biased and inconsistent (Johnston, 1963; Goldberger, 1964). In the first stage of this method, each endogenous variable is regressed on all of the predetermined variables using ordinary least squares.² Next observed

² In the first-stage, an additional predetermined variable was used to estimate the endogenous variables included in the model. This variable was the 1960 value of the hospital bed to population ratio.

Table 1. Intercorrelation Matrix for Endogenous and Predetermined Variables

	X ₁	X ₂	X ₃	X ₄	X ₅	X ₆	X ₇	X ₈	X ₉	X ₁₀	X ₁₁	X ₁₂	X ₁₃	X ₁₄	X ₁₅
X ₁	1.00	-.11	-.20	-.27	-.10	-.41	-.29	.07	-.25	-.14	.18	-.03	-.07	.08	.01
X ₂		1.00	-.54	.03	.59	-.33	.55	-.67	-.13	.62	.56	.41	-.03	-.31	-.22
X ₃			1.00	-.10	-.14	.13	-.59	.64	.04	-.15	-.47	-.20	-.14	.14	.00
X ₄				1.00	-.39	.55	.00	.00	.61	-.19	.35	-.06	-.05	-.37	-.25
X ₅					1.00	-.44	.62	-.49	-.26	.92	.21	.23	-.11	-.14	-.15
X ₆						1.00	-.16	.40	.18	-.26	.07	-.24	.13	.18	.21
X ₇							1.00	-.77	-.05	.64	.33	.24	.18	-.13	.03
X ₈								1.00	-.06	-.43	-.27	-.26	-.13	.20	.05
X ₉									1.00	-.30	.02	-.18	-.11	-.23	-.19
X ₁₀										1.00	.33	.23	-.10	-.23	-.20
X ₁₁											1.00	.34	.04	-.20	.11
X ₁₂												1.00	.34	-.05	.15
X ₁₃													1.00	.37	.81
X ₁₄														1.00	.84
X ₁₅															1.00
Mean	7.83	46.78	46.27	28.46	23.23	39.50	23.68	20.45	22.58	29.13	33.09	97.36	17.54	9.81	27.51
S.D.	13.62	34.04	23.22	16.46	32.59	4.48	6.31	8.79	14.38	34.33	2.67	4.22	9.48	9.77	16.16

NOTE: X₁ - (NW) Percentage Non-white 1970.X₂ - (URBAN) Percentage Urban 1970..X₃ - (SA) Percentage Spanish-American 1970.X₄ - (GP₁₉₆₄) Number of general practitioners per 100,000 population 1964.X₅ - (SP₁₉₆₄) Number of specialists per 100,000 population 1964.X₆ - (AGE) Percentage of persons ages 65 and over 1970.X₇ - (ED) Percentage of persons 25 years of age or older with 12 or more years of education 1970.X₈ - (INCOME) Percentage of families with incomes below \$3,000 1970.X₉ - (GP) Number of general practitioners per 100,000 population 1970.X₁₀ - (SP) Number of specialists per 100,000 population 1970.X₁₁ - (BEDS) Number of short-term general hospital beds available per 1,000 population 1970.X₁₂ - (BRNIIOSP) Percentage of births in the hospital 1968.X₁₃ - (NEONTL) Neonatal mortality rate--infant deaths per 1,000 live births 1968.X₁₄ - (PNEONTL) Postneonatal mortality rate--infant deaths per 1,000 live births 1968.X₁₅ - (INFMORT) Infant mortality rate--infant deaths per 1,000 live births 1968.

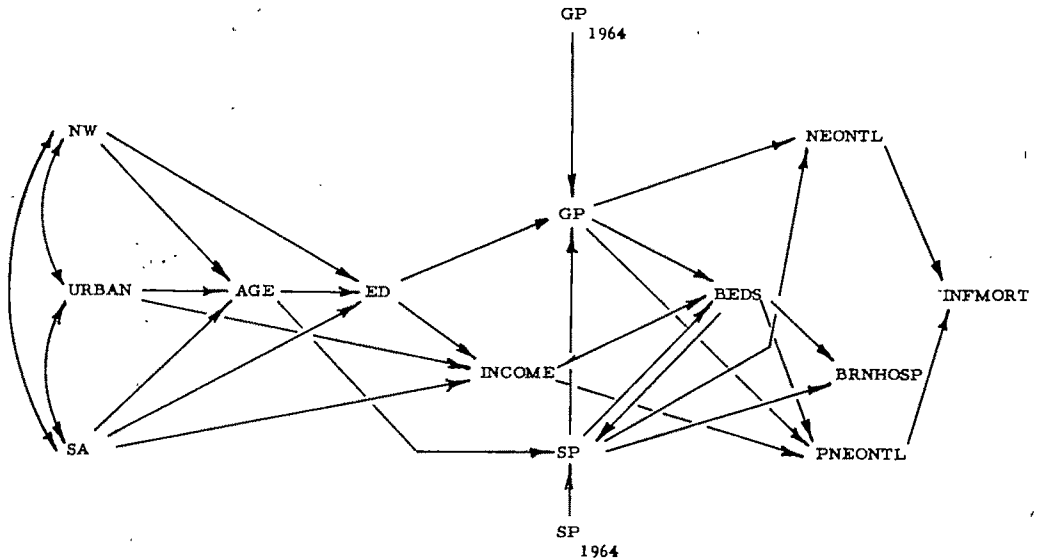


FIGURE 1. STRUCTURAL EQUATION MODEL OF THE HEALTH CARE SYSTEM.

values of the predetermined variables are substituted into these equations in order to estimate the values of the endogenous variables. In the second stage, the parameters of the original set of structural equations are estimated by ordinary least squares using the predicted values of the endogenous variables and observed values of the predetermined variables as independent variables. Coefficients estimated by this two-stage procedure are still biased but are consistent and more efficient than the values that would have been obtained using the ordinary least squares technique (Johnston, 1963; Goldberger, 1964). Parameter estimates for the structural equations obtained by using this method are presented in the next section.

The Structural Equations

An equation is written for each endogenous variable included in the model. The coefficients indicate the direct effect that an independent variable has on a dependent endogenous variable with the effect of all other variables appearing in that equation

Fisher (1971) discusses the merits of this approach and suggests that any such variable should be uncorrelated with the disturbance term in that equation and should have a significant causal influence on the endogenous variable while at the same time its effect should be somewhat independent of the other predetermined variables appearing in the equation.

controlled (Suits, 1962). For example, age is considered to be affected by both the ethnic composition and the urban structure of a county. The equation then is:

$$(1) \text{ AGE} = 16.166 - 0.169 \text{ NW} - 0.067 \text{ URBAN} - 0.047 \text{ SA} + \text{error}$$

(0.053) (0.025) (0.037) $R = 0.587$

The numbers shown in parentheses are standard errors for the regression coefficients. The multiple correlation coefficient is shown below the equation.

The sign of the coefficient for the percentage of persons residing in urban areas is negative suggesting that as urbanization increases, the percentage of the population aged sixty-five or over residing in New Mexico counties declines. This probably results from migration since out migration from rural to urban areas tends to remove young adults and add them to the urban population. Also, in a state such as New Mexico, immigration induced by the concentration of military installations and aerospace industries adds additional young adults to the urban population.

Age is also affected by the ethnic composition of the county. In those counties that contain large concentrations of Spanish-Americans and nonwhites, mostly American Indians in New Mexico, the proportion of

the aged is lower. This relationship may partly reflect the reduced life expectancy of both ethnic groups (Moustafa and Weiss, 1968) but is primarily due to their higher fertility (U.S. Bureau of the Census, 1963a, 1963b; Bogue, 1969).

The structural equation for education indicates that the presence of either minority group in a county lowers the percentage of persons twenty-five years of age or older who have completed a high school education. This finding reflects the well documented educational gap existing among Spanish-Americans and American Indians. In 1960 only 18.4 percent of the Spanish-Americans and 15.3 percent of the American Indians age fourteen and over in the southwest had completed high school. At the same time among Anglo-Americans, 22.1 per cent had completed some college and one-half had completed high school (U.S. Bureau of the Census, 1963a 1963b). Age affects education in a similar fashion. As the aged population of a county grows, the educational level declines, reflecting the general secular trend in which succeeding cohorts have attained increasingly higher amounts of education.

$$(2) \text{ ED} = 39.515 - 0.270 \text{ NW} - 0.178 \text{ SA} - 0.577 \text{ AGE} \\ (0.067) \quad (0.034) \quad (0.269)$$

$$R = 0.768$$

Income is first and foremost a function of education. This relationship obtains even when differences between males and females, age groups, and whites and nonwhites are taken into account (Glick and Miller, 1956; Miller, 1966). Equation (3) reflects this relationship. The regression coefficient for the percentage of persons twenty-five years of age who have completed twelve or more years of education is large and negative. A two percent increase in the population with at least a high school education leads to a one percent decrease in the proportion of families with incomes less than \$3,000.

The low income status of the Spanish-American population is also evident. The poverty population of a county increases with the proportion of Spanish-Americans residing in that county. This result is quite predictable since statistics from the census

indicated that 54 percent of the rural and one-third of the urban Spanish-American families living in New Mexico in 1959 had incomes under \$3,000 (Leonard and Johnson, 1967). Urban and rural differences in income are also reflected by the sign of the coefficient for this term in the equation. As a county urbanizes, the poverty population declines in New Mexico. This may be largely a result of increased job opportunities and selective migration into urban areas.

$$(3) \text{ INCOME} = 32.238 - 0.088 \text{ URBAN} + 0.092 \text{ SA} - 0.504 \text{ ED} \\ (0.040) \quad (0.061) \quad (0.266) \quad R = .782$$

Non-federal physicians have been divided into two groups, those engaged in private general practice and those engaged in specialties. The structural equations for the two physician to population ratios are shown below. Marden (1966) has shown that the distribution of physicians in the United States is influenced by demographic and ecological variables. Not only is a certain size population needed to support a medical practice, particularly a specialty; but urban areas provide physicians with the social, cultural, and professional opportunities that are important to them in establishing a location for their practice.

The composition of the population is also important. Data from the National Health Survey indicate that physician visits are higher among persons sixty-five and over and among those under five. Also physician visits differ among income groups with the highest rates occurring among persons with family incomes of \$3,000 or less or \$10,000 or more. These differences reflect a greater amount of preventive care among the high income group; while increased use among the low income group reflects greater need, the disproportionate number of elderly persons who fall into this group, and the availability of health services for persons below the poverty line (U.S. National Center for Health Statistics, 1968). Consequently, it was hypothesized that physician-population ratios would be higher in areas where there was a concentration of those segments of the population who use their services the most.

Equations (4) and (5) in general sup-

port these hypotheses. The supply of general practitioners appears to increase with the educational level of the population. Elesh and Schollaert (1972) also found the distribution of physicians in Chicago to be related to the educational level of the population served. While this finding suggests that physicians respond to the demand created by more highly educated individuals who place a high value on medical care and who can afford the cost of physicians' services, the relationship between education and demand for medical care is probably more complex. Socioeconomic factors such as education affect the form of social organization to which an individual is exposed as well as his medical orientation. Both of these factors have been shown to be related to the source of care that the individual seeks (Suchman, 1965a).

Also the number of medical specialists serving a county population increases with the population sixty-five years of age or older. Since need increases markedly with age (U.S. National Center for Health Statistics, 1966c) creating a greater demand for the services of medical specialists, it would appear that the supply of specialists is responsive to the medical needs generated by the aged. Moreover since New Mexico, like many other areas of the southwest, attracts persons of retirement age, this finding may indicate that medical specialists are recruited in part to serve this population.

Nevertheless, examination of the coefficients for the previous supply of general practitioners and specialists in equations (4) and (5) reveals that physician supply is less sensitive to changes in characteristics of the population than one might expect. The standardized coefficient of the lagged variables in each equation is considerably larger than the coefficients for other terms in that equation. This indicates that the previous supply of physicians in a county is a major determinant of present medical manpower levels. The relative magnitudes of the coefficients of the lagged variables also suggest a differential response to population changes by the two groups of physicians. General practitioners are more responsive to changes in the population than are medical specialists. This relative insensitivity of physician supply was also noted by Feldstein (1967) in analyzing

data from the several states, although he found only a slight difference in the response rates of general practitioners and specialists.

Hospital facilities appear to be a critical factor in attracting specialists. One can conclude that the supply of medical specialists is more sensitive to changes in the medical environment of a county than is the supply of general practitioners. Marden (1966) came to a similar conclusion in his study. He found significant differences between the distributions of the two groups of physicians within population size categories and concluded that factors other than size play a more important role in determining the distribution of specialists than general practitioners. While there were more general practitioners in those metropolitan areas where there were fewer hospital facilities, such facilities were found to be very important for specialists in every size category.

The supply of general practitioners also appears to be affected by the supply of medical specialists. General practitioners appear to be in greater supply in counties that have fewer specialists. This may indicate that general practitioners are substituted for specialists in areas where the latter are in short supply.

$$(4) \text{ GP} = 5.950 + 0.514 \text{ GP}_{1964} + 0.154 \text{ EL} \\ (0.135) \quad (0.545) \\ - 0.056 \text{ SP} \\ (0.096) \quad R = 0.620$$

$$(5) \text{ SP} = -22.139 + 1.026 \text{ SP}_{1964} + 1.866 \\ (0.778) \quad (0.722) \\ \text{AGE} + 3.135 \text{ BEDS} \\ (1.027) \quad R = 0.952$$

The supply of hospital beds appears to be affected by changes in economic characteristics of the population. Hospital beds are in shorter supply in New Mexico counties that contain large numbers of families with incomes below the poverty line. As the proportion of families with incomes below \$3,000 increases, the hospital bed to population ratio decreases. Since a number of national studies have shown that the demand for hospitalization is greatest among low income groups (Andersen and Anderson, 1967), this finding is of some significance.

Hospital bed supply also appears to be a

function of the number of general practitioners and medical specialists practicing in a county. As the physician-population ratio increases, so does the hospital bed-population ratio. Since physicians rely heavily on hospital facilities, they may be instrumental in affecting expansions in the existing supply of hospital beds.

$$(6) \text{ BEDS} = 5.230 - 0.166 \text{ INCOME} + 0.043 \text{ GP} + 0.010 \text{ SP} \\ (0.079) \quad (0.045) \quad (0.017) \\ R = 0.519$$

An examination of the three equations pertaining to the supply of physicians and hospital facilities suggests some tentative hypotheses concerning the process by which the supply of physicians and short-term hospital beds changes over time. Equation (6) indicates that hospital beds increase in a county with the supply of physicians since the signs for both general practitioners and specialists in this equation are positive. One explanation for this finding is that physicians may be instrumental in mobilizing the community resources necessary to expand the supply of hospital beds for a county population. Hospital facilities, in turn, are apparently strongly related to the supply of medical specialists in a county. Equation (5) indicates that as the hospital bed-population ratio increases so does the supply of specialists. This suggests that once a county is successful in expanding its hospital facilities it is then able to attract additional medical specialists.

A measure of health services use that has been shown to be important in accounting for differential infant mortality rates is the percentage of births that occur in the hospital (Altenderfer and Crowther, 1949). Such births occur under aseptic conditions, are attended by trained medical personnel, and usually involve better prenatal care. Despite the fact that 98.5 percent of the births that occurred in New Mexico during 1968 occurred in the hospital, equation (7) indicates that the availability of health services is an important factor in accounting for differences among counties on this measure of use. The supply of hospital beds in a county is of particular importance. An increase in ten hospital beds per 1,000 population in a

county results in a four percent increase in the percentage of births that occur in the hospital. Moreover, the supply of medical specialists has a similar but somewhat smaller effect on births. An increase in the supply of medical specialists results in a slight increase in the proportion of births that are attended by trained medical personnel in a hospital setting. Once the supply of health manpower and hospital facilities is taken into account, characteristics of the population do not appear to affect the proportion of births that occur in the hospital at all. This finding reflects a general change in use patterns that has been noted by other studies. Data from several national surveys indicate that differences among income groups in their use of physician services has diminished appreciably (Bice, et al., 1972). Other studies have come to a similar conclusion, citing the sensitivity of low income groups to the methods of financing care (Andersen and Benham, 1970; Richardson, 1970). Medicare and Medicaid and other forms of third-party coverage have been important factors in increasing access to medical care among low income groups.

$$(7) \text{ BRNHOSP} = 95.406 + 0.022 \text{ SP} + 0.425 \text{ BEDS} \\ (0.025) \quad (0.410) \\ R = 0.322$$

The two components of infant mortality have been considered separately in equations (8) and (9) since each has a somewhat different etiology. Neonatal deaths, or infant deaths that occur during the first twenty-seven days after birth, appear to be caused in the main by immaturity of the infant, by injury during birth, by congenital malformation, and by asphyxia and atelectasis (Morris and Heady, 1955; U.S. National Center for Health Statistics, 1966b); while deaths occurring during the remainder of the first year of life, postneonatal mortality, are largely due to causes that originate in the environment. For example, during 1950 pneumonia, influenza, diarrhea, and dysentery accounted for 27 percent of the infant deaths that occurred during the postneonatal period (Collins, et al., 1955). Data from 1963 reveal a similar pattern (U.S. National Center for Health Statistics, 1966b).

While socioeconomic variables have been

shown consistently to be determinants of infant mortality, the nature of this relationship is not clear. Willie's (1959) study of Syracuse indicated that the socioeconomic status of a census tract was more highly correlated with postneonatal mortality than with neonatal mortality. Correlations between each of these infant mortality measures and family income, however, revealed exactly the opposite pattern. A follow-up study of these 1950 data indicated that Negro and native white populations had similar neonatal mortality rates when socioeconomic status was held constant (Willie and Rothney, 1962). Studies by Stockwell (1963) and Donabedian, et al. (1965) found income and education to be better predictors of postneonatal mortality rates than neonatal rates.

An examination of equations (8) and (9) in general supports the findings of these earlier studies. As the proportion of families with incomes less than \$3,000 increases, the postneonatal mortality rate rises in New Mexico counties. One way in which this effect comes about is due to the low quality of housing, sanitation, and nutrition among low income families, since the causes of these deaths are largely due to infection (Collins, et al., 1955; U.S. National Center for Health Statistics, 1966b).

Both neonatal and postneonatal mortality rates decline as health services become more available. Equation (8) reveals that infant mortality during the neonatal period declines as the supply of both types of physicians increases. During the postneonatal period, the supply of general practitioners and hospital beds influences the rate of infant deaths. The only other study that has demonstrated a similar effect of physician supply on infant mortality was carried out by Shapiro and others (1958, 1960). The lower rate of prematurity and perinatal mortality among members of a prepaid group practice comprehensive medical care plan in New York when compared with the general population was attributed to the greater number of obstetricians and gynecologists available to the prepaid group.

One plausible interpretation of this finding is that increased infant mortality may be partly the result of inadequate prenatal and postnatal care among low income fam-

ilies and in counties where health services are not as readily available. A national study found that one out of seven mothers from families with incomes less than \$3,000 in 1953 did not see a physician at all during pregnancy and over two-thirds had seen a physician less than seven times during the prenatal period (Health Information Foundation, 1957). Moreover, Yankauer and others (1953) found infant mortality rates in New York to be related to the amount of prenatal care sought. A later study by Shah and Abbey (1971) in Baltimore indicated that prenatal care was a determinant of both neonatal and postneonatal mortality.

Since an earlier study found that differences in the percentage of births that occurred in hospitals largely accounted for differential infant mortality rates among racial and income groups (Altenderfer and Crowther, 1949), it is interesting to note that this variable does not appear to predict either measure of infant mortality in the present study. This finding, coupled with the fact that 98.5 percent of the births that occurred in New Mexico during 1968 occurred in the hospital, suggests that this factor may no longer be a significant determinant of infant mortality. It would appear that federal programs such as medicaid and the general extension of third-party insurance coverage may have largely reduced the disparities in the use of hospitals for maternity, an effect noted in studies of other measures of health services use (Richardson, 1970; Bice, et al., 1972).

$$(8) \text{ NEONTL} = 22.328 - 0.164 \text{ GP} - 0.038 \text{ SP} \\ (0.179) \quad (0.054)$$

SP

$$R = 0.185$$

$$(9) \text{ PNEONTL} = 17.516 + 0.164 \text{ INCOME} - 0.310 \text{ GP} - 1.313 \text{ BEDS} \\ (0.313) \quad (0.165) \quad (1.073)$$

$$R = 0.495$$

The last equation involves an identity. The infant mortality rate is the sum of the neonatal mortality rate and the postneonatal mortality rate. The equation is shown below.

$$(10) \text{ INF MORT} = \text{NEONTL} + \text{PNEONTL}$$

The Reduced Form Equations

Table 2 presents the estimated parameters of the reduced form equations. These equations express each endogenous variable as a function of the predetermined variables that were included in the model and can be algebraically determined from the structural equations.³ The coefficients can be interpreted as measuring the combined direct and indirect effect of each predetermined variable on the endogenous variable allowing for adjustments in other endogenous variables (Suits, 1962).

It can be seen from these equations that urbanization indirectly affects postneonatal mortality rates. A reexamination of the structural equations suggests that this results from changes in the socioeconomic characteristics of the population which in turn affect the availability of health services. The educational and income level of the population rises as a county urbanizes, resulting in an increase in the number of physicians and

hospital beds serving the population. It would appear that this then accounts for the decline in infant mortality rates that New Mexico counties experience as they become more urban.

The reduced form equations can also be used to predict 1980 values of the endogenous variables for each county if it is assumed that the parameters of the structural equations remain unchanged. Such forecasts can be made by substituting estimated values of the percentage of persons who are expected to live in urban areas by 1980, estimates of the ethnic composition of these counties in 1980, and 1970 values for the lagged values of the physician measures into the reduced form equations. For example, a ten percent increase in the number of persons residing in urban areas of De Baca county by 1980, assuming no change in the ethnic composition of the population, would result in a decline in postneonatal mortality rates from 27.8 infant deaths per 1,000 live births, to 8.5. The dramatic reduction in postneonatal mortality predicted to occur is the result of changes in the economic status of the population and the supply of physicians and hospital beds expected to come about as a result of urbanization. Urbanization affects the age structure, the educational and income levels of the population as can be seen from the first three equa-

³ The coefficients of the reduced form equation (π) have been computed from the coefficients of the structural equations by inverting the matrix of the coefficients of the endogenous variables (β) in the structural equation, postmultiplying the inverse by the matrix of the coefficients of the predetermined variables (Γ), and multiplying the resulting matrix by -1 . The matrix formula is $\pi = -\beta^{-1}\Gamma$ (Johnston, 1963; Goldberger, 1964).

Table 2. Parameter Estimates for the Reduced Form Equations

ENDOGENOUS VARIABLE	Predetermined Variable					
	CONSTANT	NW	URBAN	SA	GP1964	SP1964
AGE	16.166	-0.169	-0.067	-0.047	0.000	0.000
ED	30.187	-0.173	0.039	-0.151	0.000	0.000
INCOME	17.014	0.087	-0.108	0.168	0.000	0.000
GP	9.617	-0.006	0.010	-0.013	0.510	-0.059
SP	17.401	-0.373	-0.070	-0.184	0.071	1.050
BEDS	2.987	-0.018	0.018	-0.030	0.023	0.008
BRNHOSP	97.057	-0.016	0.006	-0.017	0.011	0.026
NEONTL	20.101	0.015	0.001	0.009	-0.086	-0.030
PNEONTL	13.405	0.040	-0.044	0.071	-0.188	0.008
INFMORT	33.506	0.055	-0.043	0.080	-0.274	-0.022

tions. Changes in these population characteristics in turn affect the supply of physicians and hospital beds in a county as revealed by the next three equations. The postneonatal mortality rate falls as a result of the increased availability of health services as well as the higher standard of living that comes about due to increased income (see equation 9).

The two sets of equations, structural and reduced, can also be used to predict the effects of state and federal policies designed to affect health care delivery. A governmental policy that would attempt to raise the number of hospital beds per thousand population in New Mexico from the present level of three to five to meet the standards of the Hill-Burton act (Palmer, 1956), could be represented by a change in the constant term of equation (6). The modified structural equations could then be solved for their reduced form. The coefficients of these new reduced form equations permit the calculation of revised values of all of the endogenous variables. In this way, the effects of various government policies could be predicted and compared.

CONCLUSION

An effective approach to the development of social indicators must bring together theory and observation so that conceptual models relating indicators that characterize social system states can be interrelated. In this fashion, variation in the values of these indicators tells us something about the functioning of the social system involved. The development of such social systems models has been hampered to date by the lack of the necessary methodology. In this paper such a methodology has been proposed and a simultaneous equation model has been developed linking sociodemographic characteristics of the population, measures of the availability of health services, and a health-status index, namely infant mortality rates. The model provides important insights into the effects of population dynamics on the health care delivery system and on the health-status of the population. The structural equations and reduced form equations taken together provide a means of predicting the effects of

various government policies that would effect the supply of physicians and hospitals.

The model is obviously a tentative one and needs to be extended to include social and social psychological variables that have been shown to affect the use of health care services and health status. For example, while a vast literature exists demonstrating differential use and health status among socioeconomic classes (Anderson, 1963; Andersen and Anderson, 1967; Richardson, 1969; Aday, 1972; Anderson, 1973), recent evidence indicates that such differences largely disappear as financial barriers to use are removed (Andersen and Beham, 1970; Richardson, 1970; Bice, et al., 1972). However, as Mechanic (1969) has observed, it is quite likely that in the future indirect effects of socioeconomic factors may be of far greater analytical significance in explaining variation in use and health status than the direct effects have been in the past. Suchman (1965a) found that the effects of demographic factors on medical care use were largely mediated by the type of social group structure in which the individual was involved and his knowledge of disease, skepticism concerning medical care, and his tendency to rely on others for support during illness.

A study of differential participation in the polio vaccine trial among socioeconomic groups where economic factors were absent revealed consistent differences in awareness of the disease, knowledge about the trial, and participation rates. What is even more significant is the fact that while 84 percent of the higher social class families learned of the trials by reading the newspaper, two thirds of the lower social class families initially learned of the trial through the schools and from other sources than the mass media (Deasy, 1956). The importance of the lay referral process among this latter group in seeking medical care has also been noted by Freidson (1960, 1961a, 1961b), Suchman (1965), and Gray and others (1966).

Another important study that reveals the importance of including social variables in models that attempt to account for differences in health status among subpopulations was carried out by Yankauer (1950) and Yankauer and Allaway (1958). They found that residential segregation in New York

largely accounted for differences in neonatal and postneonatal mortality among whites and Negroes. Clearly a more complete model of the health care area must incorporate social and social psychological variables like these.

Moreover, the organization of health care needs to be explicitly taken into account in future models since current governmental health programs attempt to affect the organization of care through the formation of health maintenance organizations and the training and use of physician's assistants, family nurse practitioners, and other allied personnel. Klarman (1963) in summarizing the findings of a number of studies that compared hospital use under prepaid group practice plans and fee for service plans found lower usage rates among subscribers to the prepaid plan. Anderson and Sheatsley (1959) came to a similar conclusion in comparing two such health insurance plans. Enrollees in the fee for service plan used more hospital services and had more surgery than participants in the prepaid plan, although in general both groups used physician services equally. Shapiro and others (1958, 1960) found major differences in prematurity and perinatal mortality in comparing a general population and the population enrolled in a prepaid group practice medical care program. These differences could not be attributed to differences in socioeconomic status between the two groups but rather reflected differences in patterns of obstetrical care.

Social factors again are important in interpreting the effect of the organization of care. Freidson (1961a) found that over one third of the members of a prepaid health plan occasionally sought outside care at their own expense, while ten percent of the subscribers hardly used the group practice for medical care at all. In accounting for this behavior, he found that the higher mobility among the former group, who were of high educational and occupational status, appeared to discourage strong commitment to neighborhood practitioners and reliance on the local lay referral system. The latter group who most frequently sought outside medical care were found to have a strong commitment to a private practitioner before they entered the plan.

Finally, it would be desirable to incorpo-

rate into the model a health status index that reflects illness as well as mortality (Lerner and Anderson, 1963; U.S. National Center for Health Statistics, 1966a; Moriyama, 1968). The stability of death rates for the U.S. Population in recent years makes the use of mortality statistics less desirable as measures of health status (Moriyama, 1968). Moreover, economic and social consequences of illness are becoming more important in evaluating levels of health. While serious work on the development of health indices has only recently begun, a promising approach has been suggested by Sullivan (1971). His index of health measures the expectancy of disability-free life and is based on both disability and mortality. While two populations may have similar mortality levels, they may differ substantially on the disability component. Such a measure then would provide a sensitive index of the impact of illness and injury on various subpopulations. Although information on disability at the county or metropolitan area level is not currently available, the mounting concern about health-status as an important quality of life indicator (U.S. Department of Health, Education, and Welfare, 1969) should lead to the collection of such data possibly on a sample basis. At that time, a health-status measure such as the expectation of life free of disability (Sullivan, 1971) should be incorporated into the model.

The approach elaborated here provides an important means of constructing social systems models based on theory and research. Parameters of these models can be estimated from extant data and the model evaluated. Based on the results, these models can be reformulated if necessary, used to make predictions, or to generate and test hypotheses regarding the effect of social and cultural processes on selected social indicators. The development of such a methodology is an essential next step in the development of social indicators that can be used for planning and evaluating governmental policies.

REFERENCES

- Adams, Bert N.
1968 *Kinship in an Urban Setting*. Chicago: Markham.
- Aday, Lu Ann
1972 *The Utilization of Health Services: Indices and Correlates*. Lafayette, Ind.: Health

- Services Research and Training Program, Department of Sociology and Anthropology, Purdue University.
- Altenderfer, Marion E. and Beatrice Crowther
1949 "Relationship between infant mortality and socioeconomic factors in urban areas." *Public Health Reports* 64 (March):331-9.
- American Hospital Association
1971 "Guide issue—part 2." *Hospitals* 45 (August):141-3.
- American Medical Association
1967 *Distribution of physicians in the U.S.* 1964. Chicago: American Medical Association.
1971 *Distribution of Physicians in the U.S. 1970.* Chicago: American Medical Association.
- Andersen, Ronald and Odin W. Anderson
1967 *A Decade of Health Services.* Chicago: University of Chicago Press.
- Andersen, Ronald and Lee Benham
1970 "Factors affecting the relationship between family income and medical care consumption." Pp. 77-95 in Herbert E. Klarman (ed.), *Empirical Studies in Health Economics.* Baltimore: Johns Hopkins Press.
- Anderson, James G.
1972a "Anticipating the second-order consequences of health care programs." *Health Services Research* 8 (Spring):5-10.
1972b "Causal model of a health services system." *Health Services Research* 7 (Spring):23-42.
1973 "Demographic factors affecting health services utilization: a causal model." *Medical Care* 11 (March-April):104-20.
- Anderson, Odin W.
1958 "Infant mortality and social and cultural factors: historical trends and current patterns." Pp. 10-24 in E. G. Jaco (ed.) *Patients, Physicians, and Illness.* New York: Free Press.
1963 "The utilization of health services." Pp. 349-67 in Howard E. Freeman, Sol Levine and Leo G. Reeder (eds.) *Handbook of Medical Sociology.* Englewood Cliffs, N.J.: Prentice Hall.
- Anderson, Odin W. and Paul B. Sheatsley
1959 *Comprehensive Medical Insurance—A Study of Costs, Use and Attitudes Under Two Plans.* Chicago: Center for Health Administration Studies, University of Chicago, Research Series No. 9.
- Bauer, Raymond A. (ed.)
1966 *Social Indicators.* Cambridge, Mass.: M.I.T. Press.
- Bice, Thomas W., Robert L. Eichhorn and Peter D. Fox
1972 "Socioeconomic status and use of physician services: a reconsideration." *Medical Care* 10 (May-June):261-71.
- Blalock, Hubert M., Jr.
1964 *Causal Inference in Nonexperimental Research.* Chapel Hill: University of North Carolina Press.
1969 *Theory Construction.* Englewood Cliffs, N.J.: Prentice Hall.
- 1971 *Causal Models in the Social Sciences.* Chicago: Aldine.
- Bogue, Donald
1969 *Principles of Demography.* New York: John Wiley.
- Christ, C. F.
1966 *Economic Models and Methods.* New York: John Wiley.
- Collins, S. D., K. S. Trantham and J. L. Lehman
1955 *Illness and Mortality Among Infants During the First Year of Life.* Public Health Monograph No. 31. Washington, D.C.: U.S. Government Printing Office.
- Collins, Selwyn
1927 *Economic Status and Health: A Review and Study of the Relevant Morbidity and Mortality Data.* Washington, D.C.: U.S. Government Printing Office.
- Deasy, Leila
1956 "Socioeconomic status and participation in the poliomyelitis vaccine trial." *American Sociological Review* 21 (April):185-91.
- Donabedian, Avedis L., Leonard S. Rosenfeld and Edward M. Southern
1965 "Infant mortality and socioeconomic status in a metropolitan community." *Public Health Reports* 80 (December):1083-94.
- Duncan, Otis Dudley
1966 "Path analysis: sociological examples." *American Journal of Sociology* 72 (July):1-16.
- Elesh, David and Paul T. Schollaert
1972 "Race and urban medicine: factors affecting the distribution of physicians in Chicago." *Journal of Health and Social Behavior* 13 (September):236-50.
- Fanshel, D. and J. W. Bush
1970 "A health status index and its application to health services outcomes." *Operations Research* 18 (November-December):1021-66.
- Feldstein, Martin S.
1967 "An aggregate planning model of the health care sector." *Medical Care* 5 (November-December):369-81.
- Feldstein, Paul J. and Jeremiah J. German
1965 "Predicting hospital utilization: an evaluation of three approaches." *Inquiry* 2 (June):13-36.
- Fisher, F. M.
1966 *The Identification Problem in Econometrics.* New York: McGraw-Hill.
1971 "The choice of instrumental variables in estimation of economy-wide econometric models." Pp. 245-72 in H. M. Blalock, Jr. (ed.), *Causal Models in the Social Sciences.* Chicago: Aldine.
- Freidson, Eliot
1960 "Client control and medical practice." *American Journal of Sociology* 65 (January):374-82.
1961a "The organization of medical practice and patient behavior." *American Journal of Public Health* 51 (January):43-52.
1961b *Patients' Views of Medical Practice.* New York: Russell Sage Foundation.

- Glick, Paul C. and Herman C. Miller
1956 "Educational level and potential income." *American Sociological Review* 21 (June): 307-12.
- Goldberger, Arthur S.
1964 *Economic Theory*. New York: John Wiley.
- Gore, M. S.
1968 *Urbanism and Family Change*. New York: Humanities Press.
- Gray, Robert M., Joseph P. Kesler and Philip M. Moody
1966 "The effects of social class and friends' expectations on oral polio vaccination participation." *American Journal of Public Health* 56 (December): 2028-32.
- Green, Howard W.
1939 *Infant Mortality and Economic Status, Cleveland and Five-City Area*. Cleveland: Cleveland Health Council.
- Health Information Foundation
1957 "Maternity care and its costs in the United States." *Progress in Health Services* 6:4.
- Johnston, J.
1963 *Econometric Methods*. New York: McGraw-Hill.
- Klarman, Herbert E.
1963 "Effect of prepaid group practice on hospital use." *Public Health Reports* 17:955-65.
- Land, Kenneth C.
1969 "Principles of path analysis." Pp. 3-37 in E. F. Borgatta (ed.) *Sociological Methodology 1969*. San Francisco: Jossey Bass.
1971 "On the definition of social indicators." *The American Sociologist* 6 (November): 322-5.
- Leonard, Olen E. and Helen W. Johnson
1967 *Low-Income Families in the Spanish-Surname Population of the Southwest*. Washington, D.C.: Economic Research Service, U.S. Department of Agriculture, Agricultural Economic Report No. 112.
- Lerner, M. and Odin W. Anderson
1963 *Health Progress in the United States 1900-1960*. Chicago: University of Chicago Press.
- Li, C. L.
1956 "The concept of path coefficient and its impact on population genetics." *Biometrics* 12 (June): 190-210.
- Marden, Parker G.
1966 "A demographic and ecological analysis of the distribution of physicians in metropolitan America, 1960." *American Journal of Sociology* 72 (November): 290-300.
- Mechanic, David
1969 "Illness and cure." Pp. 191-214 in John Kosa, Aaron Antonovsky and Irving K. Zola (eds.), *Poverty and Health*. Cambridge, Mass.: Harvard University Press.
- Miller, Herman C.
1966 *Income and Distribution in the United States*. Washington, D.C.: U.S. Bureau of the Census.
- Morris, J. N. and J. A. Heady
1955 "Social and biological factors in infant mortality." *Lancet* 1 (February): 343-9.
- Moriyama, Iwao M.
1966 "Present status of infant mortality problem in the United States." *American Journal of Public Health* 56 (April): 623-5.
1968 "Problems in the measurement of health status." Pp. 573-600 in Eleanor B. Sheldon and Wilbert E. Moore (eds.), *Indicators of Social Change*. New York: Russell Sage Foundation.
- Moustafa, A. Taher and Gertrud Weiss
1968 *Health Status and Practices of Mexican Americans*. Los Angeles: Graduate School of Business Administration, University of California, Los Angeles, Mexican American Study Project, Advance Report II.
- National Commission on Technology, Automation, and Economic Progress
1966 *Technology and the American Economy*. Washington, D.C.: U.S. Government Printing Office.
- Palmer, J.
1956 *Measuring Bed Needs for General Hospitals: Historical Review of Opinions with Annotated Bibliography*. Washington, D.C.: U.S. Government Printing Office.
- Perle, Eugene D.
1970 "Editor's introduction." *Urban Affairs Quarterly* 6 (December): 135-43.
- Richardson, William C.
1969 "Poverty, illness, and use of health services in the United States." *Hospitals* 43 (July): 34-40.
1970 "Measuring the urban poor's use of physicians' services in response to illness episodes." *Medical Care* 8 (March-April): 132-42.
- Rosenthal, Gerald D.
1965 "Factors affecting the utilization of short-term general hospitals." *American Journal of Public Health* 55 (November): 1734-40.
- Shah, F. K. and H. Abbey
1971 "Effects of some factors on neonatal and postneonatal mortality." *Milbank Memorial Fund Quarterly* 49 (January): 33-57.
- Shapiro, Samuel, Louis Weiner and Paul M. Densen
1958 "Comparison of prematurity and perinatal mortality in a general population of a prepaid group practice medical care plan." *American Journal of Public Health* 48 (February): 170-37.
- Shapiro, Samuel, Harold Jacobziner, Paul M. Densen and Louis Weiner
1960 "Further observations on prematurity and perinatal mortality in a general population and in the population of a prepaid group practice medical care plan." *American Journal of Public Health* 59 (September): 1304-17.
- Sheldon, Eleanor B. and Howard Freeman
1970 "Notes on social indicators: promises and potential." *Policy Sciences* 1 (April): 97-111.

- Stockwell, Edward G.
 1962 "Infant mortality and socioeconomic status: a changing relationship." *Milbank Memorial Fund Quarterly* 40 (January): 101-11.
 1963 "A critical examination of the relationship between socioeconomic status and mortality." *American Journal of Public Health* 53 (June): 956-64.
- Suits, D. B.
 1962 "Forecasting and analysis with an econometric model." *American Economic Review* 52 (March): 104-32.
- Sullivan, Daniel F.
 1971 A Single Index of Mortality and Morbidity. *HSMHA Health Reports* 86 (April): 347-54.
- Suchman, Edward A.
 1965a "Social patterns of illness and medical care." *Journal of Health and Human Behavior* 6 (Spring): 2-16.
 1965b "Stages of illness and medical care," *Journal of Health and Human Behavior* 6 (Fall): 114-28.
- Titmuss, Richard M.
 1943 *Birth, Poverty and Wealth: A Study of Infant Mortality*. London: Hamish Hamilton Medical Books.
- U.S. Bureau of the Census
 1963a U.S. Census of Population: 1960 Subject Reports. Persons of Spanish Surname. Final Report PC (2)—1B. Washington, D.C.: U.S. Government Printing Office.
 1963b U.S. Census of Population: 1960 Reports. Nonwhite Population by Race. Final Report PC (2)—1C. Washington, D.C.: U.S. Government Printing Office.
 1971 U.S. Census of Population: 1970. General Social and Economic Characteristics, New Mexico. Final Report PC (1)—C33. Washington, D.C.: U.S. Government Printing Office.
- U.S. Department of Health, Education and Welfare
 1969 *Toward a Social Report*. Washington, D.C.: U.S. Government Printing Office.
- U.S. National Center for Health Statistics
 1965 *An Index of Health: Mathematical Models*. Series 2, Number 5. Washington, D.C.: U.S. Government Printing Office.
 1966a *Conceptual Problems in Developing an Index of Health*. Series 2, Number 17. Washington, D.C.: U.S. Government Printing Office.
 1966b *Infant, Fetal and Maternal Mortality*. Series 20, Number 3. Washington, D.C.: U.S. Government Printing Office.
 1966c *Age Patterns in Medical Care, Illness and Disability*. Series 10, Number 32. Washington, D.C.: U.S. Government Printing Office.
 1968 *Volume of Physicians Visits*. Series 10, Number 49. Washington, D.C.: U.S. Government Printing Office.
 1970 *Vital Statistics of the United States 1968*. Volume I—Natality. Washington, D.C.: U.S. Government Printing Office.
- 1971 *Vital Statistics of the United States 1968*. Volume II—Mortality, Part B. Washington, D.C.: U.S. Government Printing Office.
- Wilcox, Leslie D. and Ralph M. Brooks
 1971 "Toward the Development of Social Indicators for Policy Planning." Paper presented at the Annual Meeting of The Ohio Valley Sociological Society, Cleveland.
- Wilcox, Leslie D., Ralph M. Brooks, George M. Beal and Gerald M. Klomglang
 1972 *Social Indicators and Societal Monitoring*. San Francisco: Jossey-Bass.
- Willie, Charles V.
 1959 "A research note on the changing association between infant mortality and socioeconomic status." *Social Forces* 37 (March): 221-7.
- Willie, Charles V. and W. B. Rothney
 1962 "Racial, ethnic and income factors in the epidemiology of neonatal mortality." *American Sociological Review* 27 (August): 522-6.
- Wold, Herman O.
 1954 "Causality and econometrics." *Econometrica* 22 (April): 162-77.
- Wold, Herman O. and L. Jureen
 1953 *Demand Analysis*. New York: John Wiley.
- Woodbury, Robert M.
 1925 *Causal Factors in Infant Mortality: A Statistical Study Based on Investigations in Eight Cities*. Washington, D.C.: U.S. Government Printing Office.
- Wright, Sewall
 1934 "The method of path coefficients." *Annals of Mathematical Statistics* 5 (September): 161-215.
 1954 "The interpretation of multivariate systems." Pp. 11-83 in O. Kempthorne, et al. (eds.), *Statistics and Mathematics in Biology*. Ames, Iowa: Iowa State University Press.
 1960 "Path coefficients and path regressions: alternative or complementary concepts?" *Biometrics* 16 (June): 189-202.
- Wrong, Dennis
 1956 *Population*. New York: Random House.
- Yankauer, Alfred
 1950 "The relationship of fetal and infant mortality to residential segregation." *American Sociological Review* 15 (October): 644-8.
- Yankauer, Alfred and Norman C. Allaway
 1958 "The relationship of indices of fetal and infant loss to residential segregation: a follow-up report." *American Sociological Review* 23 (October): 573-8.
- Yankauer, Alfred, Kenneth G. Goss and Salvatore M. Romeo
 1953 "An evaluation of prenatal care and its relationship to social class and social disorganization." *American Journal of Public Health* 43 (August): 1003.

TRENDS IN THE OCCUPATIONAL MOBILITY OF U.S. MEN, 1962-1970 *

ROBERT M. HAUSER AND DAVID L. FEATHERMAN

University of Wisconsin, Madison

American Sociological Review 1973, Vol. 38 (June):302-310

Age-specific shifts in the male occupation distribution of the U.S. from 1962 to 1970 are like those of the past several decades. There were shifts toward the ranks of salaried professionals and managers and skilled workers and away from the ranks of proprietors, laborers and farmers. These changes may be described as a shift from manual to nonmanual occupations combined with shifts from lower to higher status occupations within both the manual and nonmanual groups. Changing occupational origins accounts for a modest upgrading of the occupation distribution, while changes in mobility to first jobs have no systematic effect. The largest component of intercohort shifts in the occupation distribution is change in mobility patterns from first to current occupations. The historical trend of upward mobility among U.S. men is neither uniform nor inevitable. There was more change in occupational mobility patterns in 1962-1970 than in 1952-1962, but less than in 1942-1952. The continuation of historical trends of occupational mobility is strictly limited by the depletion of occupation groups—service workers, laborers and farmers—which have earlier served as sources of recruitment into higher status occupations.

ONLY in the past decade have satisfactory data on the rate or volume of social mobility in the United States become available. Sociologists and other observers of the American scene had long engaged in pessimistic speculation about the trend of occupational mobility (Sibley, 1942; Havighurst, 1947; Hertzler, 1952; Hollingshead, 1952) which was later countered by critical discussions (Sjoberg, 1951; Chinoy, 1955; Lenski, 1958) and by a comparison of national surveys carried out between 1945 and 1957 (Jackson and Crockett, 1964). The later evidence suggested "that no striking changes have occurred in mobility patterns and rates since World War II. . . . what movement has occurred, however, is in the direction of increasing rates of movement." (Jackson and Crockett, 1964:15).

In 1962 the Current Population Survey

(CPS) supplement, "Occupational Changes in a Generation" (OCG) carried out under the direction of Peter M. Blau and Otis Dudley Duncan, yielded the first definitive measurements of patterns and trends in occupational mobility among U.S. males. Analyses of this survey of 20,700 males aged 20-64 established that there had been substantial upward mobility in the occupational hierarchy between generations, and by an ingenious arrangement of OCG, CPS and Census data it was possible to show that more recent cohorts enjoyed greater opportunities for movement into high status occupations than their predecessors (Blau and Duncan, 1967: 90-111; Duncan, 1965). Further analyses of the 1962 data by means of age-constant intercohort comparisons have suggested that improvements in occupational opportunities in the aggregate have not been accompanied by substantial changes in the rigidity of the occupational structure (Duncan, 1968). That is, there has been no appreciable tightening or loosening of the regime connecting the occupations of men with those of their fathers.

In the past decade there has probably been as much concern about trends toward "rigidification" in American society as in any earlier period. Thus efforts to obtain a new reading on trends in oc-

* This research was supported by the National Science Foundation (Grant No. GI-31604X) and by the College of Agriculture and Life Sciences, University of Wisconsin, Madison. The authors wish to thank Otis Dudley Duncan for suggesting that we, rather than he write this paper and John M. Bregger for supplying unpublished tabulations of the Bureau of Labor Statistics, U.S. Department of Labor. Computations were carried out using facilities of the Center for Demography and Ecology by Peter J. Dickinson with the assistance of Hernando Gomez-Buendia and James R. Kluegel.

occupational mobility are surely in order. Definitive measurements of trend over the decade await the completion of a replication of the OCG survey, which is presently scheduled to be carried out in connection with the March 1973 Current Population Survey (Featherman and Hauser, forthcoming). However, by adaptation of a procedure used earlier by Duncan (1965), it is possible to obtain indirect evidence of changes in occupational mobility in the past decade.

With an early replication of the OCG survey in prospect, one may ask whether an assessment of trend by indirect methods is worthwhile at this time. We think it is. Preliminary runs from the 1973 OCG survey will not be available until late in 1974, but the discussion of recent mobility trends has already begun (Lipset, 1972). We think it desirable that the inevitable anticipations and conjectures about trends in occupational mobility be given some basis in fact. Moreover, we think our present effort has immediate methodological value in demonstrating how a continuous, if limited, monitoring of trends in occupational mobility over several decades may be based on a single baseline survey.

METHOD

Following Duncan's notation, we let $P = (p_{ij})$ be the transition matrix of an inter-generational occupational mobility table. Then, its elements represent the probability of a son's movement from the i^{th} category of father's occupation to a current occupation in the j^{th} category. Clearly, $\sum_j p_{ij} = 1.0$.

Let $A = (a_i)$ be the origin vector of the mobility table, a row vector which gives the proportion of men who originate in the i^{th} occupation class, $\sum_i a_i = 1.0$; and let $C =$

(c_j) be the vector which gives the proportionate distribution of men over destination categories, $\sum_j c_j = 1.0$. Thus, we have the

identity, $C = AP$. Likewise, we may also write $C = BQ$, where C is defined as before; while B is the vector of occupations of men in their first full-time jobs, and Q represents the matrix of transition probabilities from first to current jobs.

We use functional notation to identify the vectors and matrices of men in a given cohort observed in a particular year. Thus, $C(r,s)$ is the occupation distribution of men in the r^{th} cohort in the s^{th} year, and so on. For a selected cohort and year, then, the transition from fathers' to current occupation distributions takes the form $C(r,s) = A(r,s) P(r,s)$. From the OCG survey we have estimates of C , A , P , B , and Q for cohorts within ages 20-64 in 1962. First full-time civilian occupation and father's occupation at son's age 16 were ascertained in the OCG supplement, while current occupation was ascertained in the regular March CPS interview. In order to make inferences about changes over time in P and Q we make the following assumptions: that within the prime working ages the occupation distributions and mobility patterns of U.S. males are random with respect to mortality, net migration and movement into and out of the experienced civilian labor force and that the quality of data on current occupation, father's occupation, and first job does not vary with age or time.¹ These assumptions have two pertinent consequences. First, for men born in year r , $A(r,s+t) = A(r,s)$ and $B(r,s+t) = B(r,s)$, where t may be greater or less than zero. This says that we may use the 1962 survey to estimate the origin vectors (fathers' occupations or first jobs) observed in any year for cohorts covered in the 1962 survey. Second, the assumptions imply that it is legitimate to compare observed destination distributions across years. Thus, we can make the age-constant intercohort comparison, $C(r,s)$ with $C(r+t,s+t)$, or the intracohort comparison $C(r,s)$ with $C(r,s+t)$. Obviously, our assumptions are not perfectly met, either as to population coverage or response quality, and our inferences are subject to substantial risks of measurement error.

¹ The assumption of randomness with regard to labor force entry and exit may be relaxed if we change the population referent to all men in the civilian noninstitutional population, rather than men in the experienced civilian labor force. We have replicated our analyses with this change in definition, and it does not affect our results. The present definition permits direct comparison of our results with those of Duncan (1965).

Granting our assumptions, it becomes possible to make inferences about inter-cohort change in a mobility matrix. Consider the null hypothesis $P(r, 1962) = P(r + t, 1962 + t)$, where we have observed only $P(r, 1962)$. This says that the mobility matrix for men aged $(1962 - r)$ is unchanged t years later (or earlier). Under the null hypothesis we may write

$$\begin{aligned} C(r + t, 1962 + t) &= A(r + t, 1962 + t) \\ &\quad \times P(r + t, 1962 + t) \\ &= A(r + t, 1962 + t) \\ &\quad \times P(r, 1962), \end{aligned}$$

which we can estimate by

$\hat{C}_P(r + t, 1962 + t) = A(r + t, 1962)P(r, 1962)$, since $A(r + t, 1962 + t) = A(r + t, 1962)$ by assumption. We denote our estimate of the expected distribution here by $\hat{C}_P(r, s)$ in order to differentiate it from $\hat{C}_Q(r, s)$, the estimate based on the first job vector and the transition from first to current occupation. For example, we can estimate the 1972 occupation distribution (at age 35-44) of men born in 1927-1936 (aged 25-34 in 1962) by applying the 1962 intergeneration transition matrix of men born in 1917-1926 (aged 35-44 in 1962) to the origin vector of the younger cohort. The same logic applies to hypotheses about intercohort change in the intragenerational mobility matrix. Of course, this procedure is simply an application of the common demographic technique of indirect standardization based on the 1962 occupational mobility rates.

Comparisons among expected and observed distribution for recent years permit us to make limited inferences about change in mobility matrices in the past decade. While identity of destination vectors does not imply identity of transition matrices, differences between destination vectors clearly imply rejection of the null hypothesis (subject to the possibility that internal changes in the matrix are due solely to changes in the marginals and not at all to changes in interactions between rows and columns of the matrix).

In his 1965 paper Duncan used this procedure to measure trends from 1932 through 1962. That is, he applied the 1962 matrix for a younger cohort to the origin distribution of a cohort 10, 20 or 30 years older to obtain an expected occupation distribution

of the older cohort when it was 10, 20, or 30 years younger. Following Duncan's proposal (1965:493-4) that his procedure also be used projectively, we have applied transition matrices for older cohorts to the origin vectors of younger cohorts to obtain expected destination vectors for them in later years.

Using the destination vectors estimated from inter- and intragenerational mobility, it is possible to partition the net intercohort differences in occupation distributions for men of the same age into components attributable to intercohort changes in occupational origins, in the transition from father's occupation to first job, and in the transition from first job to current occupation. The necessary identity is

$$\begin{aligned} C(r + t, s + t) - C(r, s) &= [C(r + t, s + t) - \hat{C}_Q(r + t, s + t)] \\ &\quad + [\hat{C}_Q(r + t, s + t) - \hat{C}_P(r + t, s + t)] \\ &\quad + [\hat{C}_P(r + t, s + t) - C(r, s)]. \end{aligned}$$

The two terms in the first bracket on the right differ only because of intercohort differences in the transition matrix from first job to current occupation. That is,

$$C(r + t, s + t) = B(r + t, s + t) Q(r + t, s + t),$$

while

$$\hat{C}_Q(r + t, s + t) = B(r + t, s) Q(r, s).$$

Thus, since $B(r + t, s) = B(r + t, s + t)$ by assumption, the difference between $C(r + t, s + t)$ and $\hat{C}_Q(r + t, s + t)$ is the effect of intercohort change in the transition from first job to current occupation on the net intercohort difference. To interpret the difference in the second bracket denote the transition matrix from father's occupation to first job as $M(r, s)$.

Then

$$P(r, s) = M(r, s) Q(r, s),$$

so

$$\hat{C}_P(r + t, s + t) = A(r + t, s) M(r, s) Q(r, s).$$

Also,

$$\hat{C}_Q(r + t, s + t) = A(r + t, s) M(r + t, s + t) Q(r, s)$$

since

$$B(r + t, s) = A(r + t, s) M(r + t, s + t)$$

by assumption. Thus $\hat{C}_P(r + t, s + t)$ and $\hat{C}_Q(r + t, s + t)$ differ only because of intercohort change in the transition from father's occupation to first job, and their difference represents the effect of that change on the net intercohort difference.

Finally, $C(r,s) = A(r,s) P(r,s)$, while $C_P(r+t,s+t) = A(r+t,s) P(r,s)$, which differs from the first expression only by virtue of changes between cohorts in the vector of occupational origins. Thus, the difference between the terms in the third bracket is the effect on the net inter-cohort difference of the intercohort shift in the distribution of sons by their fathers' occupations.

Had we been limited to tabulations by standard 10-year age-breaks, our efforts would have been stymied by the fact that 1972 occupation distributions were not available when these analyses were carried out. However, since we have access to unit record tapes of the OCG survey, we have proceeded to make trend comparisons over a shorter period by varying the age-breaks in our origin vectors. Specifically, we have applied the transition matrices for those aged 35-44, 45-54, and 55-64 in 1962 to the origin vectors of those aged 27-36, 37-46, and 47-56 in March 1962 in order to generate expected distributions for men aged 35-44, 45-54, and 55-64 in March 1970. We obtained observed distributions in 1970 from the March 1970 Current Population Survey person tape. In passing we should note that with freedom to vary age-breaks in both the OCG and CPS tabulations, it is possible to make annual trend measurements at any desired ages.

NET INTERCOHORT SHIFTS, 1962-1970

The occupation distributions of men aged 35-44, 45-54, and 55-64 in 1962 and 1970 are compared in Table 1. The net intercohort shifts from 1962 to 1970 may be summarized as a fairly smooth continuation of the trends of earlier decades (Duncan, 1966). There were substantial intercohort shifts toward employment as salaried professionals and managers and smaller shifts toward employment as craftsmen, foremen and kindred workers. The former were largest at the two younger ages and the latter at the oldest age. Within the professional category there was no net shift toward self-employment; all of the net change was attributable to increases in salaried professionals. The growth among salaried managers was almost perfectly offset at each age by a substantial decline in the proportion of proprietors.² A similar, but weaker

² Friendly critics have suggested to us that the complementary net shifts between salaried and self-employed managers, officials and proprietors may be an artifact of a 1967 procedural change in the Current Population Survey which improved the quality of self-employment reports. We estimate this change of procedure could account for a shift of no more than one percent of the male occupation distribution from self-employed to salaried status within the category of managers, officials and proprietors. While our conclusions about the pattern of shifts within that category are unaffected, our numerical results probably do overstate the extent of the shifts.

Table 1. Percentage distribution by occupation and net change, 1962-1970, by age: U. S. men in the experienced civilian labor force, March 1962 and March 1970

Occupation	35-44			45-54			55-64		
	1962	1970	Change	1962	1970	Change	1962	1970	Change
Professional, technical, and kindred workers									
Self-employed	1.91	1.85	-0.06	1.51	1.59	0.08	1.71	1.55	-0.16
Salaried	10.89	14.45	3.56	7.66	10.38	2.72	7.37	8.77	1.40
Managers, officials and proprietors, exc. farm									
Salaried	9.59	13.50	3.91	8.36	13.56	5.20	9.60	11.70	2.10
Self-employed	7.62	4.15	-3.47	9.94	5.42	-4.52	10.05	5.51	-4.54
Sales workers	5.14	4.93	-0.21	5.00	4.87	-0.13	3.99	5.63	1.64
Clerical and kindred workers	6.47	6.06	-0.41	6.66	6.78	0.12	5.92	6.47	0.55
Craftsmen, foremen and kindred workers	21.16	22.77	1.61	22.56	23.45	0.89	19.51	22.53	3.02
Operatives and kindred workers	19.10	18.93	-0.17	17.68	18.84	1.16	16.10	16.82	0.72
Service workers, including private household	4.86	4.69	-0.17	6.28	5.16	-1.12	7.91	7.57	-0.34
Laborers, except farm and mine	6.96	5.25	-1.71	6.53	5.24	-1.29	6.51	5.80	-0.71
Farmers and farm managers	4.92	2.46	-2.46	6.41	3.85	-2.56	9.22	6.05	-3.17
Farm laborers and foremen	1.39	0.96	-0.43	1.41	0.87	-0.54	2.11	1.60	-0.51
Total	100.00	100.00		100.00	100.00		100.00	100.00	
Number (1,000)	11,085	10,513		9,594	10,423		6,563	7,151	

Source: March 1962 OCG survey and March 1970 Current Population Survey (person tapes).

pattern can be ascertained in net inter- and intragenerational shifts from 1952 to 1962 at younger ages in Duncan's 1965 paper on mobility trends (Table 4, p. 497). Only the decline in the percentage of farmers rivals that among self-employed managers, but the decline in the percentage of nonfarm laborers is also fairly large. The remaining categories show small downward shifts in their share of the occupation distribution.

It should be kept in mind that small percentage point shifts in the total occupation distribution imply rapid growth or decline of smaller occupational groups. For example, the decline of 2.5 percentage points in the share of men who are farmers or farm managers at ages 35-44 represents a fall of 50 percent in the proportion of men in that category.

While the March 1970 CPS estimated there were 7,151,000 men aged 55-64 in the experienced civilian labor force, the number of men 47-56 in March 1962 estimated from the OCG survey was 9,104,000. The net loss of nearly 22 percent of the cohort, due in about equal measure to retirement and mortality, is an obvious threat to our assumption of closure. Specifically, the validity of our findings for 55-64 year olds is reduced (a) insofar as labor force exits between 1962 and 1970 occurred differentially with respect to occupational origins (*not* occupations at the survey date) and (b) insofar as changes from 1962 to 1970 in occupational mobility matrices for men

in the labor force at ages 55-64 were effected by changing patterns of occupation-specific exit from the labor force. We do not think that either of these sources of invalidity could be very large, but our findings for men aged 55-64 should be interpreted with caution. In the two younger cohorts there is no *prima facie* evidence of severe violation of our closure assumption; the 1962 and 1970 estimated population totals differ by only 2.8 and 5.0 percent, respectively, for those aged 27-36 and 37-46 in 1962.

COMPONENTS OF INTERCOHORT SHIFTS

The components of intercohort change in the occupation distribution between 1962 and 1970 are shown in Table 2. The most striking feature of the table is the fact that virtually all of the net intercohort shifts in the occupation distribution are attributable to changes in the matrix of transitions from first jobs to current occupations. In no occupation group at any age is the effect of change in occupational origins or in the transition from origin to first job as large as one percentage point.

With but one exception intercohort shifts in occupational origins at each age increase the chances that a man will become a professional, salaried manager, salesman or clerical worker; and they decrease the chances of his becoming a laborer or farmer. Shifting occupational origins have virtually

Table 2. Components of intercohort change in occupation distributions due to social origins, transitions from father's occupation to first occupation, and transitions from first occupation to current occupation: U. S. men in the experienced civilian labor force, March 1962 and March 1970

Occupation	35-44			45-54			55-64		
	Origins	Father's Occ to First Job	First Job to Current	Origins	Father's Occ to First Job	First Job to Current	Origins	Father's Occ to First Job	First Job to Current
Professional, technical, and kindred workers									
Self-employed	0.13	0.36	-0.55	0.02	-0.05	0.11	-0.02	-0.05	-0.09
Salaried	0.75	0.56	2.25	0.29	0.46	1.97	0.16	-0.32	1.56
Managers, officials and proprietors, exc. farm									
Salaried	0.27	0.17	3.47	0.18	-0.16	5.18	0.19	-0.09	2.00
Self-employed	0.01	0.01	-3.49	0.00	0.41	-4.93	0.10	0.28	-4.92
Sales workers	0.21	-0.21	-0.21	0.08	-0.13	-0.08	0.17	-0.15	1.62
Clerical and kindred workers	0.21	-0.30	-0.32	0.12	-0.23	0.23	0.10	-0.23	0.68
Craftsmen, foremen and kindred workers	-0.03	-0.35	1.99	-0.01	0.39	0.50	0.21	0.29	2.52
Operatives and kindred workers	-0.30	-0.28	0.41	-0.04	-0.14	1.33	0.20	0.19	0.33
Service workers, including private household	0.00	0.03	-0.20	0.07	-0.05	-1.14	0.05	0.11	-0.50
Laborers, except farm and mine	-0.28	0.07	-1.50	-0.06	-0.11	-1.12	-0.11	0.11	-0.71
Farmers and farm managers	-0.82	0.03	-1.67	-3.58	-0.27	-1.71	-0.87	-0.18	-2.12
Farm laborers and foremen	-0.17	-0.08	-0.18	-0.08	-0.11	-0.35	-0.18	0.04	-0.37

Source: March 1962 OCG survey and March 1970 Current Population Survey (person tapes).

no impact on the likelihood that a man will become a proprietor or a service worker. Since the occupation categories are listed in an order which approximates the socioeconomic ranking of major occupation groups from top to bottom, it is fair to conclude that the overall effect of intercohort shifts in occupational origins is to produce a slight upgrading of the occupation structure. That is, the historical upgrading of the occupational structure implies a modest intercohort shift of employment from lower to higher status occupations. If recent expectations of mobility between generations are to be met in the future, there will have to be a continuing expansion of opportunities for employment in higher status occupations.

The transition from occupational origins to first jobs takes place over an interval in the life-cycle which is roughly invariant with respect to calendar time. Thus, comparisons across ages of intercohort shifts due to changes in that transition matrix represent intertemporal change. At ages 35-44 changes in the origin-first job transition matrix place more men in professional and salaried managerial jobs and fewer as salesmen, clerical workers, craftsmen or operatives; while there are virtually no effects on the proportions of proprietors, service workers, laborers, or farmers. At ages 45-54 changes in the same transition matrix place more men as salaried professionals, proprietors, and craftsmen; and fewer are placed as salaried managers, salesmen, clerical workers, operatives, and farmers; while the remaining groups are virtually unaffected. At age 55-64 shifts in the origin-first job transition matrix lead to the placement of more men as proprietors, craftsmen and operatives and fewer as salaried professionals, salesmen, clerical workers, and farmers. In light of these observations and the modest size of the observed shifts, we conclude that there are no consistent trends in the influence on the occupational structure of change in the transition matrices from occupational origins to first jobs.

Following the pattern of earlier decades (Duncan, 1965:497), net intercohort shifts in the occupation distribution are largely attributable to changes in the transition matrix from first full-time jobs to current occupations. The components due to shifts

in the transition matrix are similar across the age groups; and, of course, they are much like the net intercohort shifts described above. There are substantial positive shifts toward employment as salaried professionals and managers and as craftsmen; and there is a smaller positive shift into the operative category. There is a large shift away from proprietorship; and there are small, but consistent shifts out of the four lowest categories: service workers, farm and nonfarm laborers, and farmers. Finally, shifts involving self-employed professionals, salesmen, and clerical workers are generally small and form no consistent pattern across the age groups.

Overall, the components of intercohort change in the occupation distribution due to changes in the first job-current occupation transition matrix can be said to have increased opportunities for upward mobility. The seeming exception to this generalization, net movement out of the category of self-employed managers, may not be as much a contradiction as it appears. Proprietors are typically small businessmen, not the heads of large firms or corporations; and they have less education and lower incomes than do salaried managers. If one takes self-employment as a self-evident virtue, then he may be less sanguine about this development. The overall pattern of shifts due to change in the intracohort mobility matrices might be described as an upgrading of the occupational structure within both the manual and nonmanual sectors, accompanied by a smaller shift from manual to nonmanual occupations.

The differences between occupation distributions we have compared to form components of intercohort change are summarized using indexes of dissimilarity in Table 3. The index of dissimilarity is equal to the sum of positive percentage point differences between two distributions. It represents the percentage of cases in one distribution which would have to be shifted to a different category in order to make it identical to a second distribution. The relative sizes of the indexes on the first three lines in each row confirm our earlier observation that changes in occupational opportunities between cohorts are due primarily to changes in the transition matrix from first jobs to current

Table 3. Indexes of dissimilarity representing components of intercohort change in occupation distributions at selected ages: U.S. men in the experienced civilian labor force, March 1962 and March 1970.

Component of intercohort change	Age		
	35-44	45-54	55-64
Occupational origin	1.59	0.76	1.18
Transition from father's occupation to first job	1.22	1.26	1.02
Transition from first job to current occupation	8.12	9.32	8.71
Total intercohort change 1962-1970	9.08	10.16	9.43

Source: Tables 1 and 2.

occupations. The indexes for that transition are nearly as large as the indexes for the total intercohort comparisons, shown on the fourth line of Table 3.

By 1970 the groups at the bottom of the occupation hierarchy from which there was net outmovement during 1962-1970 contained 13.4, 15.1, and 21.0 percent of the experienced civilian labor force at ages 35-44, 45-54, and 55-64, respectively, compared to 18.1, 20.6, and 25.8 percent in 1962. By 1970 farm occupations included

only 3.4 percent, 4.7 percent and 7.6 percent of the labor force at those ages. Thus, the possibilities for continued upward mobility are limited unless there appear new patterns of movement out of occupations in the middle of the hierarchy.

LONG-TERM TRENDS

In Table 4 we present our estimates of components of intercohort occupational shifts during 1962-1970 due to changes in intergenerational and intragenerational mobility matrices along with Duncan's (1965) estimates for men aged 35-44 and 45-54 in earlier periods. Note that the intergenerational effects shown here include the effects of changes in both the occupational origin-first job and first job-current occupation transition matrices. Unfortunately, we are unable to separate self-employed from salaried professionals prior to 1952.

The indexes of dissimilarity, shown at the base of each column, suggest that net changes in the mobility matrices had a larger effect on the occupational distribution during 1942-1952 than in 1952-1962 or 1962-1970. Because the professional and managerial categories are collapsed we have obviously under-estimated the decline in net occupational redistribution from 1942-1952 to the present; but the decline, if real, is surely not monotonic. Shifts in the occupation distribu-

Table 4. Differences, in percentage points, between occupation distributions for men of specified ages produced by 1962 intergenerational and intragenerational mobility matrices and by matrices for earlier and later years

Occupation	Intergeneration mobility					Intrageneration mobility				
	35-44			45-54		35-44			45-54	
	1952-1942	1962-1952	1970-1962	1962-1952	1970-1962	1952-1942	1962-1952	1970-1962	1962-1952	1970-1962
Professional, technical, and kindred workers										
Self-employed		0.3	-0.2	-0.1	0.1		0.3	-0.6	0.0	0.1
Salaried	0.9	3.5	2.8	1.5	2.4	1.7	2.8	2.2	1.9	2.0
Managers, officials and proprietors, exc. farm										
Salaried		2.4	3.6	0.7	5.0		2.4	3.5	0.9	5.2
Self-employed	3.1	0.0	-3.5	1.0	-4.5	3.4	-0.4	-3.5	0.5	-4.9
Sales workers	-1.5	0.1	-0.4	0.3	-0.2	-1.3	0.3	-0.2	0.3	-0.1
Clerical and kindred workers	0.2	0.4	-0.6	0.6	0.0	0.5	0.7	-0.3	0.9	0.2
Craftsmen, foremen and kindred workers	3.7	-0.8	1.6	0.1	0.9	3.3	-1.0	2.0	0.2	0.5
Operatives and kindred workers	3.3	-2.0	0.1	0.0	1.2	2.7	-1.8	0.4	-0.4	1.3
Service workers, including private household	-1.1	-0.3	-0.2	-0.3	-1.2	-1.4	-0.3	-0.2	-0.2	-1.1
Laborers, except farm and mine	-3.1	-0.5	-1.4	-1.0	-1.2	-3.3	-0.4	-1.5	-1.1	-1.1
Farmers and farm managers	-3.5	-2.6	-1.6	-2.3	-2.0	-3.5	-2.3	-1.7	-2.5	-1.7
Farm laborers and foremen	-2.0	-0.5	-0.3	-0.5	-0.5	-2.1	-0.3	-0.2	-0.5	-0.4
Index of dissimilarity	(11.2)	(6.7)	(8.2)	(4.2)	(9.6)	(11.6)	(6.5)	(8.1)	(4.7)	(9.3)

Source: March 1962 OCG survey and March 1970 Current Population Survey (person tapes) and J. D. Duncan, "The Trend of Occupational Mobility in the United States," *American Sociological Review* 30 (August, 1965):497, Table 4.

tion due to changing mobility patterns are clearly larger during 1962-1970 than in 1952-1962 both at ages 35-44 and 45-54.

At age 35-44 changing mobility matrices produced more movement into professional employment during 1952-1962 than in either 1942-1952 or 1962-1970. At age 45-54 there was no clear pattern of change between 1952-1962 and 1962-1970. There has been a clear shift away from the category of managers, officials and proprietors in the past three decades. At age 35-44 there was a net shift of 3.4 percent due to changes in intragenerational mobility during 1942-1952, but no net shift during 1962-1970. The apparent explanation is a continuing net movement into the ranks of salaried managers, compensated by net movement away from proprietorship, where both sorts of changes occurred more rapidly during 1962-1970 than in the preceding decade. There have been essentially no net movements into or out of sales or clerical occupations during the period covered by Table 4.

At age 35-44 there was substantial net movement into the ranks of craftsmen and operatives in 1942-1952, and there were small net shifts away from and into those categories in 1952-1962 and 1962-1970 respectively. At age 45-54 there were essentially no shifts in the craft and operative categories due to changing mobility regimes between 1952-1962 and 1962-1970. There is a consistent pattern of net movement out of the four lowest manual occupation categories. The net shift away from the two farm categories appears to have declined continuously (along with the relative numbers in those categories) over the three decades. Shifts away from services and nonfarm labor were smaller in 1952-1962 than in the preceding decade, but the net out movement may have increased again from 1962 to 1970.

SUMMARY

Intercohort net shifts in the male occupation distribution between 1962 and 1970 are similar to those observed over the past several decades. There were shifts toward employment as salaried professionals and managers and as craftsmen, foremen and kindred workers and shifts away from employment as self-employed managers, as

laborers, and in farm occupations. In terms of the status hierarchy of occupations, these changes consist of a shift from manual to nonmanual occupations combined with shifts from lower to higher status occupations within both the manual and nonmanual groups.

For men aged 35-44, 45-54 and 55-64 we have decomposed the sources of intercohort shifts into three components: (a) changes in the distribution of occupational origins (fathers' occupations) between cohorts, (b) changes in mobility between occupational origins and first full-time occupations, and (c) changes in mobility between first jobs and current occupations. At each age we find that changing occupational origins account for a modest upgrading of the occupation distribution, while changes in mobility to first jobs produce small and unsystematic shifts in the distribution. The largest component of intercohort shifts in the occupation distribution is change in mobility patterns from first to current occupations. While the first two components each account for net shifts of 0.75 to 1.60 percent of the occupation distribution, changed patterns of intragenerational mobility account for net shifts of 8 to 9 percent of the occupation distribution. Thus, the total intercohort shifts in the occupation distribution are essentially reflections of those shifts produced by changing patterns of intragenerational mobility.

While the experience of the period 1962-1970 continues the historical tendency toward upward mobility among U.S. men, that tendency is neither uniform nor inevitable. For example, there appears to have been more change in occupational mobility patterns in 1962-1970 than in 1952-1962, but less than in 1942-1952. The continuation of historical trends of occupational mobility is strictly limited by the depletion of occupation groups—service workers, laborers and farmers—which have earlier served as sources of recruitment into higher status occupations.

Although definitive analyses of mobility trends await the outcome of the 1973 survey of "Occupational Changes in a Generation," we do not think that we have yet exhausted the usefulness of the indirect methods employed here. For example, one promising line of inquiry is based on a comparison of black

and white mobility trends using the set of components developed here; and a second attempts to identify the ways in which changes in educational attainment have affected mobility patterns.

REFERENCES

- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: John Wiley and Sons, Inc.
- Bregger, John E.
1971 "Revisions in occupational classifications for 1971." *Employment and Earnings* (February):5-8. U.S. Department of Labor, Bureau of Labor Statistics.
- Chinoy, Ely
1955 "Social mobility trends in the United States." *American Sociological Review* 20 (April):180-6.
- Duncan, Otis Dudley
1965 "The trend of occupational mobility in the United States." *American Sociological Review* 30 (August):491-8.
1966 "Occupation trends and patterns of net mobility in the United States." *Demography* 3 (Number 1):1-18.
1968 "Patterns of occupational mobility among Negro men." *Demography* 5 (Number 1):11-22.
- Featherman, David L. and Robert M. Hauser
Forth-coming "Design for a replicate study of social mobility in the United States." In K. L. Land and S. Spilerman (eds.) *Social Indicator Models*. New York: Russell Sage.
- Havighurst, Robert J.
1947 "The influence of recent social changes on the desire for social mobility in the United States." In Bryson, Finkelstein, and Maciver (eds.) *Conflicts of Power in Modern Culture*, Seventh Symposium. New York: Harper and Bros.
- Hertzler, J. O.
1952 "Some tendencies toward a closed class system in the United States." *Social Forces* 30 (March):313-23.
- Hollingshead, August B.
1952 "Trends in social stratification: a case study." *American Sociological Review* 17 (December):679-86.
- Jackson, Elton F. and Harry J. Crockett, Jr.
1964 "Occupational mobility in the United States: a point estimate and trend comparison." *American Sociological Review* 29 (February):5-15.
- Lenski, Gerhard E.
1958 "Trends in inter-generational occupational mobility in the United States." *American Sociological Review* 23 (October):514-23.
- Lipset, Seymour Martin
1972 "Social mobility and equal opportunity." *The Public Interest* 29 (Fall):90-108.
- Sibley, Elbridge
1942 "Some demographic clues to stratification." *American Sociological Review* 7 (June):322-30.
- Sjoberg, Gideon
1951 "Are social classes in America becoming more rigid?" *American Sociological Review* 16 (December):775-83.

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

ON URBAN ALIENATIONS AND ANOMIE: POWERLESSNESS AND SOCIAL ISOLATION *

CLAUDE S. FISCHER

University of California, Berkeley

American Sociological Review 1973, Vol. 38 (June):311-326

We tested the hypothesis derived from Louis Wirth (1938) that urban life, all else equal, leads to alienation by secondary analysis of three large surveys. We examined two dimensions: powerlessness, operationalized with a sense of personal competence scale, and social isolation, operationalized by scales and items reflecting a distrust of others (and interpretable as a sense of anomie). Results (1) show no association of size of Standard Metropolitan Statistical Area with powerlessness. (2) Felt social isolation (i.e., distrust) was weakly but consistently ($r \approx .07$) associated. We explore explanations for this relationship and favor one based on the social composition of the community. Also, we found that knowing one's neighbors was substantially negatively related to community size. This we attribute to the relative unimportance of proximity in the urban setting which reduces neighborhood contacts in favor of trans-local ones.

THE themes of alienation and of urban life have long been intertwined in social philosophical and sociological thought. Alienation—in all its meanings—has been tied to industrialization (Marx, 1961), “mass society” (Reisman, 1953), and the “decline of community” (Stein, 1964), all forces seemingly epitomized by the city. In keeping with this history, a recent discussion of alienation “from Marx to Marcuse” was entitled, “The Urban Alienations” (Seeman, 1971; cf Schacht, 1971, and Israel, 1971, for extended reviews of alienation).

In urban sociology, the definitive contribution to this literature is Louis Wirth's (1938) paper, “Urbanism as a Way of Life,” which partly built on the earlier work of Simmel (1905) and Park (1925). Accord-

ing to this analysis, the aggregation of great numbers of diverse people creates both the reality and the perception of individual impotence. At the same time, the protective withdrawal this environment forces the individual into and the destruction it causes to social bonds renders man isolated from, fearful of, hostile to, and manipulative of his fellow man (cf. review in Fischer, 1972).

This paper reports a preliminary empirical test of the proposition that urban life is associated with alienation. We shall deal with only two dimensions of this multifaceted phenomena: powerlessness and social isolation. Powerlessness was defined by Seeman (1959, 1972), as “the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of outcomes . . . he seeks” (1959). Social isolation was defined as “the individual's low expectancy for inclusion and social acceptance, expressed typically in feelings of loneliness or feelings of rejection or repudiation” (1972).

A reviewer's comments have brought to our attention the possibility that the measure of social isolation which we use (see below) might better be interpreted as tapping a subjective sense of anomie—a feeling that normative consensus is too weak to allow one to trust other people. The reader may prefer this interpretation to our use of social isolation. The two concepts are, of course, logically linked—a social condition of anomie

* The research reported here was supported mainly by a fellowship from the Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University. It is a revision of a chapter of a dissertation submitted to the Department of Sociology, Harvard University. The survey data used were obtained from the Inter-University Consortium for Political Research and the Institute for Social Research. Neither these institutions nor the original investigators are responsible for the present use and interpretation of the data. Supplementary research funds were provided by Public Health Service Grant MH-18625 to Lee Rainwater and by the Center for the Behavioral Sciences, Harvard. Thanks are rendered to Ronald P. Abeles, David J. Armor, Diane Barthel, Paul Burstein, Karl Deirup, Andre Modigliani and Lee Rainwater for helpful comments and assistance.

is presumably marked by the weakening of intimate social bonds. In either case, we are testing Wirth's prediction that urbanism estranges the individual. For ease of presentation, we shall use the term social isolation.

Urbanism is defined here as population concentration and is measured by the size of the individual's town or metropolitan area. This procedure inevitably submerges finer ecological distinctions among communities of the same size and within communities. However, we read Wirth's theory as dealing with the impact of precisely such macroscopic environmental variables on individuals (further discussion of this point appears in Fischer, 1972).

The specific hypotheses we shall examine are (1) the more urban (larger) a person's community of residence, the greater his sense of powerlessness, and (2) the greater his sense of social isolation (or anomie). The analysis will try to establish what *independent* effect urban life might have when associated individual-level correlates are controlled.

THE LITERATURE

Powerlessness. The behavioral impact of subjective powerlessness has been established by correlational, laboratory experimental and field experimental studies. Expectations of failure lead to inaction (cf. reviews in Rotter, 1966; Seeman, 1972).

For all this work, we know little about the distribution of this perception across the population other than that it is more frequent among those who are objectively powerless: the poor and the black. A quick examination of an extensive bibliography on Internal versus External Locus of Control, the psychologists' version of powerlessness (MacDonald and Throop, 1971, plus updates supplied by MacDonald), as well as one on alienation in general (Lystad, 1969), failed to uncover works specifically treating an urban-powerlessness association. This academic silence may indicate repeated null results. Such was the case in one limited study of small businessmen in Minnesota (Photiadis, 1967).¹

¹ Dean (1961) reports that powerlessness was

Social Isolation. Much more relevant work has been done on this alienation. It can be divided, as our data will be, into two parts: subjective perceptions of isolation and behavioral manifestations of isolation.

Regarding subjective perceptions, we note ethnographic descriptions of urban life which, contrary to the hypothesis, emphasize close, personalistic attitudes on the part of the people studied (e.g., Gans, 1962; Whyte, 1955; Young and Willmott, 1957; Epstein, 1967). At the same time, many ethnographies of rural, or, at least peasant, societies report great interpersonal distance and hostility (e.g., Foster, 1967, 1960-61; Banfield, 1958; Wylie, 1964).

On a more quantitative level, Seeman et al. (1971) found a low degree of expressed isolation in metropolitan Los Angeles; and Christie and Geis (1970:318) failed to associate urban residence with an exploitative orientation toward others. These studies fail to support the hypothesis.²

With respect to behavior or reported behavior, we mark first a demographic fact: In the United States at least, the proportion of isolated persons—those living as "primary individuals" (alone or with nonrelatives)—increases with the size of the city.³ However,

slightly lower among rural migrants than among the city-born in a sample taken in Columbus, Ohio ($r = .1$).

² The Srole (1956) anomia scale is another candidate for inclusion in this review. However, though it contains an item or two relevant to social isolation, more of the items deal with feelings of futility, meaninglessness and despair. One attempt to associate it with social involvement (Bell, 1957) showed only a weak relationship. In any case, studies with this scale have had mixed and largely negative results in correlating it to urbanism (e.g., Nelsen and Witt, 1972; Mizruchi, 1969; Killian and Grigg, 1962; Nelsen and Frost, 1971).

³ The author's analysis of the 1960 Census One-in-a-thousand sample showed that this pattern was not a function of size of metropolitan area, but solely a function of the size of the central city. A one-fourth subset of heads of households under the age of sixty-three had the following distribution of persons living alone or with a non-relative:

	Outside S.M.S.A.	S.M.S.A.	S.M.S.A. of 1,000,000+
Ring of center city	5.2% (N=1,651)	6.5 (1,425)	6.2 (1,925)
Center city	10.1 (1,915)	13.9 (1,734)	18.7 (1,976)

the bulk of ethnographic and survey research on urban samples, emphasizes the deep involvement of city residents with family and friends (e.g., Axelrod, 1956; Gans, 1962; Gutkind, 1965; Greer, 1962; Bell and Boat, 1957). Comparative studies are rare but generally report few urban-rural differences in social involvement (Reiss, 1959; Sutcliffe and Crabbe, 1963; Bultena, 1969; Wright and Hyman, 1958; Curtis, 1971). Two exceptions are noted: Key (1968) found participation to be curvilinearly related to community size in a manner interacting with type of participation. Guterman (1969) discovered New York hotel employees to report less depth in their personal ties than did hotel workers in smaller cities.

Though urban-rural differences are weak if existent, strong effects of residence have been found in one type of comparison: city versus suburb. Neighboring is greater in suburbs, even when controls for other variables are applied (Tallman and Morgner, 1970; Tomeh, 1964; Fava, 1959).

Our review suggests that these hypotheses have yet to be tested in large-scale samples. This paper begins that task by examining previously collected survey data.

DATA AND MEASURES

Three surveys were used: (1) the 1968 Survey Research Center Election Poll, including a double-sample of black respondents; (2) the 1971 wave of the "Income Dynamics" survey. This pool was taken of heads of households under sixty and includes a heavy over-sample (by about 50%) of poor families, especially the urban poor (I.S.R., 1971); (3) the Almond and Verba (1963) Five-Nation Study, of which only the United States and United Kingdom samples were used.⁴

The dimensions of alienation were operationalized in the following way:

(A) *Powerlessness*. Two reversed versions of the Institute for Social Research "Personal Competence" scale (Robinson and

Shaver, 1968:102-5) were used. The S.R.C. survey included the following items:

(1) "Do you think it is better to plan your life a good way ahead, or would you say life is too much a matter of luck to plan ahead very far?"

(2) "When you do make plans ahead do you usually get to carry out things the way you expected, or do things usually come up to change your plans?"

(3) "Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been sure about it?"

(4) "Some people feel they can run their lives pretty much the way they want to; others feel that the problems of life are sometimes too big for them. Which one are you most like?"

The items were scored with the efficacious response low, the powerless one high and the others in the middle. The proportion of powerless responses ranged from 27% to 61% in this sample, an average of 42%. The items were standardized and summed to form a scale. Interitem correlations yielded a reliability coefficient (Chronbach α) of .64.

The Income Dynamics survey included items 2 and 3, plus

(5) "Are you the kind of person that plans his life ahead all the time or do you live more from day to day?"

(6) "Do you think a lot about things that might happen in the future, or do you usually just take things as they come?"

In this sample, inefficacious responses ranged from 36% to 57%, an average of 46%. Scale α was .52.

(B) *Social Isolation (Anomie)*. Subjective sense of isolation was measured by variations of the Rosenberg (1956) "Misanthropy" Scale—termed "Trust" and "Faith in People" scales by Robinson and Shaver (1969:529-630). The S.R.C. data include:

(1) "Would you say that most of the time people try to be helpful or that they are mostly looking out for themselves?"

(2) "Do you think that most people would try to take advantage of you if they got a chance or would they try to be fair?"

(3) "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?"

The items were scored from trustful to "depends, don't know" to distrustful. The proportion of distrustful responses were

⁴ The S.R.C. Election Poll and the Five Nation Study were obtained from the Inter-University Consortium for Political Research. The Income Dynamics survey was obtained from the Institute for Social Research by Professor Lee Rainwater on a grant from the Public Health Service, MH-18625.

40%, 32% and 45%, respectively. Scale α was .74.

Variations on these three questions, plus two more, appeared in the Five-Nation Study:

(1) "Would you say that most of the time people are more inclined to help others, or are more inclined to look out for themselves?"

(2) "If you don't watch yourself, people will take advantage of you. Do you agree or disagree with that?"

(3) "Some people say most people can be trusted. Others say that you can't be too careful in your dealing with people. How do you feel about that?"

(4) "No one is going to care much what happens to you when you get right down to it (agree-disagree)."

(5) "Human nature is fundamentally cooperative (agree-disagree)."

The questions were scored, standardized and summed as in the previous scales. Reliabilities were low, especially for Germany ($\alpha = .42$), Italy (.27) and Mexico (.26). Only the United States and United Kingdom had minimally acceptable α 's (.60 and .53).⁵ Isolated answers ranged from about 40% to 75% for both nations, except for item 5 which was about 13% (U.S. average, 45%; U.K. average, 46%).

The sense of incompetence and the distrust scales appear together in the S.R.C. survey. They correlate at .32—an association common in the literature (e.g., Dean, 1961).

A few additional single items on social involvement were used and will be introduced in the discussion of results.

Statistical analyses were performed with the DATA-TEXT package (Armor and Couch, 1972).

RESULTS

Powerlessness. An unweighted means analysis of covariance was performed on the reversed S.R.C. Personal Competence Scale. Two factors were crossed: Size of Standard Metropolitan Statistical Area \times Center/

Ring—whether the individual lived in the center city or in the suburbs. (For persons outside an S.M.S.A. this meant living in a town—center—versus a rural area—ring.) The results of this analysis are shown in Table 1, Part A.

The data indicate essentially no effect of place of residence on sense of competence. However, to test the Wirthian hypothesis adequately, we should take into account variables such as class which might be masking an urban-powerlessness association. In fact, since the Wirthian theory explains alienation in terms of the direct experience of urban life per se (Fischer, 1972), all individual attributes other than the alienations themselves should be held constant. Part B of Table 1 reports the results of adjusting the data for a set of covariates. These controls were age, sex, race, class, religion, stage in the life-cycle and Southern residence.⁶ The outcome was that size of S.M.S.A. as an independent effect missed statistical significance. People outside S.M.S.A.'s were only slightly less powerless than were others. Substantive significance was lacking as well. S.M.S.A. accounted for only 0.36% of the total variance and 2.4% of the explained variance. The S.R.C. data do not support the hypothesis.

We replicated the analysis with the Income Dynamics data. Here, S.M.S.A. size was crossed by a factor representing the family's relative national rank on net income. Since the unweighted means analysis of variance equalized cell N 's, we were thereby partly adjusting the data for the large sample of poor families. The results are shown in Table 2.

The results were, again, essentially negative. The S.M.S.A. effect was significant in the predicted powerless direction, but accounted for less than $\frac{1}{4}$ of 1% of the total variance and only 2.7% of the explained

⁵ A great problem in the scale is acquiescence. When, for example, item #5 is removed, the α 's for Italy and Mexico jump to .41 and .36. Analysis of the scale suggests that despite its problems, it probably provides an unbiased estimate of the construct (author, unpublished paper). However, the low reliabilities in the non-English-speaking nations could make results difficult to interpret. So, they were dropped.

⁶ These controls are straight-forward, except perhaps for Southern residence; for it could be argued that regional differences are a function of urban-rural differences. Southernness was controlled because, consistent with the canons of conservative hypothesis-testing, it subjects the urban variable to the more stringent test; and because the autonomy of regional differences seems historically supported. In any case, further analysis revealed that the overall conclusions would not have been changed even if Southernness has not been controlled.

Table 1. Powerlessness: Reversed Personal Competence Scale (S.R.C. 1968) by Size of S.M.S.A., by Ring/Center--Before and After Adjustment for Covariates (Unweighted Means Analysis of Covariance)

A. Before Adjustment				
Place in S.M.S.A.	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Ring	M = 2.78 SD = (1.37) N = (349)	2.78 (1.19) (213)	2.70 (1.24) (200)	2.76
Center	2.67 (1.32) (278)	2.87 (1.36) (244)	2.94 (1.21) (157)	2.83
Unweighted Mean	2.73	2.83	2.82	2.79
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	0.82	.439	0.11
B. Ring/Center	1	1.01	.315	0.07
A X B	2	2.08	.126	0.29
Total	1440			
B. After Adjustment				
Place in S.M.S.A.	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Ring	M = 2.67	2.85	2.94	2.82
Center	2.66	2.82	2.80	2.76
Unweighted Mean	2.67	2.84	2.82	2.79
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	3.00	.051	0.36
B. Ring/Center	1	0.86	.355	0.05
A X B	2	0.36	>.500	0.04
Covariates ^a	11	21.88	<.001	14.39
Total	1440			

^aRace, sex, age, Protestantism, social class (a scale composed of education, occupation and income), Southern residence, life-cycle stage (five dummy variables).

variance. After covariates were controlled, no S.M.S.A. effect was left.

Regression analyses were also performed on this data, the results of which are summarized in Table 3. The table displays the zero-order r , the partial r after controlling for many individual-level attributes and the contribution of community size to the explained variance. The findings are strikingly parallel in the two surveys and suggest that

there was no association between powerlessness and urbanism as measured here.⁷

Social Isolation. Versions of the Rosenberg Misanthropy Scale were used to measure the individual's sense of isolation from others (sense of anomie, perhaps). The results of analyses of covariance on the scale

⁷ The reader may be curious about the effect of low scale reliability on the results. It was to check on that problem that we calculated proportion of

Table 2. Powerlessness: Reversed Personal Competence Scale (Income Dynamics, 1971)^a by Size of S.M.S.A., by Income^b--Before and After Adjustment for Covariates (Unweighted Means Analysis of Covariance)

A. Before Adjustment				
Net Income	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Lowest 30% of Nation	M = 3.36 SD = (1.17) N = (649)	3.30 (1.13) (593)	3.41 (1.18) (921)	3.36
Middle 40%	2.77 (1.16) (526)	2.77 (1.12) (503)	2.95 (1.22) (651)	2.83
Highest 30%	2.40 (1.06) (261)	2.40 (1.10) (332)	2.53 (1.09) (393)	2.44
Unweighted Mean	2.84	2.82	2.96	2.88
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	6.07	.003	0.23
B. Income	2	221.98	<.001	8.40
A X B	4	0.50	>.500	0.03
Total	4833			
B. After Adjustment				
Net Income	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Lowest 30%	M = 3.34	3.25	3.25	3.28
Middle 40%	2.82	2.36	2.94	2.87
Highest 30%	2.41	2.45	2.57	2.48
Unweighted Mean	2.86	2.85	2.92	2.88
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	1.41	.244	0.05
B. Income	2	128.35	<.001	4.66
A X B	4	1.56	.181	1.13
Covariates ^c	8	26.79	<.001	3.89
Total	4833			

^aThis sample is heavily weighted with the urban poor and is composed of heads of households under 60 years of age.

^bThe measure used for income was termed "well-offness" (I.S.R., 1971). It reflects the family's net income as a ratio of its needs and includes the leisure time taken by family adults. The categories are calculated on the basis of weighting so that they are representative, unlike the sample itself.

^cAge, sex, minority status (black or Latin), South, life-cycle stage (four dummy variables).

Table 3. Powerlessness: Reversed Personal Competence Scale--Zero-Order and Partial Correlations with Community Size,^a by Survey

Community Size	Zero-Order r	Partial ^b r	% of Explained Variance ^c
S.R.C., 1968 (N=1440)	.023	.039	1.0
Income Dynamics (N=4833)	.027	.037*	0.9

* $p < .05$.

^aCommunity Size was measured, for the S.R.C. survey, by a ten-point scale representing city size nested within size of S.M.S.A.; for Income Dynamics, by a six-point scale representing the largest city in the respondent's area: under 10,000/10-25,000/25-50,000/50-100,000/100-500,000/500,000+.

^bControls for S.R.C. were: race, sex, age, Protestant, Jewish, father's occupation, education, occupation, income, life-cycle stage (five dummy variables), raised in South, residing in South.

Controls for Income Dynamics were: age, minority status, education, income, occupation, Protestant, life-cycle stage (four dummy variables), size of family, residing in South.

^cProportion of the explained variance which community size provides when entered into the regression equation with control variables.

are presented in Table 4 for the S.R.C. survey, and in Table 5 for the Almond and Verba Survey.

These data show a mild association in the predicted direction once confounding variables are controlled. The zero-order figures, before adjustment, indicate curvilinearity and complexity, particularly in the Center/Ring X S.M.S.A. interaction (Table 4, Part A). Both these effects were probably due to affluent suburbanites; and, consistent with this suggestion, when covariance adjustments were made (Part B of Tables 4 and 5), they disappeared. In their place appeared significant effects of community size. The major difference in the community sizes appears to have been between non-metropolitan and metropolitan places, with people in the latter slightly more distrustful.

Two individual questions from the Income Dynamics Survey were also examined: (1) "Do you trust most other people, some, or very few?"; (2) "How much does it matter

explained variance. Beyond that, we ran some regression analyses in the S.R.C. data, correcting for attenuation. The correction raised the zero-order r from .023 to .029 and had comparable positive but small effects on the partial results. In the Income Dynamics data, we examined each item separately. For no item was the zero-order r greater than .067, the partial greater than .037 or the proportion of explained variance greater than 1.4%.

what other people think about you?" (scored from "a lot" to "it doesn't matter"). These items were also mildly related to urbanism. Analyses of covariance not shown here revealed, after adjustment, significant monotonic effects of S.M.S.A. size—i.e., regular though quite small increases in an isolated direction (0.17% and 0.15% of the total variances, respectively).

As before, regressions were run in which a greater number of controls were used. The results are indicated in Table 6. And, as before, the partial r 's showed a good deal of consistency. They indicate, as did the analyses of variance, that there was an association with community size, though a very small one (on the average, independently explaining about $\frac{1}{2}$ of 1% of the variance).⁸

No striking or meaningful interactions were found—including one predicted by the urban sociology literature: that formal organizational membership in cities counteracts alienation.

Social Involvement. One would expect there to be behavioral concomitants of a sense of social isolation; and, if the present

⁸ Correcting for attenuation in the S.R.C., 1968, survey, raised the zero-order r to .04, the sixteenth-order partial to .08 and the urbanism contribution to 4.0%.

Table 4. Social Isolation: Misanthropy Scale (S.R.C., 1968) by Size of S.M.S.A.--Before and After Adjustment for Covariates

A. Before Adjustment				
Place in S.M.S.A.	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Ring	M = 2.57 SD = (1.55) N = (348)	2.81 (1.61) (213)	2.30 (1.53) (200)	2.56
Center	2.33 (1.50) (278)	2.85 (1.57) (244)	2.93 (1.64) (157)	2.70
Unweighted Mean	2.45	2.83	2.62	2.63
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	6.85	.002	0.93
B. Ring/Center	1	2.84	.093	0.19
A X B	2	9.11	<.001	1.24
Total	1439			
B. After Adjustment				
Place in S.M.S.A.	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Ring	2.42	2.84	2.67	2.64
Center	2.28	2.76	2.83	2.62
Unweighted Mean	2.35	2.80	2.75	2.63
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	11.28	<.001	1.34
B. Ring/Center	1	0.06	>.500	0.00
A X B	2	1.24	.289	0.14
Covariates ^a	11	20.44	<.001	13.32
Total	1439			

^aCovariates were race, age, sex, Protestant, social class (education, occupation, and income), region, and life-cycle (five dummy variables).

hypotheses be true, those behaviors should be affected by urban residence.

The Almond and Verba data include information on the number of organizations to which respondents belonged. In analysis of this survey and a review of other studies, Curtis (1971) concluded that there was no relationship between community size and membership in voluntary associations. Our examination of this material also showed no

significant effects, before or after covariance adjustment. The partial r was, in the United States, $-.067$ ($p < .05$; 1.9% of the explained variance) and $-.019$ (N.S.) in the United Kingdom.⁹

⁹ Formal associations are ambiguous phenomena for the Wirthian model. On the one hand, they reflect social involvement—non-alienation. On the other, they are formal means of integration, presumably outgrowths of anomie.

Table 5. Social Isolation: Misanthropy Scale (Almond and Verba Survey) by Size of Town--Before and After Adjustment for Covariates

A. Before Adjustment				
Nation	Size of Town			
	Under 5,000	5,000-100,000	Over 100,000	Unweighted Mean
United Kingdom	M = 2.57 SD = (.56) N = (240)	2.52 (.56) (299)	2.63 (.62) (424)	2.58
United States	2.64 (.62) (296)	2.59 (.63) (243)	2.63 (.61) (422)	2.62
Unweighted Mean	2.61	2.56	2.63	2.60
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. Town Size	2	2.45	.086	0.25
B. Nation	1	2.16	.142	0.11
A X B	2	0.71	.490	0.07
Total	1932			
B. After Adjustment				
Nation	Size of Town			
	Under 5,000	5,000-100,000	Over 100,000	Unweighted Mean
United Kingdom	2.50	2.48	2.60	2.52
United States	2.64	2.65	2.71	2.67
Unweighted Mean	2.57	2.57	2.65	2.60
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. Town Size	2	4.59	.011	0.42
B. Nation	1	25.29	<.001	1.17
A X B	2	0.43	>.500	0.04
Covariates ^a	11	21.79	<.001	11.07
Total	1932			

^aCovariates were status (scale composed of education, spouse's education, income, interviewer rating), occupation, age, sex, region (poor versus rich), Protestant, life-cycle (five dummy variables).

A better test of the hypothesis would be the degree to which the individual actually participated in organizations. Such a question was asked in the Income Dynamics Survey: "How often do you go to social clubs or organizations" (Scored never/less than once per week/once per week or more; 66% reported "never"). The following question was also asked: "About how many peo-

ple in this neighborhood do you know by name?" (Scored, 0-2/3-5/6-9/10-19/20+ or "All"; 42% fell into the last category.) These two probes are, on a prima facie basis, indications of social involvement. (They also correlate, though at a low level, with the distrust item on the survey, at -.129 and -.166 respectively.)

Table 6. Social Isolation: Misanthropy Scales and Other Items: Zero-Order and Partial Correlations with Community Size,^a by Survey

Community Size	Zero-Order r	Partial ^b r	% of Explained Variance
A. S.R.C., 1968 "Misanthropy"	.037	.079**	3.9
B. Almond and Verba "Misanthropy"			
United Kingdom	.073*	.074*	5.9
United States	-.012	.053	1.7
C. Income Dynamics			
"Distrust" Item	.136***	.064***	1.7
"What Others Think" Item	.063***	.033*	1.4

*p < .05; ** p < .01; ***p < .001.

^aFor community size scales in the S.R.C. and Income Dynamics Surveys, see Table 3. In the Almond and Verba Survey: a five-point scale for size of town--under 5,000/5-20,000/20-50,000/50-100,000/100,000+.

^bControls were the same as in Table 3 for S.R.C. and Income Dynamics. For Almond and Verba: race (in the U.S.), age, sex, Protestant, social status (scale composed of education, spouse's education, income and interviewer rating), occupation, life-cycle stage (five dummy variables) and region of nation (rich versus poor).

Analyses of variance are presented in Table 7 for these two items. The covariance adjustments are not presented because they left the patterns essentially unchanged.

The social club results are not as predicted. The item has no overall association with S.M.S.A. But, it did show an interesting interaction: low income persons outside S.M.S.A.'s had the lowest attendance (the r between S.M.S.A. and attendance for low income people is +.108, p < .001). At the same time, high income non-S.M.A. persons had the highest attendance (the r for the high income group is -.082, p < .05). A plausible explanation exists for why non-urban poor people attended meetings less than the urban poor--distances and lack of resources to overcome them. At the same time, the negative association among the well-to-do might reflect urban alienation. However, the correlation is weak and there remains the absence of correlation among the middle income group (r = .042, N.S.). On the whole, this item provides little support for the hypothesis (overall r = .034, partial r = .001).

That is not the case, however, for the "neighbors" item (Table 7, Part B). There is a strong relationship such that the larger the metropolitan area, the fewer neighbors a respondent claimed to know (overall r = -.282; partial r = -.206; percent of ex-

plained variance = 29.2). This was true within all subgroups examined. For example, in the white, middle-income group, the proportion claiming to know only 0, 1, or 2 neighbors increased from 6.9% in non-S.M.S.A. areas to 12.7% in large S.M.S.A.'s; and those reporting 20+ or all neighbors decreased from 58.8 to 38.8%. Among low-income Southerners, the percentages are 3.6 to 16.6 and 70.6 to 31.4%. Further examination of the data indicated that the effects would have been even stronger had the suburbs, which as previous research suggests are more neighborly, been separated out. (The reader will note that the standard deviations of the item increase with S.M.S.A. size. This is perhaps testimony to the residential heterogeneity of metropolitan areas.) The strongest association in this paper is, then, that the larger the metropolitan area, the fewer neighbors an individual claimed to know. This finding is consistent with one reported by Key (1968) that "neighboring" declined from the rural to the more urban places in his Midwestern sample and also with results of a Swedish study comparing town residents to those in its rural hinterland (Swedner, 1960).

A similar pattern of results was obtained with an item asking whether or not the respondent had family within walking distance of home. The larger the S.M.S.A., the

Table 7. Social Involvement: Social Club Attendance and Number of Neighbors Known (Income Dynamics Survey) by S.M.S.A., by Income

A. Frequency of Club Attendance ^a				
Net Income ^b	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Lowest 30% of Nation	M = 1.23 SD = (0.51) N = (642)	1.33 (0.60) (587)	1.33 (0.60) (901)	1.30
Middle 40%	1.44 (0.64) (517)	1.46 (0.64) (500)	1.47 (0.67) (638)	1.46
Highest 30%	1.82 (0.82) (256)	1.68 (0.74) (327)	1.66 (0.72) (386)	1.72
Unweighted Mean	1.50	1.49	1.49	1.49
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	0.07	>.500	0.00
B. Income	2	152.13	<.001	5.99
A X B	4	6.48	<.001	0.51
Total	4753			
B. Number of Neighbors Known Scale ^a				
Net Income ^b	Community Size			
	Outside S.M.S.A.	S.M.S.A.	Large S.M.S.A.	Unweighted Mean
Lowest 30%	M = 4.27 SD = (1.19) N = (642)	3.46 (1.47) (590)	3.08 (1.52) (913)	3.61
Middle 40%	4.13 (1.27) (524)	3.54 (1.43) (500)	3.24 (1.52) (650)	3.64
Highest 30%	4.23 (1.18) (259)	3.90 (1.22) (331)	3.69 (1.34) (391)	3.94
Unweighted Mean	4.21	3.63	3.34	3.73
Factor	Analysis of Variance Table			
	df	F	Significance	% of Variance
A. S.M.S.A.	2	143.97	<.001	5.58
B. Income	2	47.83	<.001	0.97
A X B	4	13.12	<.001	.53
Total	4799			

^aFor scoring on scales, see text.^b"Income" defined in Table 2.

less likely this was to be the case (ANOVA: Regression: $r = -.118$; partial $r = -.115$; $F = 30.97$, $p < .001$, 1.24% of total variance. 17.1% of explained variance). This effect

was somewhat inflated by suburbanites who, while knowing their neighbors more, were less likely to include relatives among them. The finding is similar to one reported by Klatzky (n.d.:55), that men in small communities were more likely to live near their parents than were men in large cities.

At this point, we recapitulate the basic results:

(1) Community size and powerlessness were not associated.

(2) There was a very small association between community size and sense of social isolation.

(3) With respect to social involvement, presumably both cause and effect of social-psychological isolation, the findings were contradictory: Attendance at social meetings was unrelated, but knowing one's neighbors was strongly related to urbanism—as was having relatives among those neighbors. The more metropolitan, the fewer know neighbors and the fewer have relatives nearby.

DISCUSSION

The negative findings on powerlessness call for little discussion. This fundamental dimension of personality, in all its names—sense of efficacy, internal locus of control, fate control—seems unaffected by urbanism. Two cautions should be noted: (1) This conclusion rests on two surveys, but on one scale measuring sense of competence. Replication with other measures is necessary. (2) This finding pertains to America, circa 1970. There are sound theoretical arguments (e.g., Sjoberg, 1964; Greer, 1962) to suggest that urban-rural differences—though intrinsically real—are erased in modern, mass societies. These arguments are challenged by findings that such differences persist (e.g., Nelsen, Yokely and Madron, 1971), but the arguments call for cross-cultural replication.

We will deal here with two questions: (1) Given the tenuous association of urbanism with social psychological isolation (partial $r = .064$, 1.7% of explained variance, Income Dynamics Survey)—and the lack of an urbanism-isolation finding in the literature—how is it that knowing one's neighbors was highly related (partial $r = -.206$, 29% of explained variance)? (2) How can the small urbanism-sense of isolation association be explained?

Two ready answers exist for the first

question: (a) that the neighbors finding was in only one survey with an unusual sample. However, it does replicate Key (1968) and the strength of the correlation makes this a weak response; (b) that subjective psychological scales are simply too unreliable and invalid (cf. Phillips and Clancy, 1972); that the neighbor item is the one to be believed, the scale to be ignored.

The interpretation we favor is a third: that both the results are valid—sense of isolation is only marginally associated with urbanism, but non-neighborhood is much more so. The argument forwarded is that the results reflect the relative freedom from spatial constraints available in the urban environment—that people in the city can choose their friends and associates from within or without the neighborhood as they wish, making of the locality a “community of limited liability” (Janowitz, 1967). One can choose to be locally anonymous, and yet still *not be isolated* because of the availability and ease of contacts outside the immediate area. On the other hand, the rural situation implies that, through lack of alternatives, neighbors are associates; and through absence of anonymity, one knows one's neighbors—willing or not. “The sociology of village life makes neighboring mandatory. In cities this type of neighboring . . . is mandatory no longer” (Keller, 1968:48). The conclusion of this argument is that there may be a rural-urban difference in neighboring, but little, if any, in total real and felt social isolation.

What evidence is there for this explanation? (1) When physical distance is controlled, city size does not affect frequency of contact with kin (Klatzky, n.d.), and even physical distance is unrelated to emotional ties to kin (Litwak, 1960)—evidence of the nil effect of urbanism and of the possibility of transcending space. (2) Studies of social ties among urban residents find that such bonds do exist, but mostly outside the local area (e.g., Lansing: Smith et al., 1954; San Francisco: Bell and Boat, 1957; Los Angeles: Reimer and McNamara, 1957; Cairo: Petersen, 1971; Toronto: Wellman et al., in press; cf. review in Greer, 1962:89 ff.). (3) Key (1968) found, that even as neighboring declined from rural to urban places, other forms of social contact, including “informal groups,” increased (cf. Swedner,

1960). (4) We can point to some evidence in the present data, as well:

(a) The lack of association between S.M.S.A. and the social club item is similar to the distinction Key and Swedner found between neighboring and other social involvements. With increasing urbanism, the first can drop and the latter still remain constant (see discussion in Keller, 1968:Ch. 1).

(b) If neighboring does become more a matter of free choice in the urban setting, then one should expect to find a higher association between psychological dispositions and neighboring in urban than in rural places. In terms of our data, the correlation between social psychological isolation (the "how many people do you trust" item) and neighboring should increase with community size, because in the larger communities the distrustful are freer to stay aloof.¹⁰

This was indeed the case: The relationship between the two items increased with increases in urbanism. The correlation between distrust and neighboring was $-.06$ among non-S.M.S.A. persons ($b = -.09$), $-.14$ and $-.17$ among S.M.S.A. and large S.M.S.A. respondents ($b = -.23, -.30$). The latter two slopes are greater than the first one at $p < .002$ and $p < .001$. (This interaction did not exist among low-income persons, and was strongest among middle-income respondents.) These results reflect the following pattern, for example: In the middle-income group living outside S.M.S.A.'s, there was little difference in the proportion "locally-isolated" (knowing less than three neighbors) by their level of trust. 5.4% of the most trusting were "locally isolated"; 7.6% of the least trusting were. In large S.M.A.S.'s, differences appeared. Among the most trusting 11.5% were "locally isolated"; among the least trusting, 26.1% were. Seemingly, the larger the metropolis, the freer are the misanthropic to ignore their neighbors.¹¹

¹⁰ We make here a causal assumption that psychological orientation leads to involvement, an assumption that seems justified by the alienation research (Seeman, 1972). Obviously, though, experience with involvement reciprocally affects feelings—an assumption that our previous analysis was based on.

¹¹ Consistent with this analysis is the finding that no such S.M.S.A. interaction appeared in the distrust-social club attendance correlations. (Attendance is equally voluntary everywhere.) Also, there was such an interaction in the club attendance-neighboring correlations, because, we argue, the

(c) Consistent with this hypothesis of freedom from proximity, the likelihood of having family within walking distance declined with urbanism. Previous studies (e.g., Reiss, 1959; Bell and Boat, 1954; Key, 1968; Bultena, 1969) indicate that family contacts are at least as common in cities as in the countryside. The present results suggest that those contacts are translocal. Furthermore, if it is true that both friends and family are equally present but are more dispersed over space in cities, then one should expect the overlap of relative and neighbor to be smaller in cities. This was the case. The correlation between having relatives nearby and number of neighbors known was $.22$ in non-S.M.S.A. places, $.12$ in large S.M.S.A. areas. (The difference in slopes, $.53$ versus $.37$, was significant at $p < .01$.) Both relatives and friends are dispersed in the urban setting (cf. Wellman et al, in press).

We conclude on the basis of these arguments that total social isolation is only weakly if at all associated with urbanism, but that the contacts people have, become freer of the locality the more urban the place.

(These findings also support some theoretical arguments [e.g., Frankenburg, 1965] that role differentiation increases with urbanism. In this case, the concordances of friend-neighbor and relative-neighbor decrease with increases in community size.)

The second finding to be discussed is that felt social isolation (as measured by items reflecting suspicion and distrust) was related to urban residence. This result is, to be sure, quite marginal, with the independent effect running at about $.07$ ($R^2 = .005$; percent of explained variance = 3–5%). To change feelings of isolation, adjusting population concentration would be a crudely inefficient procedure. However, with respect to our theoretical concerns, explanations of even this small effect deserves attention.

The simplest explanation is that it reflects the contemporary "urban crisis" of crime; it may be a transient effect. But, if we choose to consider the findings as more generalizable than that, we can examine at least three explanations, the first two drawn from Wirthian theory (Wirth, 1938; Fischer, 1972).

voluntarism of local interaction increases with S.M.S.A. size.

(1) The intense demands of urban life which result from the activity of densely-packed multitudes lead the individual to withdraw from contact to protect himself (Simmel, 1905; Milgram, 1970). Social distance and anti-social attitudes could be such defensive mechanisms. This explanation posits no mediation between the city environment and individual psychology.

(2) Urban life is anomie. That is, social ties (such as kinship) are weakened and, with them, social norms. In this environment, where the rules of behavior have broken down, a sense of distrust and isolation is both realistic and inevitable. This explanation posits the breakdown of institutions as the mediating variable between urbanism and alienation. (However, the studies we cited in discussing dispersal cast doubt on the assumption that there is such an anomie state in urban life.)

(3) Another explanation focuses on the social context of the urban environment. The argument is that cities are disproportionately the site of social groups whose lifestyles are strange to each other. Their presence may generate a sense of distrust for "most people" even while the individual may be comfortably and fully a member of his own networks (Fischer, 1973). This point is somewhat like Wilson's (1970) argument that the source of "urban unease" is the public misbehavior (serious or otherwise) of other people.

Evidence for this last argument would be that attributes of the individual's social environment (i.e., characteristics of the surrounding population) explain the urbanism-distrust correlation. Some data from the S.R.C. 1968 survey hint that this might be the case. When a factor scale measuring the S.E.S. level of the person's county¹² was entered into the regression equation—with all individual attributes held constant—the partial r between community size and the misanthropy scale dropped from .079 to .041. (County S.E.S. was *positively* correlated with misanthropy after sixteen individual-level controls. Why this is so is unclear.) Then, entry of a factor scale reflecting the presence of "primary individuals"¹³ reduced

it to .027, and entry of the county's homicide rate (in 1967) reduced it further to .008. (The multiple partial R for the three county variables is .12, $p < .001$). This mildly substantiates the thesis that the social composition of the urban environment, rather than the environment directly, shapes alienation.

At least two objections can be made to this analysis. (1) These "contextual" variables are simply urbanism measured in different form. However, when community size was controlled, the multiple partial R for the three are still .09 ($p = .01$), indicating that they represent more than dummies for size and are more effective as predictors than size. (2) The effects of controlling for "isolates" and the homicide rate are testimony to the anomie explanation above. Cities isolate people, increase crime, and thereby generate a sense of estrangement. This reply ignores the major effect of the county S.E.S. control, but is otherwise consistent with the findings. It pushes the issue back to explaining the source of the correlation between urbanism and the proportion of primary individuals (does urbanism cause their increase, or is it self-selection?) and the urbanism-homicide correlation (does urbanism cause it, or is it an accident of population distribution?). These issues are beyond the present data.

CONCLUSIONS

Survey data examined here indicate that no association exists between size of community and sense of personal incompetence. A real but small association was found between urbanism and a sense of social isolation, as indicated by distrust of "most people." Three explanations for this finding were proposed, two of which seem plausible—that urban anomie or that social composition generates distrust. We favor the latter explanation. Finally, some of the data indicate that, while urbanism may not isolate individuals from social contact, it may incline them to shift that contact away from the locality.

These conclusions call into question the intellectual folk-myths concerning the alienation of urban life. Fundamental personality

percent unmarried females, number of people per household, and number of rooms per house (the latter two negatively weighted). ($\alpha = .78$.)

¹² Composed of median education (1960), median rent (1970), percent of housing units without plumbing (1970). The reliability was .85.

¹³ Composed of percent of population over 65,

dimensions—such as sense of control—are not affected directly by the gross ecological differences of town and country. Personalities are shaped in smaller social contexts than that. The marginally greater distrust in cities may testify to contemporary urban problems. Or, perhaps it may reflect the plurality of social contexts in urban life, each of which—while fully integrating its own members—appears foreign to the others about it. In any event, the attribution of alienation to “urbanism as a way of life” seems incorrect.

REFERENCES

- Almond, G. and S. Verba
1963 *The Civic Culture*. Princeton: Princeton University Press.
- Armor, D. J. and A. S. Couch
1972 *A DATA-TEXT Primer*. New York: Free Press.
- Axelrod, M.
1956 “Urban structure and social participation.” *American Sociological Review* 21 (February):14-18.
- Banfield, E. C.
1958 *The Moral Basis of a Backward Society*. New York: Free Press.
- Bell, W.
1957 “Anomie, social isolation and class structure.” *Sociometry* 20 (June):105-16.
- Bell, W. and M. D. Boat
1957 “Urban neighborhoods and informal social relations.” *American Journal of Sociology* 62 (June):391-8.
- Bultena, G. L.
1969 “Rural-urban differences in familial interaction.” *Rural Sociology* 34 (March):5-15.
- Christie, R. and F. L. Gels
1970 *Studies in Machiavellianism*. New York: Academic Press.
- Curtis, J.
1971 “Voluntary association joining: a cross-cultural comparative note.” *American Sociological Review* 36 (October):872-80.
- Dean, D. G.
1961 “Alienation: its meaning and measurement.” *American Sociological Review* 26 (February):753-7.
- Epstein, A. L.
1967 “Urbanization and social change in Africa.” *Current Anthropology* 8:275-96.
- Fava, S. F.
1959 “Contrasts in neighboring: New York City and a suburban county.” Pp. 122-30 in W. M. Dobriner (ed.), *The Suburban Community*. New York: Putnam.
- Fischer, C. S.
1972 “‘Urbanism as a way of life’: A review and an agenda.” *Sociological Methods and Research* 1 (November):187-242.
1973 “Toward a subcultural theory of urbanism.” Unpublished paper, University of California, Berkeley.
- Foster, G. M.
1967 *Tzintzuntzan: Mexican Peasants in a Changing World*. Boston: Little, Brown.
- Frankenburg, R.
1965 *Communities in Britain*. Harmondsworth, England: Penguin.
- Gans, H. J.
1962 *The Urban Villagers*. New York: Free Press.
- Greer, S.
1962 *The Emerging City*. New York: Free Press.
- Gutkind, P. C. W.
1965 “African urbanism, mobility and social network.” Pp. 389-400 in G. Breese (ed.), *The City in Newly Developing Countries* (1969). Englewood Cliffs, New Jersey: Prentice-Hall.
- Guterman, S. S.
1969 “In defense of Wirth’s ‘urbanism as a way of life’.” *American Journal of Sociology* 74 (March):492-9.
- Institute for Social Research
1971 *A Panel Study of Income Dynamics*. Ann Arbor: Institute for Social Research.
- Israel, J.
1971 *Alienation from Marx to Modern Sociology*. Boston: Allyn and Bacon.
- Janowitz, M.
1967 *The Community Press in an Urban Setting*. Chicago: University of Chicago.
- Keller, S.
1968 *The Urban Neighborhood*. New York: Random House.
- Key, W. H.
1968 “Rural-urban social participation.” Pp. 305-12 in Sylvia F. Fava (ed.), *Urbanism in World Perspective*. New York: Crowell.
- Klatzky, S. R.
n.d. *Patterns of Contact with Relatives*. Washington: American Sociological Association.
- Killian, L. and C. M. Grigg
1962 “Urbanism, race and anomia.” *American Journal of Sociology* 67 (May):661-5.
- Litwak, E.
1960 “Geographic mobility and extended family cohesion.” *American Sociological Review* 25 (June):385-94.
- Lystad, M. H.
1969 *Social Aspects of Alienation: An Annotated Bibliography*. National Institute of Mental Health. Washington: U.S. Government Printing Office.
- MacDonald, A. P., Jr. and W. F. Throop
1971 “Internal-external locus of control: a bibliography.” *Psychological Reports, Supplement* 1-V28.
- Marx, K.
1961 “Economic and philosophical manuscripts.” Translated by T. Bottomore. Pp. 87-196 in E. Fromm, *Marx’s Concept of Man*. New York: Fredrick Uncar.
- Milgram, S.
1970 “The experience of living in cities.” *Science* 167 (13 March):1461-8.
- Mizruchi, E. H.
1969 “Romanticism, urbanism and small town in mass society: an exploratory analysis.”

- Pp. 243-51 in P. Meadows and E. H. Mizruchi (eds.), *Urbanism, Urbanization and Change: Comparative Perspectives*. Reading, Massachusetts: Addison-Wesley.
- Nelsen, H. M. and E. Frost
1971 "Residence, anomie and receptivity to education among Southern Appalachian Presbyterians." *Rural Sociology* 36 (December):521-32.
- Nelsen, H. M. and H. P. Witt
1972 "Religion and the migrant in the city." *Social Forces* 50 (March):379-84.
- Nelsen, H. M., R. L. Yokley and T. W. Madron
1971 "Rural-urban differences in religiosity." *Rural Sociology* 36 (September):389-96.
- Park, R. E.
1952 "The city as a social laboratory." In *Human Communities*. (1925) Glencoe, Illinois: Free Press.
- Petersen, K. K.
1971 "Villagers in Cairo: hypotheses versus data." *American Journal of Sociology* 77 (November):560-73.
- Phillips, D. L. and K. J. Clancy
1972 "Some effects of 'social desirability' in survey studies." *American Journal of Sociology* 77:921-40.
- Photiadis, J. D.
1967 "Social integration of businessmen in varied size communities." *Social Forces* 46 (December):229-36.
- Reiss, A. J., Jr.
1959 "Rural-urban and status differences in interpersonal contacts." *American Journal of Sociology* 65 (September):182-95.
- Riemer, S. and J. McNamara
1957 "Contact patterns in the city." *Social Forces* 36 (December):137-40.
- Riesman, D.
1953 *The Lonely Crowd*. Garden City: Doubleday Anchor.
- Robinson, J. P. and P. R. Shaver
1969 *Measures of Social Psychological Attitudes*. Ann Arbor: Institute for Social Research.
- Rotter, J. B.
1966 "Generalized expectancies for internal versus external control of reinforcements." *Psychological Monographs*, Whole No. 609.
- Rosenberg, M.
1956 "Misanthropy and political ideology." *American Sociological Review* 21 (December):690-5.
- Schacht, R.
1971 *Alienation*. Garden City: Doubleday Anchor.
- Seeman, M.
1959 "On the meaning of alienation." *American Sociological Review* 24 (December):783-91.
1971 "The urban alienations: some dubious theses from Marx to Marcuse." *Journal of Personality and Social Psychology* 19 (August):135-43.
1972 "Alienation and engagement." Pp. 441-66 in A. Campbell and P. E. Converse (eds.), *The Human Meaning of Social Change*. New York: Russell Sage.
- Seeman, M., J. M. Bishop and J. E. Grigsby, III
1971 "Community and control in a metropolitan setting." Pp. 423-50 in P. Orleans and R. Ellis (eds.), *Race, Change and Urban Society*, *Urban Affairs Annual Review*, Vol. 5. Los Angeles: Russell Sage.
- Simmel, G.
1957 "The metropolis and mental life." Pp. 635-46 in P. K. Hatt and A. J. Reiss, Jr. (eds.), *Cities and Society*. New York: Free Press.
- Sjoberg, G.
1964 "The rural-urban dimension in preindustrial, transitional and industrial societies." Pp. 127-59 in R. E. L. Faris (ed.), *The Handbook of Modern Sociology*. Chicago: Rand-McNally.
- Smith, J., W. H. Form and G. P. Stone
1954 "Local intimacy in a middle-sized city." *American Journal of Sociology* 60 (November):276-84.
- Srole, L.
1956 "Social integration and certain corollaries: an exploratory study." *American Sociological Review* 21 (December):709-16.
- Stein, M. R.
1964 *The Eclipse of Community*. New York: Harper Torchbook.
- Sutcliffe, J. P. and B. D. Crabbe
1963 "Incidence and degrees of friendship in urban and rural area." *Social Forces* 42 (October):60-7.
- Swedner, H.
1960 *Ecological Differentiation of Habits and Attitudes*. Lund, Sweden: CWK Gleerup.
- Tallman, I. and R. Morgner
1970 "Life-style differences among urban and suburban blue-collar families." *Social Forces* 48 (March):334-48.
- Tomeh, A. K.
1964 "Informal group participation and residential pattern." *American Journal of Sociology* 70 (July):28-35.
- Wellman, B., P. Craven, M. Whitaker, H. Stevens, A. Shorrer, S. DuToit, H. Bakker
Forth- "Community ties and support systems." coming in L. S. Bourne, R. D. MacKinnon and J. W. Simmons (ed.), *The Form of Cities in Central Canada: Selected Papers*.
- Whyte, W. F.
1955 *Street Corner Society*. Chicago: University of Chicago.
- Wilson, J. Q.
1968 "The urban unease." *The Public Interest* (Summer).
- Wirth, L.
1938 "Urbanism as a way of life." *American Journal of Sociology* 44 (July):3-24.
- Wright, C. R. and H. H. Hyman
1958 "Voluntary association memberships of American adults." *American Sociological Review* 23 (June):284-94.
- Wylie, L.
1964 *Village in the Vaucluse*. New York: Harper and Row (Colophon).
- Young, M. and P. Willmott
1957 *Family and Kinship in East London*. Baltimore: Penguin.

EDUCATION AND PREJUDICE OR EDUCATION AND RESPONSE-SET? *

MARY R. JACKMAN

Michigan State University

American Sociological Review 1973, Vol. 38 (June):327-339

This paper re-examines the theory of working class authoritarianism and attempts to assess the evidence for a competing interpretation of the empirical relationship between education and prejudice which posits that the relationship is heavily confounded with an education-related acquiescence response-bias that derives from differing cognitive styles and cue-searching mechanisms. Our analysis of data from a 1964 national survey of the United States relies mainly on use of a modified version of Campbell and Fiske's Multitrait-Multimethod Matrix (1959) to ascertain how much of the relationship between education, authoritarianism and anti-Semitism is due to a shared method effect of the F and anti-Semitism scales. The analysis also utilizes a path model of the theory of education-related response-bias where the effect of education and general knowledge on responses to the F-scale and two different measures of anti-Semitism is completely mediated by two unobserved variables, "method" and "pure anti-Semitism." The paper concludes that the theory of working class authoritarianism has depended heavily for support on the use of items phrased as positively-worded agree-disagree statements that yield an education-related acquiescence response-bias that has very little to do with the respondent's true attitudes.

THE empirical relationship between education and prejudice is generally regarded as evidence for the theory of "working class authoritarianism," but an alternative theory of education-related response-bias (or cognitive style) can be used to account for such a relationship without recourse to notions of differential endorsement of democratic norms among those with differing amounts of education. This paper seeks to assess the empirical support for these two competing interpretations.

THEORETICAL BACKGROUND

Working Class Authoritarianism. The authors of *The Authoritarian Personality*

(Adorno *et al.*, 1950) were the first to observe systematically a relationship between authoritarianism (as measured by their thirty-item California F-scale) and ethnocentrism and prejudice toward outgroups. They interpreted the "authoritarian personality" as an outgrowth of early childhood experiences where parents use harsh and rigid forms of discipline and make their love and approval contingent on an unquestioning obedience from the child. The child responds to rigid parental discipline by developing a strong hostility toward the parents which is too dangerous to express: rigid repression of hostility toward the parents leads to an identification and idealization of authority and a concomitant displacement of the hostility on to a safe outlet—minority groups—and a projection on to them of the authoritarian's own punished and repressed impulses. The authors of *The Authoritarian Personality* thus viewed authoritarianism as a personality characteristic that stemmed from a harsh disciplinary upbringing and that led directly to negative and hostile feelings toward minority groups.

In 1954 Hyman and Sheatsley raised the issue that authoritarianism was related to education, and they suggested that the measure may reflect educational or status differences rather than inherent personality traits. The theory of "working class authoritarian

* I would like to thank Warren O. Hagstrom, Robert W. Jackman, William M. Mason, Russell Middleton, Karl F. Schuessler, and William H. Sewell for their helpful criticisms of earlier drafts of this paper. I am especially grateful to David L. Featherman for suggesting the utility of a path model with unobserved variables for this problem and to both him and Robert M. Hauser for their assistance in obtaining a solution to that model. Any remaining errors remain the author's sole responsibility. The research was supported in part by a grant from the Graduate Research Committee at the University of Wisconsin, and the data were kindly made available by Gertrude Selznick and Stephen Steinberg through the Data and Program Library Service at the University of Wisconsin.

ism," presented by Lipset in 1959, develops this notion further:

the lower strata are *isolated* from the activities, controversies, and organizations of democratic society—an isolation that prevents them from acquiring the sophisticated and complex view of the political structure which makes understandable and necessary the norms of tolerance (Lipset, 1959:112).

In a similar vein Selznick and Steinberg (1969:184) conclude that "lack of education is the primary factor in [anti-Semitism's] acceptance" and account for this by positing the existence of two antithetical cultures in American society: one the "enlightened" culture of the well-educated which prizes democratic and scientific values, and the other the "common" culture of the poorly educated which is simplistic, authoritarian and anomic in its perspective.

The uneducated are cognitively and morally unenlightened because they have never been indoctrinated into the enlightened values of the larger society and in this sense are alienated from it. . . . As individuals move from grade school to high school, to college and the university, they are brought into progressively closer contact with ideal values and increasingly acquire criteria for the rejection of the common culture (Selznick and Steinberg, 1969:157).

Thus both Lipset and Selznick and Steinberg see authoritarianism as resulting primarily from a lack of exposure to and training in democratic norms, rather than as an expression of an inherent personality characteristic stemming from a harsh disciplinary upbringing. We should note that this reworking of the term implies an even more substantial modification of its meaning, a modification which is made explicit by Selznick and Steinberg in their statement:

The common culture and the enlightened culture differ in their psycho-dynamic import, the unenlightened culture giving freer reign to the more primitive psychic mechanisms. As both Freud and Durkheim recognized, the enlightened culture is an inhibiting culture; it is designed to restrain and discipline the more primitive impulses of man (1969: 168-9).

Thus the theory of working class authoritarianism abandons the notion that a high score on the F-scale describes a personality response to harsh repression and instead as-

sumes that it represents a *failure* to repress man's "primitive impulses" and to "indoctrinate" him into democratic norms.¹

Both Lipset and Selznick and Steinberg see education as the phenomenon that most effectively predicts a person's exposure to enlightened, or democratic, norms; and thus they see education as the prime cultural stratifier of society,² with low participation in voluntary and political organizations, little reading, economic insecurity, and general social isolation all contributing to the greater predominance of authoritarianism among the less educated strata: this in turn predisposes the less educated to take a prejudiced and hostile stance toward minority groups. In both works, the elite or higher status groups are pictured, rather ironically, as possessing cultural norms and values that are far more conducive to the maintenance of "democratic" systems than are the cultural mores of the masses.

Authoritarianism or Acquiescence? A major challenge to the theory of working class authoritarianism has been a simple methodological argument that the F-scale used to measure the concept is invalid. While a number of other important criticisms have been made of the theory of working class authoritarianism on conceptual, theoretical, empirical, and methodological grounds (Jackman, 1972), this paper is restricted to a consideration of the criticism that an education-related response bias accounts for the apparent relationship between education and "prejudice." There has been much uncer-

¹ Note that this modification of the term authoritarianism implies a quite different philosophy about the nature of man: while the theory of Adorno *et al.* assumes that man develops best in an atmosphere of freedom, the implication of the theory of working class authoritarianism is that man develops best under conditions of restraint.

² Some confusion over Lipset's position has arisen because he termed his theory *working class* authoritarianism, and at least two of Lipset's critics (Lipsitz, 1965; Zeitlin, 1967:250-4) have attempted to reject the theory by demonstrating that the relationship between occupational status and authoritarianism diminishes greatly when a control for education is introduced. However, Lipset himself made that observation in his original outline of the theory; and he explicitly states that "Increases in tolerance associated with higher educational level are greater than those related to higher occupational level" (1959:109-10).

tainty and disagreement over what the content of the F-scale items is actually tapping,³ but even more pressing is the recurrent charge that the phrasing of the items has caused many respondents to respond to the *form* of the item rather than to the content. Since all the items on the California F-scale are phrased as abrupt "authoritarian" statements with which the respondent may agree or disagree, it has been argued that many respondents may score high on the F-scale because they will say "yes" to any questionnaire statement, regardless of content.

The first to open this methodological attack on the F-scale was Cohn (1953); and many other studies followed, prompting at least three different reversed versions of the F-scale to appear (Bass, 1955; Christie *et al.*, 1958; Peabody, 1961). While for Christie *et al.* (1958), content-free acquiescence was a relatively minor problem associated only with respondents in a state of ideological confusion, Bass (1955) and Peabody (1961; 1964; 1966) saw it as a serious flaw. Bass (1955) argued that acquiescence was associated with item ambiguity, and reported that it accounted for 60 percent of the variance in the F-scale. Peabody interpreted "yea-saying" as a response tendency when the subject is uncertain (1966) and reported that acquiescence response-set accounted for 75 percent of the answers to all items on the F-scale (1964): he argued that "yea-saying" was far more prevalent than "nay-saying," making low scores on the F-scale fairly valid but high scores invalid; and he reported that the correlation between the F-scale and anti-Semitism (measured with positive and reversed versions of both scales) appeared to be due to a covariation of the combined asymmetrical effect of acquiescence response-set and attitudes opposing authoritarianism and anti-Semitism in the two scales (1961). Couch and Keniston (1960) constructed a 360-item Over-All Agreement Score that was designed to be unaffected by any specific content, as a measure of "yea-saying" response bias, and they found that it was cor-

related .37 with scores on a ten-item positive F-scale and only .09 with scores on a balanced F-scale consisting of five positively- and five negatively-worded items.

While all of the studies reviewed so far relied on college students as subjects, Angus Campbell *et al.* (1960) drew on data from a national cross-sectional sample to suggest that not only was acquiescence a serious contaminating factor among high scorers on the F-scale; but, further, that acquiescence was negatively related to education. They posited that increasing years of education give the individual greater cognitive sophistication which predisposes him negatively toward simplistically worded statements, regardless of content. Meanwhile, the poorly educated respondent is more likely to be "fooled" by the phrasing of an agree-disagree item and to acquiesce to it because his own *style* of expression conforms more readily to the phrasing of the item. Thus, while the better educated respondent is predisposed to approach any sweeping statement with caution, the comparatively poor cognitive sophistication of the less educated respondent prevents him from giving a considered response to such a statement. To support their argument they found (using an abbreviated five-item F-scale) that when they used the original F-scale items, authoritarianism was negatively associated with education; but with the Christie reversals, authoritarianism was *positively* associated with education, indicating that "yea-saying" is more prevalent among respondents with little education and that "nay-saying" is more prevalent among the highly-educated: the "yea-saying" of the poorly educated remained a more serious problem, however, with responses on original and reversed items being more content-consistent among the college educated than among less educated respondents.

Work by Converse (1969) on attitudes and non-attitudes is also relevant to the notion of education-related acquiescence response-set. Converse theorized that respondents in mass publics are often ignorant on many subjects tapped in public opinion questionnaires but that they approach the questionnaire like an I.Q. test; and rather than reveal their ignorance on some subject, they will express an opinion where a non-

³ One of the earliest observations of the ambiguity of the items was by Christie, who postulated that the content of the items might be interpreted quite differently by individuals occupying different positions in the class structure of society (1954:175).

opinion might be more appropriate. We can generalize Converse's argument to suggest that such respondents might also try and guess the "correct" response; when items are weighted in one direction, the response that lies in that direction may be more likely to appear as the "correct" one to the ignorant or uncertain respondent. Since we might expect general knowledge to be a function of years of education, we can suggest that the low general knowledge and low cognitive sophistication of the less educated respondent lead him to interpret cues in a simple, positively-worded, agree-disagree questionnaire item quite differently from the well-educated respondent: while the former takes the phrasing of the item as a cue to say "yes," the latter takes such phrasing as a cue to say "no."

Thus two factors, low general knowledge and low cognitive sophistication, contribute to the greater tendency of the poorly educated respondent to acquiesce to any agree-disagree item, regardless of content. In line with the ideas of Bass (1955) and Carr (1971), we can argue that this tendency to acquiesce will become exacerbated as the substantive content of the item becomes more ambiguous and the respondent reacts more and more to the *form* rather than to the uncertain content of the item in his search for cues.

Two major defenses have been made of the California F-scale in light of the literature on acquiescence response-bias. First, the argument has been made that acquiescence should not be regarded as a *contaminating* factor in responses to the F-scale because it is itself a part of the authoritarian syndrome: people who acquiesce to any statement, regardless of content, have submissive personalities that will acquiesce to any authority; thus they provide the backbone of support for authoritarian and dictatorial regimes (e.g., Lipset, 1959:115; McClosky, 1964; Selznick and Steinberg, 1969:xix-xx). However, even if we can accept the idea that acquiescence response-bias represents something more fundamental to the individual's personality than merely a response-bias invoked by a specific form of item in a questionnaire-interview context, the expansion of the concept of authoritarianism to include extreme suggestibility does

not tie in well with earlier conceptualizations of the term as describing an inherently rigid personality structure. While the original formulation of the term by Adorno *et al.* described a personality rigidly constrained to hold certain sets of beliefs, the new modification of the term undermines its substantive significance by suggesting that an authoritarian's mind is very flexible and can hold any kind of attitude rather than being specifically constrained to hold "authoritarian" attitudes. Lipset himself is ambiguous on the issue since he describes the working class authoritarian as both "rigid" (1959: 98, 100) and "suggestible" (1959:115).

A more convincing defense of the F-scale has been made by Samuelson and Yates (1967), who studied the Peabody (1959; 1961) and Christie (1958) reversals to the F-scale and suggested that double agreement to original and reversed items may not always be inconsistent because many reversals may not be the *exact* opposites of the original F item, i.e., of equal intensity and sharing the same neutral point. Samuelson and Yates found that mean ratings (by students) of size of disparity between original and reversed F items covaried highly with the amount of double agreement in responses to the pair by other groups of students: the difference between the disparity rating of an F item and the reversed Peabody item and the same F item and the corresponding reverse Christie item correlated - .64 with the difference between the proportion of double agreement responses each pair received.

This research raises a serious critique of the research we reviewed earlier, all of which used reversals of the original items to demonstrate acquiescence: Samuelson and Yates have demonstrated empirically that among college students, ratings by other college students of the degree of disparity between two items is highly (negatively) associated with the proportion of double agreements the pair receive. As they point out, such a result does not invalidate the notion that responses to the original F items may be due to acquiescence rather than content—they argue that (quasi) ratio scales would be needed to give a definitive answer to that question—but their work does invalidate past *demonstrations* of acquiescence in the F-scale among college students that have de-

pended on double agreement scores to original and "reversed" items.⁴

Following the article by Samuelson and Yates (1967), Donald Campbell *et al.* (1967) reported research where they examined the correlations of positive and reversed versions of the F-scale with positive and reversed versions of other scales (such as the Ethnocentrism Scale) rather than examining correlations between positive and reversed versions of the F-scale alone. They reported that direction-of-wording accounted for some of the covariance between the F- and E-scales, but that trait covariance was greater. However, the data for the Samuelson and Yates (1967) and the Donald Campbell *et al.* (1967) studies came from samples of college students; and research reported by Angus Campbell *et al.* (1960) suggested that content-free acquiescence was negatively associated with education and contributed more heavily to the apparent authoritarianism of the less educated than it did to the lower authoritarianism scores of the highly educated. Thus, the results reported by Samuelson and Yates (1967) might not be reproduced in a cross-sectional sample; but in the absence of data like Samuelson and Yates' for a cross-sectional sample, we suggest that an attack on the theory that education and the authoritarian syndrome are negatively associated cannot rest on a demonstration of double agreements to original and reversed items. In the following analysis we use a technique that does not depend on double-agreement estimates but uses a logic similar to that of Donald Campbell *et al.* (1967) to estimate what proportion of the relationship between education, authoritarianism, and anti-Semitism is due to invalid measurement.

DATA ANALYSIS

The data employed in this paper comes from a national survey of the United States conducted by the National Opinion Research Center in 1964 for Gertrude Selznick and

Stephen Steinberg, as part of a study on prejudice funded by the Anti-Defamation League. The survey collected a modified probability (block quota) sample: the total N is 1976. It was from this data that Selznick and Steinberg drew support for their theory about the authoritarian culture of the poorly educated. The key evidence that they presented to support their theory about "mass" and "enlightened" cultures consisted of demonstrated relationships in percentaged cross-tabulations between education, F-scale score and anti-Semitism, and education, anomie score and anti-Semitism.⁵ Measures of authoritarianism, anomie and anti-Semitism used in these tables were summated scales of positive responses to positively-worded agree-disagree statements. The full wording of the items in the F, anomie, and anti-Semitism scales is reported in Appendix A. The F-scale is an abbreviated five-item version of the original California F-scale in which all positive responses are the authoritarian ones; the anomie scale consists of three items which are also positively-worded; while the Index of Anti-Semitic Belief consists of eleven stereotypical belief items, of which eight are positively-worded statements, two are positively-worded questions and one is a negatively-worded statement.⁶

Our first step in the analysis was to construct a modified version of Campbell and Fiske's (1959) multitrait-multimethod matrix to test the validity of the major attitudinal variables used by Selznick and Steinberg. The purpose of the multitrait-multimethod matrix is to assess both convergent and discriminant validity.⁷ As Campbell and

⁵ Both Lipset (1959) and Selznick and Steinberg (1969) elaborate their models with other variables such as social participation and general knowledge; but relationships among education, F, anomie, and ethnic attitude provide the fundamental core of the theory.

⁶ Selznick and Steinberg defended their use of such items by arguing that acquiescence was very slight in this sample, while the relationships between education and authoritarianism, anomie, and anti-Semitism respectively were too strong to be seriously confounded by the small amount of acquiescence (1969: xix-xx, 11).

⁷ Selznick and Steinberg employed the following procedures to assess the validity of their Index of Anti-Semitic Belief (see pp. 23-6): (a) They tested to see whether score on the whole Index was cor-

⁴ We might note, however, that the F-Scale items may be too ambiguous in content for the construction of "exact" reversals to be a viable possibility. If this is the case, the interpretation of responses to the original items becomes suspect, quite apart from the problem of acquiescence response-set.

Fiske (1959) point out, a valid measure of a concept should not only correlate at an acceptably high level with other measures of the same concept: it should also (a) correlate more highly with other measures of the same concept that use different methods than it does with different concepts measured by the same method, and (b) enjoy the same relationship with other variables as do other measures of the same concept that use a different method. Such a validation procedure is essential for assessing artifactual effects of different methods of measurement and is highly appropriate for situations where one proposes to test a theory about the relationship among a number of concepts with a set of empirical measures that all employ the same method.

Since all three attitudinal variables used by Selznick and Steinberg are measured by the same method (i.e., summated scales of positively-worded statements with which the respondent can agree or disagree), the relationships that Selznick and Steinberg reported may be due to a shared method effect of the three measures that is itself associated with education rather than to a valid empirical association of the attitudes with each other and with education. In view of previous theorizing in the literature about education-related acquiescence in questionnaire items of the type used by Selznick and Steinberg, the construction of a multitrait-multimethod matrix would seem particularly advisable before using the measures in an empirical test of a theory such as theirs.

Unfortunately, our matrix must be modified, as Selznick and Steinberg's questionnaire contained more than one method of measurement for only one of the three major attitudinal variables in their theory, namely, anti-Semitism. In addition to their Index of Anti-Semitic Belief which measured the acceptance of stereotypical beliefs about Jews (phrased as statements), there were a few others items measuring social distance feel-

ings towards Jews that were phrased as questions with response options that were equally balanced. Three of these items were selected to construct a summated social distance scale (see Appendix A for the wording of the three items),⁸ and a correlation matrix was calculated for the following variables: education, authoritarianism (one measure—Selznick and Steinberg's F-scale), anomie (one measure—Selznick and Steinberg's anomie scale), and anti-Semitism (two measures—Selznick and Steinberg's Index of Anti-Semitic Belief [A-S1] and our A-S Scale of Social Distance Feelings [A-S2]); also included in the matrix is a fifteen-item general knowledge index (see Appendix A for the full wording of the items).

In examining Table 1, we are interested in checking two majority validity requirements:

(a) the A-S1 scale should correlate more highly with the A-S2 scale (which is tapping the same concept with a different method of measurement) than it does either with the F-scale or the anomie scale (which tap different concepts with the same method of measurement) or with education (which is a conceptually distinct variable that does not share the same method of measurement—education was an open question); and

(b) the correlation between anti-Semitism and the other theoretically relevant variables (i.e., education, F, and anomie) should be the same, whichever measure of anti-Semitism is used.

Examination of the matrix in Table 1 indicates that neither of these conditions is met by these data. First, the A-S1 scale correlates just as highly with the F and anomie scales (same method, different traits) as it does with the A-S2 scale (different method, same trait). This suggests that the A-S1 scale is not completely a measure of response-set, but that it is a seriously con-

related with probability of giving a prejudiced response on individual items. This procedure is not very stringent, since a high score on the whole Index implies by definition a higher likelihood of scoring prejudiced on each individual item; (b) They tested to see whether score on the Index was related to responses to negative beliefs about Jews not included in the Index.

⁸ In a correlation matrix of component items not presented here, items 1 and 2 of the A-S2 scale correlated more highly with some items from the F and anomie scales than they did with item 7 from the A-S1 scale: however, this was disregarded since both low correlations were with the same item from the A-S1 scale. Otherwise, the social distance items (A-S2) correlated more highly with each other and with the items from the A-S1 scale than they did with the other traits.

Table 1. Multitrait-Multimethod Matrix for Anti-Semitism, F, Anomie, Education, and General Knowledge*

	1	2	3	4	5	6
1 Education**					
2 General Knowledge	.582				
3 F-scale	-.418	-.371			
4 A-S1 Scale	-.367	-.354	.426		
5 A-S2 Scale	-.099	-.186	.128	.420	
6 Anomie Scale	-.335	-.319	.413	.427	.173
7 A-S1 (b) Scale	-.357	-.351	.381	.886	.377	.403

* N = 1654. Non-whites and Jews excluded from the analysis. Data from 1964 NORC national survey of the U.S.

** Education was scored as a continuous variable ranging from 0 through 17+ years of education.

taminated measure of anti-Semitism. Second, the relationships between anti-Semitism and F; anomie, general knowledge and education respectively, appear fairly substantial when the A-S1 scale is used, but drop precipitously to extremely small relationships when the A-S2 scale is used. Although the A-S1 scale is not exactly equivalent in content to the A-S2 scale, the former being a measure of the cognitive component of an attitude while the latter reflects the affective component, both are intended to tap different aspects of the same underlying phenomenon, anti-Semitism. Thus the conceptual gap between A-S1 and A-S2 is relatively trivial, compared to the unambiguous conceptual gap between each of these and the F or anomie scales. In addition, the validity matrix indicates that although the A-S1 scale does not correlate more highly with the A-S2 scale than it does with F and anomie, the A-S2 scale does correlate much more highly with A-S1 than it does with F and anomie. This is consistent with our interpretation that the pattern of our results is primarily due to response-set, rather than to differences in the content of the two anti-Semitism scales.

It might be argued that the difference between the correlations obtained with the A-S1 scale and the A-S2 scale could be due to differential unreliability in the two scales since the latter has only three items while the former has eleven items: all other things

being equal, a longer scale will be more reliable than a shorter one; and this could be differentially attenuating the correlations obtained with the A-S2 scale. Although we can compute an internal consistency measure such as Cronbach's Alpha for the scales involved, it is highly unlikely that our scales meet the assumptions of random, uncorrelated errors and essential tau-equivalence that are necessary and sufficient to make Cronbach's Alpha equivalent to a lower-bound estimate of reliability (Bohrnstedt, 1970; Goldberger, 1971; Lord and Novick, 1968; Novick and Lewis, 1967). Although essential tau-equivalence is less stringent than the assumptions of tau-equivalence or parallel forms, it remains a fairly stiff assumption to meet (as Novick and Lewis, 1967:7 have observed), especially with cross-sectional survey data.⁹ A more satisfactory resolution to the problem of possible differ-

⁹ Using the formula from Lord and Novick (1968:88), the Alpha coefficients for A-S1 and A-S2 are calculated as .851 and .512 respectively. Assuming a reliability of .933 for the measure of respondent's education (this figure is taken from Featherman, 1972), the correlations between A-S1 and education and A-S2 and education respectively can be "corrected" for attenuation with the formula given in Bohrnstedt (1970:84), yielding a corrected correlation between A-S1 and education of -.412 ($R^2 = .17$) and a corrected correlation between A-S2 and education of -.143 ($R^2 = .02$). The latter correlation still fails to approach the magnitude of the former.

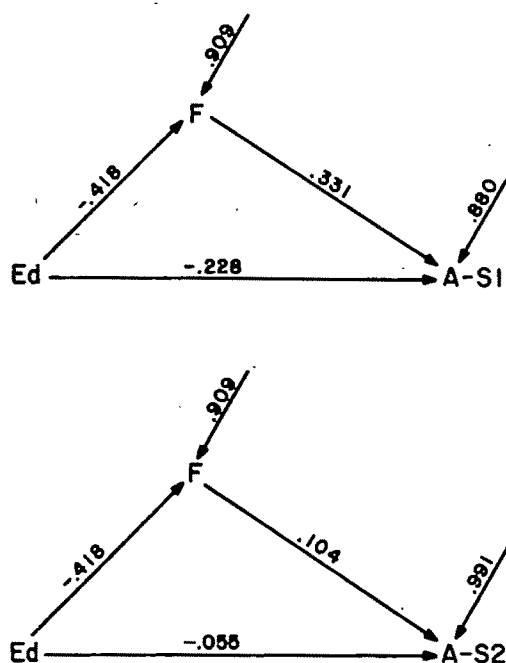


FIGURE 1. PATH MODELS FOR TWO DIFFERENT MEASURES OF ANTI-SEMITISM WITH EDUCATION AND "AUTHORITARIANISM." *

(a) Using A-S1 Scale:

(b) Using A-S2 Scale:

* N=1654. Non-whites and Jews excluded from the analysis. Missing data were assigned mean values. Variables scored as in Table 1 and Appendix A. Data from 1964 national survey of the United States.

ential reliability in the A-S1 and A-S2 scales would be to re-estimate the correlations in Table 1 with versions of A-S1 and A-S2 of equal length to one another. Since the data restrict the A-S2 scale to three items, we constructed a reduced three-item version of the A-S1 scale (A-S1(b) in Table 1) to estimate whether the difference between the results obtained with these two scales is due solely to their different lengths. The A-S1(b) scale is composed of the three items from the A-S1 scale with the highest item-to-total correlations, which were thus regarded as most representative substantively of the full scale (although there is very little variation in the item-to-total correlations of the eleven items).¹⁰ The results in Table 1 indicate

¹⁰ The items selected for A-S1(b) are items 3, 8 and 11. (See Appendix A for their wording.) Their item-to-total correlations are .716, .689 and .747 respectively.

that the correlations obtained with the A-S1 (b) scale are only minimally smaller than those obtained with A-S1, and the overall pattern of the correlations remains the same whether one refers to the A-S1 scale or the A-S1(b) scale. Not surprisingly, many other factors apart from scale length and reliability influence the size of observed correlations; and in this case we argue that the effect of relative scale length and unreliability on the results is at best very minor. For the remainder of the analysis, the full A-S1 scale will be used to ensure comparability with earlier work by Selznick and Steinberg (1969).

As a further demonstration of the dramatic difference between the results achieved by use of the two different measures of anti-Semitism, Figure 1 presents two path models of the theorized relationship between education, F, and anti-Semitism.¹¹ Model A, which uses the A-S1 scale, presents persuasive evidence in favor of the theory of working class authoritarianism; while in Model B, which uses the A-S2 scale, the paths from education and F to anti-Semitism drop to almost zero.¹² Where Model A explains almost 23 percent of the variance in anti-Semitism, Model B accounts for less than 2 percent of the variance in the same phenomenon. We might hypothesize that a similar change in the relationship between education and "authoritarianism" might occur if questions with balanced response options were substituted for the positively-worded statements of the F-scale.

As a final step in the analysis, Figure 2 presents a path model which attempts to specify the theory of education-related response-bias more explicitly than was possible in the multitrait-multimethod matrix. In this model, the F and A-S1 scales are assumed to be indicators of an underlying hypothetical "method" factor (acquiescence response-bias), and the A-S1 and A-S2 scales

¹¹ The models in Figure 1 are intended to represent the major elements of the theory presented by Selznick and Steinberg (1969:esp. 156-62). They posited that education had a direct effect, as well as an indirect effect through authoritarianism, on anti-Semitism.

¹² Equivalent results were achieved when the anomie scale was substituted for the F-scale, but these results are not reported here.

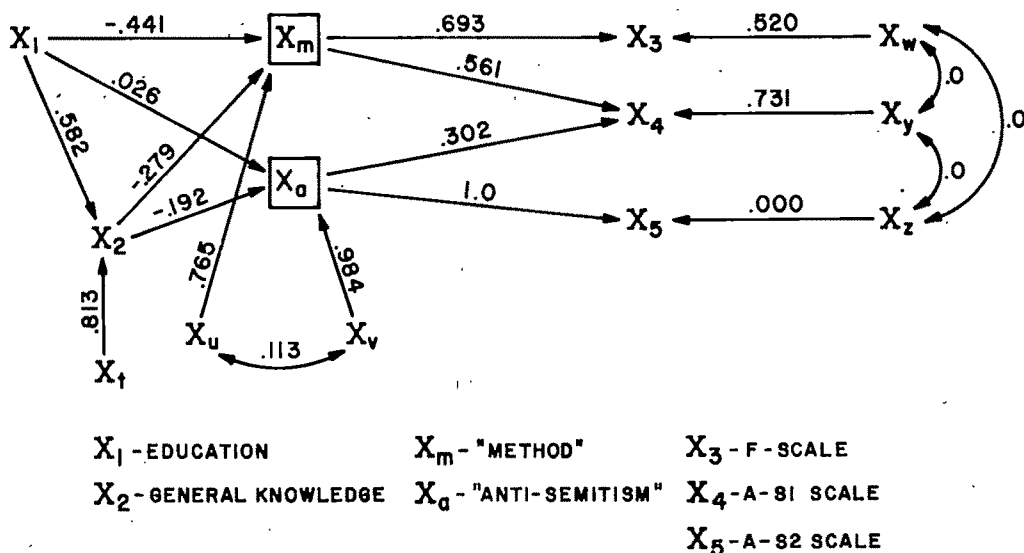


FIGURE 2. PATH MODEL OF THE THEORY OF EDUCATION-RELATED RESPONSE-BIAS.

are assumed to be indicators of an underlying hypothetical "pure anti-Semitism" factor; these unmeasured variables are postulated to mediate entirely the effect of education and general knowledge on the F, A-S1 and A-S2 scales. (See Appendix B for the solution for the model). The "general knowledge" variable is relevant to a model of education-related response-bias because of our hypothesis that the relationship between education and acquiescence response-bias is partly mediated by the individual's level of general knowledge: the variable is also intended in part as a proxy for cognitive sophistication on the rationale that a person's cognitive sophistication will develop along with his general knowledge.

To support the theory of education-related response-bias, the model should yield strong negative paths from education and general knowledge to the unmeasured "method" factor, while the paths from these explanatory variables to the unmeasured "pure anti-Semitism" factor should be very weak (the model would support the theory of working class authoritarianism if the reverse were true). Furthermore, the F-scale should load on to the "method" factor more heavily than should the A-S1 scale because the F-scale items are more ambiguous in content; however, the A-S1 scale should still load on to the "method" factor at least as heavily as it does on to the "pure anti-Semitism" factor

as an indication of strong contamination of the scale by response-bias. Finally, the A-S2 scale should load more heavily on to the "pure anti-Semitism" factor than should the A-S1 scale (the model assumes that there is no path from the "method" factor to A-S2 because there is no reason to expect acquiescence response-bias in the items of A-S2) and the two hypothetical factors should be orthogonal since content-free acquiescence should not be related at all to "pure anti-Semitism."

The results yielded by the model in Fig.

Table 2. Zero-Order Correlations of Unobserved Variables with Observed Variables*

	"Method"	"Anti-Semitism"
1 Education	-.603	-.094
2 General Knowledge	-.535	-.177
3 F-Scale	.693	.122
4 A-S1 Scale	.615	.401
5 A-S2 Scale	.184	1.00
"Method"		.176

* N = 1654. Non-whites and Jews excluded from the analysis. Data from 1964 NORC national survey of the U.S.

ure 2 are not completely consistent with the theory of education-related response-bias, but they do lend considerable support to it. First, comparing p_{4m} with p_{4a} , the A-S1 scale appears to be partly an indicator of "pure anti-Semitism" but to be heavily contaminated by response-bias; however, the A-S1 scale does not appear to be as heavily contaminated by response-bias as does the F-scale which is composed of more ambiguous items. Second, comparing p_{5a} with p_{4a} , we see that the A-S2 scale appears to be a much more valid indicator of "pure anti-Semitism" than is the A-S1 scale.¹⁸ Finally, the negative paths from education and general knowledge to the "method" factor are strong, as the theory of education-related response-bias predicts; and the two explanatory variables account for almost 42 percent of the variance in the "method" factor, while the same variables account for only 3 percent of the variance in the "pure anti-Semitism" factor.

A problem for the theory of education-related response-bias is that the two hypothetical variables are not completely orthogonal: if the acquiescence in the F and A-S1 scales is totally content-free as the theory posits, the "method" factor should be quite unrelated to any "pure anti-Semitism" factor; but instead, the two hypothetical variables are weakly correlated ($r = .176$) suggesting that some part of the shared variance between the F and A-S1 scales may be substantive. Employing a variance decomposition of the correlation between the F and A-S1 scales, 8.6 percent of the variance runs through the "pure anti-Semitism" factor while the rest can be attributed to the shared hypothetical "method" factor underlying the two scales. The weak correlation between the two unmeasured variables suggests that people who acquiesce to generalized, platitudinous items (such as those on the F-scale) may also be a *little* more likely genuinely to hold simplified stereotypical beliefs about another ethnic group (both positive and negative, although in this case only negative beliefs were tapped).

¹⁸ The solution yielded a value for p_{5a} of 1.047, which we attributed to rounding error (since the correlations used to estimate the paths were taken to only three decimal places) and rounded down to 1.0.

CONCLUSION

This paper examined two competing theoretical interpretations of the empirical relationship between education and prejudice—the theories of working-class authoritarianism and education-related response-bias—and attempted to assess the evidence for each. The results from our analysis strongly suggest that education-related acquiescence response-bias was a shared method effect of the F, anomie, and Selznick-Steinberg anti-Semitism scales: their Index of Anti-Semitic Belief correlates as highly with other traits—F and anomie—which share the same method of measurement (i.e., positively-worded agree-disagree statements) as it does with another measure of the same trait (the A-S scale of Social Distance Feelings). Moreover, *the relationship between education, F (or anomie), and anti-Semitism does not survive the replacement of the A-S1 scale with the A-S2 scale* which is composed of social distance items with equally balanced response options. Results of a path-analytic representation of the theory of education-related response-bias with two unobserved variables in the model ("method" and "pure anti-Semitism") were largely congruent with the latter theory, suggesting that past demonstrations of the relationship between education, authoritarianism, and anti-Semitism have been seriously confounded with an education-related acquiescence that is at best weakly related to true anti-Semitism.

REFERENCES

- Adorno, T. W., E. Frenkel-Brunswick, D. J. Levinson and R. N. Sanford
1950 *The Authoritarian Personality*. New York: Harper.
- Bass, B. M.
1955 "Authoritarianism or acquiescence?" *Journal of Abnormal and Social Psychology* 51:616-23.
- Bohrnstedt, George W.
1970 "Reliability and validity assessment in attitude measurement." Pp. 80-99 in Gene F. Summers (ed.), *Attitude Measurement*. Chicago: Rand McNally.
- Campbell, Angus, Philip E. Converse, Warren E. Miller and Donald E. Stokes
1960 *The American Voter*. New York: Wiley.
- Campbell, Donald T. and Donald W. Fiske
1959 "Convergent and discriminant validation by the multitrait-multimethod matrix." *Psychological Bulletin* 56 (January):81-105.

- Campbell, Donald T., Carole R. Seigman and Matilda B. Rees
1967 "Direction-of-wording effects in the relationships between scales." *Psychological Bulletin* 68 (November):292-303.
- Carr, Leslie G.
1971 "The Srole items and acquiescence." *American Sociological Review* 36 (April):287-93.
- Christie, Richard
1954 "Authoritarianism re-examined." Pp. 123-96 in Richard Christie and Marie Jahoda (eds.), *Studies in the Scope and Method of "The Authoritarian Personality."* Glencoe: Free Press.
- Christie, R., J. Havel and B. Seidenberg
1958 "Is the F scale irreversible?" *Journal of Abnormal and Social Psychology* 56: 143-59.
- Cohn, T. S.
1953 "The relation of the F scale to a response set to answer positively." *American Psychologist* 8:335 (abstract).
- Converse, Philip E.
1969 "Attitudes and non-attitudes: continuation of a dialogue." Pp. 168-89 in Edward R. Tufte (ed.), *The Quantitative Analysis of Social Problems.* Reading, Mass.: Addison-Wesley.
- Costner, Herbert L.
1969 "Theory, deduction, and rules of correspondence." *American Journal of Sociology* 75 (September):245-63.
- Couch, A. and K. Keniston
1960 "Yeasayers and naysayers: agreeing response set as a personality variable." *Journal of Abnormal and Social Psychology* 60:151-74.
- Duncan, Otis Dudley, David L. Featherman and Beverly Duncan
1968 *Socioeconomic Background and Occupational Achievement: Extensions of a Basic Model. Final Report, project No. 5-0074 (EO-191); contract No. OE-5-85-072, U.S. Office of Education.* Ann Arbor: University of Michigan.
- Featherman, David L.
1972 "Achievement orientations and socioeconomic career attainments." *American Sociological Review* 37 (April):131-43.
- Goldberger, Arthur S.
1971 "Psychometrics and econometrics: a survey of communalities." *Psychometrika* 36 (June):83-107.
- Hauser, Robert M. and Arthur S. Goldberger
1971 "The treatment of unobservable variables in path analysis." Pp. 81-117 in H. L. Costner (ed.), *Sociological Methodology 1971.* San Francisco: Jossey-Bass.
- Hyman, H. H. and P. B. Sheatsley
1954 "The Authoritarian Personality"—a methodological critique." Pp. 50-122 in Richard Christie and Marie Jahoda (eds.), *Studies in the Scope and Method of "The Authoritarian Personality."* Glencoe: Free Press.
- Jackman, Mary R.
1972 *Attitudes Toward Ethnic Groups: an Examination of Their Structure and Theoretical Determinants.* Unpublished doctoral dissertation, University of Wisconsin, Madison.
- Lipset, Seymour Martin
1959 *Political Man.* London: Heinemann.
- Lipsitz, Lewis
1965 "Working class authoritarianism: a re-evaluation." *American Sociological Review* 30 (February):103-9.
- Lord, F. M. and M. R. Novick
1968 *Statistical Theories of Mental Test Scores.* Reading, Mass.: Addison-Wesley.
- McClosky, Herbert
1964 "Consensus and ideology in American politics." *American Political Science Review* 58 (June):361-82.
- Novick, Melvin R. and Charles Lewis
1967 "Coefficient alpha and the reliability of composite measurements." *Psychometrika* 32 (March):1-13.
- Peabody, D.
1959 *The Organization of Social Attitudes: A Study in Response Set.* Unpublished doctoral dissertation, Harvard University.
- 1961 "Attitude content and agreement set in scales of authoritarianism, dogmatism, anti-Semitism and economic conservatism." *Journal of Abnormal and Social Psychology* 63:1-11.
- 1964 "Models for estimating content and set components in attitude and personality scales." *Educational and Psychological Measurement* 24:255-69.
- 1966 "Authoritarianism scales and response bias." *Psychological Bulletin* 65 (January):11-23.
- Rokeach, Milton
1967 "Authoritarianism scales and response bias: comment on Peabody's paper." *Psychological Bulletin* 67 (May):349-55.
- Samuelson, Franz and Jacques F. Yates
1967 "Acquiescence and the F scale: old assumptions and new data." *Psychological Bulletin* 68 (August):91-103.
- Selznick, Gertrude and Stephen Steinberg
1969 *The Tenacity of Prejudice: Anti-Semitism in Contemporary America.* New York: Harper.
- Werts, Charles E., Robert L. Linn and Karl G. Jöreskog
1971 "Estimating the parameters of path models involving unmeasured variables." Pp. 400-10 in H. M. Blalock, Jr. (ed.), *Causal Models in the Social Sciences.* Chicago: Aldine.
- Zeitlin, Maurice
1967 *Revolutionary Politics and the Cuban Working Class.* Princeton, N.J.: Princeton University Press.

APPENDIX A

(a) *Selznick-Steinberg Index of Anti-Semitic Belief (A-S1)*

1. Do you think the Jews have too much power in the United States?
2. How about the business world, do you think the Jews have too much power in the business world?
3. Jews are more willing than others to use shady practices to get what they want.
4. Jews are more loyal to Israel than to America.
5. Jews are just as honest as other businessmen.
6. Jews have a lot of irritating faults.
7. International banking is pretty much controlled by Jews.
8. Jews don't care what happens to anyone but their own kind.
9. Jews always like to be at the head of things.
10. Jews stick together too much.
11. The trouble with Jewish businessmen is that they are so shrewd and tricky that other people don't have a fair chance in competition.

Items 1 and 2 were scored as follows in this paper: Yes = 2; No = 0; Don't Know = 1. Item 5 was scored: True = 0; False = 2; Don't Know = 1. The remaining items were scored: True = 2; False = 0; Don't Know = 1.

(b) *Selznick-Steinberg F-Scale*

1. No weakness or difficulty can hold us back if we have enough will power.
2. Sex crimes, such as rape and attack on children deserve more than mere imprisonment, such criminals ought to be publicly whipped, or worse.
3. Much of our lives are controlled by plots hatched in secret places.
4. Reading the stars can tell us a great deal about the future.
5. People can be divided into two distinct classes, the weak and the strong.

Items were scored as follows in this paper: Agree = 2; Disagree = 0; Don't Know = 1.

(c) *Selznick-Steinberg Anomie Scale*

1. Most people in government are not really interested in the problems of the average man.
2. You sometimes can't help wondering whether anything is worthwhile anymore.
3. Nowadays a person has to live pretty

much for today and let tomorrow take care of itself.

Items were scored as follows in this paper: Agree = 2; Disagree = 0; Don't Know = 1.

Note: our scoring of the items in the three scales above differs from that of Selznick and Steinberg, who gave respondents a score of 1 if they gave a prejudiced, authoritarian, or anomie response, and scored them zero otherwise.

(d) *Anti-Semitism Scale of Social Distance Feelings (A-S2)*

1. Suppose you had a child who wanted to marry a Jew who had a good education and came from a good family. How would you feel about this, would you object strongly (scored 4), somewhat (scored 3), a little (scored 2), or not at all (scored 0)?
2. How would you feel about having Jews in your neighborhood? Would you like to have some Jewish neighbors (0), wouldn't it make any difference to you (0), or would you prefer not to have any Jewish neighbors (4)?
3. Suppose your political party wanted to nominate a Jew for President of the United States, that is, a religious Jew who would go to synagogue every week the way a Christian goes to church every Sunday. Would this disturb you very much (4), somewhat (3), very little (2), or not at all (0)?

Note: in this scale, "Don't Knows" were scored intermediate between the tolerant response and the lowest possible prejudiced response, on the assumption that most "Don't Knows" were undecided about whether or not to be tolerant.

(e) *General Knowledge Scale*

Please look at this card and tell me whether each of the people I mention is a writer, someone in sports, a politician or an entertainer. Very few people would know all of the names, so if you don't know someone, just say so.

- | | |
|--------------------|-----------------------|
| 1. Roger Maris | 9. Y. A. Tittle |
| 2. Robert Frost | 10. William Faulkner |
| 3. William Holden | 11. Dean Rusk |
| 4. Vince Edwards | 12. Mark Twain |
| 5. William Miller | 13. Clarence Williams |
| 6. Herman Melville | 14. Douglas Dillon |
| 7. Frank Fontaine | 15. Arnold Palmer |
| 8. Hubert Humphrey | 16. Paul Newman |

Correct answer scored 1, else 0.

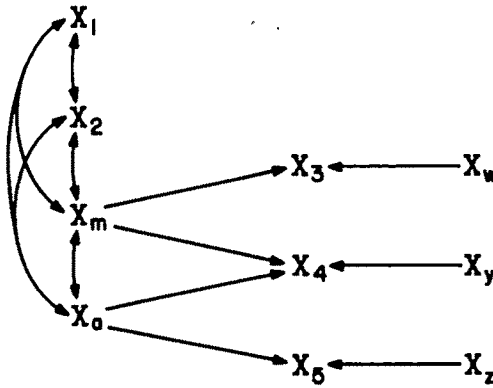


FIGURE 3. SIMPLIFIED FORM OF THE MODEL IN FIGURE 2.

APPENDIX B: SOLUTION FOR THE PATH MODEL IN FIGURE 2.

The three equations of the model in Figure 3 are:

$$\begin{aligned} X_3 &= P_{3m}X_m + P_{3w}X_w \\ X_4 &= P_{4m}X_m + P_{4a}X_a + P_{4y}X_y \\ X_5 &= P_{5a}X_a + P_{5z}X_z \end{aligned}$$

where X_w , X_y , and X_z are residuals assumed to be uncorrelated with each other and with all the predetermined variables of the model.

The three structural equations are each multiplied through by each of the other known variables, yielding a just-identified recursive structure with nine equations in nine unknowns:

$$\begin{aligned} r_{12} &= p_{2m}r_{1m} & (1) \\ r_{23} &= p_{3m}r_{2m} & (2) \\ r_{14} &= p_{4m}r_{1m} + p_{4a}r_{1a} & (3) \\ r_{24} &= p_{4m}r_{2m} + p_{4a}r_{2a} & (4) \\ r_{15} &= p_{5a}r_{1a} & (5) \end{aligned}$$

$$r_{25} = p_{5a}r_{2a} \quad (6)$$

$$r_{34} = p_{3m}p_{4m} + p_{3m}r_{ma}p_{4a} \quad (7)$$

$$r_{45} = p_{4a}p_{5a} + p_{4m}r_{ma}p_{5a} \quad (8)$$

$$r_{25} = p_{3m}r_{ma}p_{5a} \quad (9)$$

Rearranging equations (1), (2), (5), and (6):

$$r_{1m} = r_{1a}/p_{2m} \quad r_{2m} = r_{2a}/p_{2m}$$

$$r_{1a} = r_{15}/p_{5a} \quad r_{2a} = r_{25}/p_{5a}$$

Substituting into (3) and (4):

$$r_{14} = (p_{4m}/p_{2m})r_{15} + (p_{4a}/p_{5a})r_{15} \quad (10)$$

$$r_{24} = (p_{4m}/p_{2m})r_{25} + (p_{4a}/p_{5a})r_{25} \quad (11)$$

Rearranging equation (9):

$$p_{5a}r_{ma} = r_{25}/p_{2m} \quad p_{3m}r_{ma} = r_{25}/p_{5a}$$

Substituting into (7) and (8):

$$r_{34} = p_{3m}p_{4m} + (p_{4a}/p_{5a})r_{25} \quad (12)$$

$$r_{25} = p_{4a}p_{5a} + (p_{4m}/p_{2m})r_{25} \quad (13)$$

Solving (10) and (11), let

$$p_{4a}/p_{5a} = C_1 \quad (14)$$

$$p_{4m}/p_{2m} = C_2 \quad (15)$$

Substituting C_1 and C_2 into (12) and (13):

$$p_{4a}p_{5a} = C_2 \quad (16)$$

$$p_{4m}p_{2m} = C_1 \quad (17)$$

Equations (14) and (16) can be solved for p_{4a} and p_{5a} , and equations (15) and (17) can be solved for p_{2m} and p_{4m} .

With a solution obtained for the simplified form in Figure 3, the correlations among X_1 , X_2 , X_m , and X_a can be used to obtain the remaining path coefficients in Figure 2. For discussion of the specification and estimation of path models including unobserved variables see Costner (1969); Duncan, Featherman and Duncan (1968); Hauser and Goldberger (1971); and Werts, Linn and Jöreskog (1971).

THE INVOCATION OF LEGAL NORMS: AN EMPIRICAL INVESTIGATION OF DURKHEIM AND WEBER *

B. C. CARTWRIGHT

Yale University

R. D. SCHWARTZ

State University of New York at Buffalo

American Sociological Review 1973, Vol. 38 (June):340-354

This paper examines the ways in which legal norms enunciated by India's industrial tribunals are used by management and labor in their bilateral negotiations. Data from Indian courts and companies permit an empirical contrast between Durkheim's and Weber's analyses of the legal process. The evidence substantiates portions of both approaches: legal norms tend to be invoked as a function both of need for normative regulation and of the authority of the courts. A theoretical synthesis of the two positions is proposed.

INTRODUCTION

IN empirical theories of law, the contribution of law to social order is always problematic. The contemporary sociology of law approaches the classical problems of social order by asking the following questions:

1. *The development of legal processes:* Under what conditions will a differentiated legal process emerge in a society?
2. *The generation of legal norms:* How do legal processes formulate explicit normative guidelines and when are they employed?
3. *The dissemination and invocation of legal norms:* To what extent are legal norms disseminated and used throughout the population?¹
4. *The direction of legal impact:* Under what conditions will legal norms and the operation of the legal process divide as well as unite a society?

* The authors wish to thank H. Ball, D. Black, M. Bernstein, W. Felstiner, J. Guben, R. Kagan, R. Lempert, R. Pipkin, D. Ploch, J. Sanders, R. S. Warner and S. Wheeler for their helpful comments and suggestions on a previous draft of this paper. We are indebted to the Carnegie Corporation for a travel grant, to the Ford Foundation through Northwestern's Council for Intersocietal Studies for analysis funds, and to the Russell Sage Foundation for write-up support. We also want to acknowledge our gratitude to S. Rao, the co-author of our forthcoming monograph, *Law and the Accommodation of Interests*, whose friendship, patience, and concern for India and its laws made this study possible.

¹ By the term "invocation," we mean a specific reference to a norm in interactions with parties to whom the norm applies. It does not connote approval or disapproval, compliance or violation.

This paper examines mechanisms which influence the invocation of legal norms. How widely are these norms used and how is their use related to social situation and legal experience?

To explore these issues, we found it helpful to compare two views of legal invocations. The first emphasizes the parties' need for normative regulation. It sees the parties actively seeking, and adopting norms to affirm collective values and reduce the tensions of stressful situations. The second emphasizes the burdens of authoritatively imposed rules. It sees the law as a potential obstacle to the parties' interests, and assumes that the parties will try to minimize legal considerations except for the rare, intractable dispute that requires a judicial resolution.

The first view implies that legal norms will be widely invoked in a society; whereas, the second implies that legal invocations will be limited to groups (such as lawyers and litigants) having dealings with the state. Durkheim and Weber, respectively, represent these approaches to legal phenomena,² and we have chosen to use some of their substantive hypotheses as a framework for our analysis.

The empirical data for this paper derive from our research into the legal regulation of labor-management relations in two Indian jurisdictions (Greater Bombay and Mysore). They consist of a ten-year archival sample of labor-management disputes before the Indus-

² Our interpretation of Durkheim will be based on *The Division of Labor* (1964) rather than his later works.

trial Tribunals (1952-1962) and a questionnaire survey of factory managers in firms with one hundred or more employees (seventy-two firms in Bombay and 109 firms in Mysore). The basic dependent variable for this analysis will be the reference to Tribunal decisions in plant-level, labor-management discussions.

DURKHEIM AND WEBER AS LEGAL THEORISTS

Durkheim

In Durkheim's view, law provides a synthetic expression of cultural values. He hoped to demonstrate that the legal specification of cultural values contributed to social solidarity in both simple and complex societies, albeit in different ways. In simple societies, criminal law defined and heightened community boundaries; and in complex societies, the civil law resolved the normative strains created by the division of labor.

In his analysis of modern societies, Durkheim emphasized the special requirements imposed on the law by the growth of urban centers. For Durkheim, the division of labor was associated with high rates of contiguous interaction in densely populated areas (Land, 1970; Schnore, 1958). Durkheim conjectured that persons in such areas would be confronted by a highly differentiated society in which:

1. self sufficiency was impossible and therefore, survival required participation in multiple exchange transactions;
2. contrary to the classical market model that emphasized impersonal price mechanisms ["action at a distance"], exchange transactions required contiguous interactions for the transmission of goods and information;

and thus there would be,

3. no possibility for individuals to avoid the conflicts of uncertainty, coordination, and contingent interdependency that would arise in their exchange transactions with strangers.

In addition, Durkheim assumed that:

1. the immediate parties to a transaction could not resolve all the necessary ex-

change contingencies on their own initiative;³

2. by centrally accumulating the experiences of multiple transactions, legal institutions provided a set of normative solutions for nearly all the problems of particular parties;

he therefore concluded that,

3. contract law, the prototype of a modern legal institution, would be widely invoked in urban areas to expedite transactions by informing the parties of their potential reciprocities, reassuring them of their transactional partner's dependability, and providing moral if not legal sanction for performing normatively approved exchanges.⁴

In applying this analysis to labor-management relations in modern industry, Durkheim recognized that industrial workers faced additional strains: those created by large-scale industrial organization. He suggested that increases in a firm's employees led to an increased division of labor; and he hypothesized that a high division of labor would increase the rates, costs, and complexities of labor-management transactions: "... as industrial functions become specialized, the conflict becomes more lively, instead of solidarity increasing ... small-scale industry, where work is less divided, displays a relative harmony between worker and employer. It is only in large-scale industry that these relations are in a sickly state" (Durkheim, 1964: 354-6).

Therefore, provided that available laws reflect pervasive values and contribute to

³ "We can neither foresee the variety of possible circumstance in which our contract will involve itself, nor fix in advance with the aid of simply mental calculus what will be in each case the rights and duties of each, save in matters in which we have a very definite experience" (Durkheim, 1964:213).

⁴ "Contract-law is that which determines the juridical consequences of our acts that we have not determined. It expresses the normal conditions of equilibrium, as they arise from themselves or from the average. A resumé of numerous, varied experiences, what we cannot foresee individually is there provided for, what we cannot regulate is there regulated, and this regulation imposes itself upon us, although it may not be our handiwork, but that of society and tradition" (Durkheim, 1964:214).

harmonious relations between parties,⁵ Durkheim's overall views on invocations can be summarized by the proposition:

—since the rates and costs of exchange transactions and the occasions for potential conflict are greatly accelerated in large firms and urban areas, the invocation of legal norms will also be accelerated in large firms and urban areas.

Weber

In Durkheim's model, legal norms often resemble the products of an idealized public utility which operates with (a) insatiable demand; (b) negligible distribution costs; (c) no competition; and (d) minimal variations in the means, factors, and quality of production [all norms reflect the same decision-making technology and the same cultural ingredients]. Weber, on the other hand was primarily concerned with the historical and situational contingencies of legal authority; and he regarded both the generation and consumption of legal norms as highly problematic.

First, Weber assumed that legal decisions could not be adequately explained by cultural values and societal differentiation. He hoped to demonstrate that changes in the social organization of the legal process (particularly role changes) systematically modified the derivation and interpretation of legal norms. By stressing the variability of normative specifications, Weber viewed law as a constructed artifact shaped by the values and interests of particular legal functionaries.

Second, Weber had a high regard for both the multiplicity of norm-senders in a society

and the cognitive, problem-solving capabilities of most disputants. Although particular groups (like the early capitalists) might use legal norms to organize their affairs, Weber was skeptical about the utility of such norms for day-to-day transactions. Given the inevitable conflicts between the lay public's instrumental orientation and the "intrinsic necessities of logically consistent formal legal thinking" (Weber, 1968:885), Weber felt that legal norms were often placed at a competitive disadvantage by the remedies of non-legal authorities (those arising from religious, economic, and solidarity groups as well as from the disputants themselves).

Third, Weber was less optimistic than Durkheim about the universal dissemination of legal norms. He recognized that monopolizing a legal profession severely constrains the distribution of legal pronouncements. Lawyers have a professional interest in controlling access to legal knowledge, and their position would be seriously undermined if untrained laymen could readily learn and interpret the law.

A Weberian perspective would question the effectiveness of legal invocations as generalized mechanisms of conflict resolution and would search for ties between such invocations and the situational incentives controlled by legal functionaries. In particular, it would stress the temporal specificity of legal invocations and the importance of technical expertise in channeling legal norms to specific disputants.

Figure (1) summarizes these theoretical positions in two plausible, alternative models of legal invocations. The Weber model assumes that legal invocations are related to experiences with formal adjudication and the services of a professional legal counsel. The Durkheim model assumes that legal invocations are related to the normative problems created by large firms and cities. Both models form recursive, causal chains; and the Indian data provides empirical indices that can be used to examine the predicted relationships in these models.

THE INDUSTRIAL TRIBUNALS AS AN INSTITUTIONAL SETTING

Before examining the empirical data, it will be helpful to outline some of the institu-

⁵ Because of space limitations, this paper will not examine evidence on the perceived effectiveness, fairness, and legitimacy of judicial decisions. One can generally assume, however, that most Indian managers accept the necessity (if not the desirability) of legal regulations in labor-management relations. For example, when asked whether they support continuance of the industrial courts, 81% of the sample favored no change or minor changes in the courts; 14% favored major changes; and only 5% favored abolition. In addition, when asked about the effect of legal concepts on negotiations, 70% said that legal concepts made negotiations easier; 25% said they had no effect; and only 5% said they made negotiations harder (see our forthcoming monograph for a more detailed discussion of these issues).

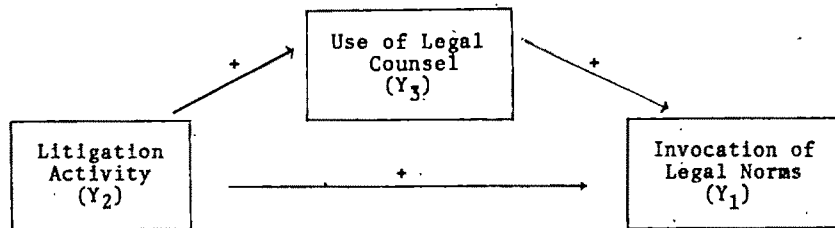
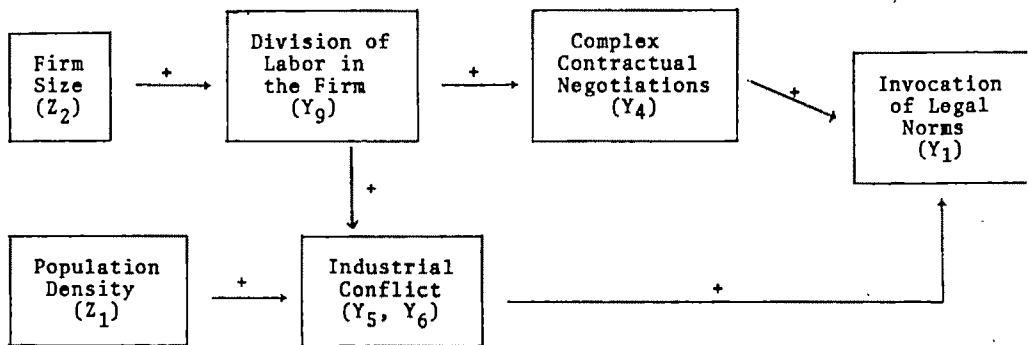
Weberian Perspective:Durkheimian Perspective:

Figure 1. Theoretical Models

tional characteristics of the Indian Industrial Tribunals. The Industrial Tribunals were established in 1947 as a form of quasi-judicial compulsory arbitration for labor-management disputes. They have jurisdiction over such issues as wages, hours, leave, job classifications, and retrenchment; and the presiding officers are usually former judges of the state supreme courts.

As a quasi-judicial, regulatory agency, the Tribunals issue sanctionable awards; but they are not required to conform to the procedural and evidentiary norms of the regular civil courts. For example, they have the authority to create binding collective contracts for all employees in a firm; and employers who fail to implement an award are liable for criminal penalties (imprisonment and fines) as well as civil suits. The Tribunals also have the authority to summon witnesses and documents, but they are not bound by the Indian Evidence Act and the parties need not be represented by legal counsel.

Over time, the Industrial Tribunals have

developed a variety of normative guidelines for resolving labor-management disputes. For example, the so-called Bonus Formula evolved by the Labor Appellate Tribunal (a national appellate court abolished in 1956) provided criteria for distributing surplus profits among shareholders, the firm, and the workmen. The Supreme Court of India accepted the general principles of the Bonus Formula in 1959; and in subsequent decisions, the Court has considered such issue as the definition of an integrated production unit and the determination of extraneous profits unrelated to worker productivity.

The dispute-resolution process has three main stages:

- (a) conciliation before a Labor Commissioner
- (b) Tribunal hearings
- (c) appellate hearing (High Court; Supreme Court).

A dispute is first registered with the Labor Commissioner who attempts to resolve it

through mediation and conciliation. If conciliation is unsuccessful, either the State Labor Ministry or the parties (by joint agreement) can refer the dispute to the Tribunal.

After receiving a dispute, the Tribunal holds a series of hearings to frame the issues and hear final arguments. During this period, the parties can reach an out-of-court settlement. The Tribunal is required to review all such settlements, and it can deny legal enforcement to those showing evidence of coercion. If an out-of-court settlement is not

reached, the Tribunal issues a binding award. If either party wishes to challenge the award, the Tribunal decision can be appealed to the High Courts and the Supreme Court under Article 136 of the Indian Constitution.

EMPIRICAL INDICES

The basic empirical problem is to determine who pays attention to Tribunal norms and why. Table (1) summarizes the empirical indices available in the Indian data. Several of these require additional comments.

Table 1. Variables

Notation	Theoretical Construct	Empirical Index	Scale
Y_1	Invocation of legal norms	Reference to decided cases in plant-level, labor-management discussions	Frequent (1) Infrequent (0)
Y_2	Litigation activity	Appearances before the Labor Commissioner, the Industrial Tribunals, or the appellate courts	Yes (1) No (0)
Y_3	Use of legal counsel	Consultation with a legally-trained adviser (staff lawyer or outside attorney) on matters other than litigation	Yes (1) No (0)
Y_4	Contractual complexities	Total number of labor-management agreements (1952-1962)	(0-10) agreements
Y_5	Industrial conflict	Total number of strikes and lockouts (1952-1962)	(0-15) strikes and lockouts
Y_6	Industrial conflict	The average number of written complaints per employee per month	(0-1) complaints
Y_7	Adjudication	Appearances before the industrial tribunals or the appellate courts	Yes (1) No (0)
Y_8	Frequency of hearings	Total number of hearings at each stage of the court process (1952-1962)	(0-26) hearings
Y_9	Division of labor in the firm	Total number of unions in the firm	(0-6) unions
Z_1	Population density	Population of town or city where the firm is located	(5,000-2.8 million) people
Z_2	Firm size	Total number of firm employees	(100-5,000) employees

Litigation Activities. Our data on litigation activities is based on responses in the firm questionnaire. Ideally, we would have recovered the issues and results of each court experience, but the present data are based on the number of cases settled at a particular stage of the Tribunal process for each firm. This means that the questionnaire data aggregate the overall court experience for each firm by using the firm (rather than the case) as the basic unit of measure.

Since we cannot trace the temporal sequence of each court experience, we cannot explore hypotheses of the form, "litigation on issue A at time t led to the invocation of norms concerning issue B during a non-litigated dispute at time $t+1$." However, by using the firm as basic unit measure, we can determine: (a) the total number of legal hearings for each firm [aggregated across all stages of the Tribunal process]; and (b) the highest level of the Tribunal process reached by each firm. This means that we can subdivide the total sample into,

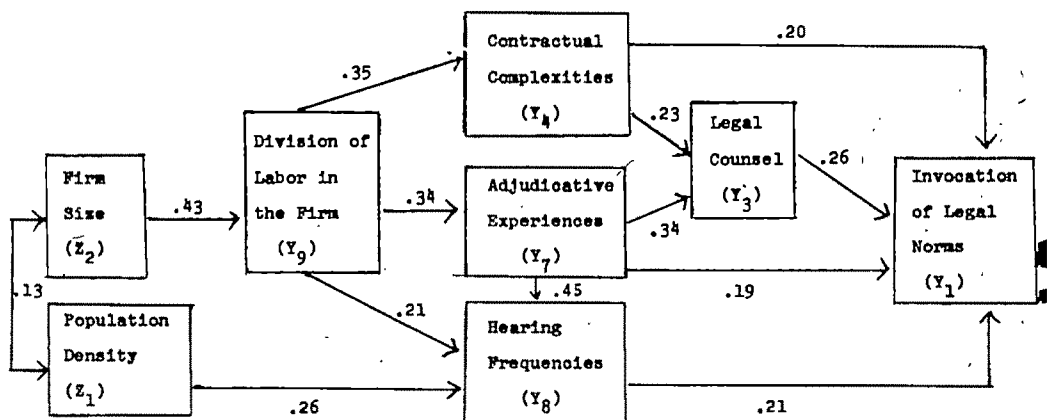
1. firms that have no contact with the Tribunal system [$n = 42$];
2. firms that have only had contact with the Labor Commissioner [$n = 44$];
3. firms that have experienced at least one Tribunal award but no appellate decisions [$n = 37$];

4. firms that have experienced both Tribunal awards and appellate decisions [$n = 32$].

Since the formality of the dispute resolution process increases with each stage of the Tribunal system we can tap both the presence and absence of court experiences as well as variations in the mediative-adjudicative quality of litigating experiences at different stages of the process.

Division of Labor in the Firm. Functional differentiation in an organization usually refers to the development of systematic role structures. It is often measured by the number of job classifications and authority levels in a firm. For Durkheim, however, the critical element in functional differentiation is not the multiplying of specialized roles per se but the multiplying of interdependent exchange units. Durkheim was primarily interested in the normative problems of exchange transactions (to which a body of contract law could be applied) rather than the problems of private administrative actions. Since unions are the basic unit for conducting labor-management transactions, it seems appropriate to treat the number of unions in a firm as an index of plant-level functional differentiation.

Contractual Problems. The presence of multiple unions in a plant creates serious



- (1) These linear path coefficients are standardized beta coefficients estimated from equations with the insignificant parameters removed. Owing to the problems of interpreting high correlations among cross-products, no interaction relationships are included (Althausen, 1971). The residual correlations among the six equations in the model range from 0.00 to 0.12 with a mean correlation of 0.04.

Figure 2. Final Integrated Model⁽¹⁾

contractual problems in India. Except for certain areas in Bombay, Indian labor law has no provision for selecting a single representative union. This means that a separate labor-management agreement must be negotiated with each union. Collective contracts binding all workers in a firm can only be secured by a Tribunal award or a Labor Commissioner settlement.

Since Indian unions are generally small, highly transitory, and mutually competitive (S. M. Panday, 1966), it seems likely that firms with multiple unions face a complex set of negotiations; and one can hypothesize that Tribunal guidelines become an important ingredient in formulating labor-management issues. To measure this contractual complexity, we selected an index that measures the total number of labor-management agreements for a firm during the period (1952-1962). This index assumes that firms with many agreements are more likely to face an unstable set of negotiating patterns than firms with few agreements.

Legal Invocations. The content of legal invocations can vary widely from the citing of general legal principles and doctrines to the citing of specific court decisions. From a methodological viewpoint however, one must be cautious in selecting an invocation index. Indices framed in terms of general normative principles (such as standards of good faith and due process) often make it difficult to differentiate between legal and cultural norms. We need to identify normative guidelines that have been generated and institutionalized within the legal process (as opposed to traditional folkways).

Hence, we chose a conservative index of legal invocations that refers to the citation of court decisions in labor-management negotiations. Table (2) summarizes the responses of plant managers to the question, "In negotiations with labor, how often is there reference to decided cases?"^{6,7} The

Table 2. "In Negotiations with Labor, How Often Is There Reference to Decided Cases?"

Case Reference	N	Percentage
Never	65	36.6
Rarely	29	16.3
Sometimes	46	25.8
Often	27	15.2
Everytime	11	6.1
Total	178	100.0
No response	3	

(particularly if it implies that the legal officials rather than the disputants are citing the decided cases).

We do not have observational data on plant level interactions, but the questionnaire contains some evidence on labor-management negotiations outside the formal litigation process. (a) The firm managers were asked to discuss their reasons for entering labor contracts. Of firms who had Tribunal or appellate awards, five entered a contract because of a court award; fourteen did so to avoid litigation with the court; and forty-six to avoid a combination of litigation and strikes. It seems plausible that any firm which enters a contract to avoid legal litigation is likely to engage in labor-management negotiations outside the formal courtroom setting.

(b) In a section of the questionnaire dealing with pay and bonus issues, the firm managers were asked to list the number of pay changes in the last ten years prompted either by court decisions or bilateral negotiations. By examining the dates given for these pay changes, it was possible to divide the sample into two categories: firms for which there was an overlap between bilateral activity and Tribunal litigation for the same period of time; and firms which had bilateral negotiations on pay issues and no pending Tribunal litigation. The results tend to support the existence of pay negotiations outside the Tribunal litigation process. First, of the firms that engaged in pay negotiations, over two-thirds reached a settlement without Tribunal litigation. (Ninety-five firms engaged in pay negotiations, and of these sixty-nine had no pending Tribunal litigation.) Second, for firms having Tribunal or appellate awards, pay settlements outside the court were more prevalent than for cases that involved a mixture of bilateral negotiations and Tribunal litigation: 36.6% of the Tribunal-appellate firms engaged in non-Tribunal pay negotiations; whereas, 26.7% of the Tribunal-appellate firms combined Tribunal litigation with bilateral negotiations. Third, for firms who reached bilateral pay settlements, negotiating activity was more likely to occur

⁶ Although this section of the firm questionnaire refers to "labor negotiations and discussions in your factory," one might be skeptical about the prevalence of plant-level negotiations in India and hypothesize that most of these negotiations are actually disguised legal hearings. Since approximately 75% of our sample had at least some contact with the legal system, this is a serious rival hypothesis

distribution indicates a high variation in relative frequency of case citations. 52.9% of the firms never or rarely referred to decided cases; and 47.1% of the firms referred to decided cases sometimes, often, or every time. One might argue that this index is biased against Durkheim (in that litigants are more likely to cite decided cases than non-litigants). However, if it can be shown that court opinions are invoked beyond the litigant pool, the plausibility of the Durkheim interpretation would be greatly strengthened.

RESULTS

In the analyses to follow, we will first examine the Weber and Durkheim models (Figure 1) and then try to integrate them in a single model. The statistical results for these models are summarized in Table (3). The equations are estimated by least squares, and (t) values are used to determine the presence or absence of a statistically significant causal path.

The construction of the integrated model will proceed dialectically by asking, at each

among those with court experience than among those without: 86.9% of those who reached a bilateral pay settlement had some contact with the legal system (including the Labor Commissioner proceedings); whereas, 13.1% of those who reached a bilateral pay settlement had no contact with the legal system. These results tend to support the view that firms with Tribunal-appellate experience also engage in labor negotiations outside the legal system; and, therefore, it is likely that Tribunal decisions are cited in negotiations outside the formal adversary process.

⁷ Given the structure of the question on case references, it is possible that a firm which refers to decided cases "every time" only makes constant reference to its own past cases. To check this rival hypothesis, we compared both the questionnaire responses and the court schedules for all firms that appeared in both the firm and Tribunal samples (a total of twenty-two firms). Part of the questionnaire, not discussed in this paper, asked the respondents to check a list of legal terms and issues used in their factory. Of the twenty-two firms in the joint firm-court sample, two used exactly the same terms that appeared in their Tribunal cases; whereas, twenty used a greater number of terms (the probability of a significant Chi square difference is 0.001). While these results are not conclusive, it seems likely that most firms invoke other cases besides their own in discussing labor-management issues.

step how Durkheim and Weber would be likely to interpret the empirical results. Dialogues of this kind are, of course, highly speculative; but we have tried to capture the spirit of Durkheim and Weber in these interpretations, and we hope that the reader will regard the dialogue as a plausible reconstruction of basic theoretical positions rather than a literal exegesis.

The Weber Model. The relationships in the Weber model seem to be highly confirmed. Both litigation activity and the use of legal counsel increase the frequency of case citations in labor-management discussions. For example, in comparing the presence or absence of litigating experiences (where litigation refers to cases before the Labor Commissioners, Tribunals, or appellate courts), 83.3% of the firms with no court experience had a low reference to decided cases; and 16.7% had a high reference to decided cases. Conversely, 42.5% of the firms with some court experience had a low reference to decided cases; and 57.7% had a high reference to decided cases (for a 40.8% difference in case references for firms with and without litigation experience).

The Durkheim Model. The relationships in the Durkheim model are more problematic. The Durkheim model postulates two causal chains: the first chain links firm-size to case references (with the division of labor and contractual complexities as intervening mechanisms), and the second chain links population density to case references (with industrial conflict as an intervening mechanism). The results seem to confirm the first chain and only partly confirm the second.

In the first chain, each variable is sequentially related, and each intervening mechanism absorbs the effects of the preceding stages. For example, firm size is related to the number of unions in a plant (division of labor); the number of unions is related to the total number of labor-management agreements (contractual complexities); and there appears to be no causal path between firm size and the number of agreements (see equation D-2). In addition, there is a direct causal path from contractual complexities (Y_4) to legal invocations (Y_1).

In the second chain, the end-points (population and legal invocations) are positively related, but the causal links to the interven-

Table 3. Parameter Estimations¹

Equations	Final Equations	Dependent Variable	t-Test Values										R ²	Theoretical Predictions		Statistically Significant Parameters
			Y ₂	Y ₃	Y ₄	Y ₅	Y ₆	Y ₇	Y ₈	Y ₉	Z ₁	Z ₂		Weberian Perspective	Durkheimian Perspective	
																t>1.96
<u>Weberian Model:</u>																
W (1)		Y ₃	5.83										19.1	Y ₂		Y ₂
W (2)		Y ₁	3.07	4.47									26.4	Y ₂ , Y ₃		Y ₂ , Y ₃
<u>Durkheimian Model:</u>																
D (1)		Y ₉								1.71	6.10	19.8		Z ₂		Z ₂
D (2)		Y ₄								3.40	1.75	0.73	14.4	Y ₉		Y ₉
D (3) - A		Y ₅			0.18					1.32	0.80	2.06	8.6	Y ₉ , Z ₁		Z ₂
D (3) - B		Y ₆			1.31					0.69	1.64	1.55	5.4	Y ₉ , Z ₁		
D (4)		Y ₁			2.99	1.40	1.75			1.79	2.09	0.16	24.1	Y ₄ , Y ₅ , Y ₆		Y ₄ , Z ₁
<u>Integrated Model:</u>																
I (1)	✓	Y ₉								1.71	6.10	19.8		Z ₂		Z ₂
I (2)	✓	Y ₄								3.40	1.75	0.73	14.4	Y ₉		Y ₉
I (3) - A		Y ₂			1.44					3.80	3.51	0.15	25.2	Y ₉ , Z ₁		Y ₉ , Z ₁
I (3) - B	✓	Y ₇			0.80					2.57	1.40	1.31	14.2	Y ₉ , Z ₁		Y ₉
I (3) - C	✓	Y ₈			0.80			5.80		2.81	3.54	0.63	42.8	Y ₉ , Z ₁		Y ₇ , Y ₉ , Z ₁
I (4)	✓	Y ₃			1.98			2.92	1.53	1.05	1.01	0.14	26.0	Y ₄ , Y ₇ , Y ₈		Y ₄ , Y ₇
I (5) - A		Y ₁	2.23	3.66	2.43					0.94	1.38	0.14	32.2	Y ₂ , Y ₃ , Y ₄ , Y ₅ , Y ₆ , Y ₇ , Y ₈		Y ₂ , Y ₃ , Y ₄
I (5) - B	✓	Y ₁		2.91	2.54			2.20	2.13	0.44	1.05	0.17	40.1	Y ₂ , Y ₃ , Y ₄ , Y ₅ , Y ₆ , Y ₇ , Y ₈		Y ₂ , Y ₃ , Y ₄ , Y ₇ , Y ₈

¹The coefficients in this table were estimated by ordinary least squares. Since the dependent variable is a (0,1) binary variable, this estimating procedure violates the assumption of homoscedastic disturbances and produces biased and inconsistent estimators for the standard errors of the coefficients (Johnson, 1963: 227).

Various probit and logit transformations have been proposed to correct this problem (Finney, 1964; Goodman, 1972; Theil, 1970), but, unfortunately, they are not directly applicable to small samples with multiple, continuous independent variables (since relative cell frequencies are required to derive the various probability estimates). In addition, the two-stage variance correction suggested by Goldberger (1964: 249-250) is only applicable to equations where the estimated values of the dependent variable fall inside the range (0,1) [equations I(3)-C, I(4), and I(5)-B had approximately 5% of their estimated values greater than 1.0 or less than 0.0].

By examining the equations where the two-stage procedure was applicable, however, we were able to determine that, in all cases, ordinary least squares produced a more conservative estimate than the two-stage procedure [e.g., in equation I(3)-B, the *t* value for Y(4) was 0.80 for ordinary least squares and 0.98 for the two-stage procedure]. Therefore, rather than introducing a set of ad hoc decision rules (such as applying different estimating procedures to different equations or partitioning the continuous independent variables to meet the logit requirements), we have decided to report the ordinary least square as probably the most justifiable procedure under the circumstances [Ashenfelter, 1969, reaches a similar conclusion; for a sample size of 1,393, and 34 variables, he found that the (*t*) value estimates from ordinary least squares and the two-stage procedure deviated from each other within the range, 0.69 to 0.02, with a mean deviation of 0.26].

Table 3a. Observed Correlations

	Y ₁	Y ₂	Y ₃	Y ₄	Y ₅	Y ₆	Y ₇	Y ₈	Y ₉	Z ₁	Z ₂
Y ₁	1.00	.36	.45	.41	.18	.21	.44	.46	.33	.28	.23
Y ₂		1.00	.41	.29	.24	.21	.54	.54	.41	.35	.19
Y ₃			1.00	.30	.07	.18	.38	.38	.33	.20	.14
Y ₄				1.00	.14	.12	.22	.28	.34	.22	.28
Y ₅					1.00	.04	.34	.27	.22	.12	.22
Y ₆						1.00	.15	.13	.08	.15	-.09
Y ₇							1.00	.56	.34	.14	.25
Y ₈								1.00	.42	.37	.21
Y ₉									1.00	.17	.43
Z ₁										1.00	.13
Z ₂											1.00

ing mechanisms (strikes and complaints) are relatively weak. Except for a slight relationship to firm size, the intervening mechanisms do not seem related to either antecedent or subsequent variables in the model. For example, the division of labor in a plant does not seem to produce strikes and complaints; and when controlled for population density and contractual complexity, no causal link appears to exist between strikes and complaints and the frequency of legal invocations.⁸

Litigation as a Durkheimian Variable. These results suggest that both court experiences and contractual complexities are related to legal invocations, but the data do not seem to support the conflict mechanisms in the Durkheim model. One could argue that industrial conflict requires a more re-

finer set of strike indicators (see, for example, Britt and Galle, 1972), or one could argue that industrial conflict needs to be explained by independent variables that stress worker homogeneity and geographical isolation (Kerr and Siegel, 1954). The Indian data do not permit us to examine these explanations; but in the interest of integrating these two models, it should be noted that a Durkheim analysis of industrial conflict is not limited to strikes and complaints. Litigation is a prime form of labor-management conflict, and the results of the Weber model suggest that litigation provides an alternative index for measuring normative tensions.

Since the original Weber model treated litigation (i.e., propensity to go to court) as an exogenous or unexplained variable, litigation can be incorporated into the Durkheim model as an endogenous or explained variable. This formulation uses litigation activity (Y₂) as an empirical index of normative stress, and it predicts that court cases will be concentrated in structural arrangements (such as densely populated cities and firms with a high division of labor) where it is difficult to mobilize established cultural understandings.

The data tend to support the revised Durkheim model (see equation I-3a). There is a significant causal link from both population density (Z₁) and the number of unions in a plant (Y₉) to the presence of litigation

⁸ At this point in the analysis, one could modify the second chain by dropping the conflict variables and establishing a direct link from population density (Z₁) to legal invocations (Y₁). This formulation would be compatible with Durkheim, but it would not reflect his emphasis on the role of normative strains in urban areas. Without conflict variables in the model, there is no way to distinguish between tension-reduction and improved communication networks as rival mechanisms for the presence of case citations in urban areas. In addition, this formulation offers no clear-cut advantage over the Weber model (since both models would explain approximately the same proportion of variation in legal invocations).

activity (Y_2). For example, firms in metropolitan areas are more likely to engage in litigation than firms in rural areas. In Greater Bombay 88.9% of the firms have had some form of court experience; whereas, in rural Mysore the figure is 53.6% (for a 35.3% difference).

Although litigation seems to be associated with large cities and multiple unions, one need not accept a Durkheimian interpretation of these results. Litigation in urban areas may reflect greater physical access to the courts rather than higher tension levels, and litigation in multiple union firms may indicate opposition to specific court decisions rather than a general state of anomie. To Weber, however, the critical problem lies in assuming that litigation is a sign of strain rather than a social process in which variations in the kind and quality of experiences can affect participant outcomes. Weber would be skeptical of the notion that conflict per se has much causal impact on legal invocations. He would contend that the potential for authoritative interventions provides the incentive for introducing legal norms into labor-management discussions. Rather than emphasize conflict relations, Weber would assume that litigation is an authority relation with legal functionaries and that the parties invoke legal norms pri-

marily to predict and control the outcomes of legal decisions.

Internal Court Processes as a Weberian Variable. If the parties are primarily responding to the demands and expectations of legal functionaries, one would expect them to respond to structural differences at different stages of the court process. From our field observations, it appears that the formality of the dispute-resolution process increases as one moves up the appellate system. The Labor Commissioner hearings provide an informal conciliation process in which the parties are brought together and encouraged to settle their dispute by mediation. The Labor Commissioner may discuss various legal precedents in the course of the proceedings, but all issues must be resolved by the consent of the parties; and he has no authority to issue binding, unilateral decisions. On the other hand, the Tribunals and the appellate courts have the right to resolve the parties' substantive disagreements; and within the context of a formal adversary system, they often refer to legal precedents in making their judgments.

If Weber is correct, it seems likely that legal norms will become increasingly important to the parties as a dispute moves up the appellate system. Table (4) summarizes the results on the effect of court experiences on

Table 4. The Effect of Court Participation on Case Reference

Case Reference ¹	Level of Court Participation								Total	
	No Legal Experience		Labor Commis- sioner Only		Tribunal Award ²		Appellate Decision			
	N	%	N	%	N	%	N	%	N	%
Low	35	83.3	28	63.6	13	35.1	7	21.9	83	53.5
High	7	16.7	16	36.4	24	64.9	25	78.1	72	46.5
Total	42	100.0	44	100.0	37	100.0	32	100.0	155	100.0

$$\chi^2 = 34.73 \quad \chi^2(p) = .0001 \quad \lambda_b = .40 \quad \text{Gamma} = .66 \quad \text{Sommers D} = .35$$

¹The low category combines the responses for never and rarely. The high category combines the responses for sometimes, often, everytime.

²The firm questionnaire allowed us to distinguish between Tribunal awards and pre-award compromises for the period 1958-1963 but not for the period 1952-1957. Therefore, the Tribunal award category refers only to known awards in the 1958-1963 period, and it excludes 23 firms that had Tribunal cases in the 1952-1957 period (where it was impossible to determine whether or not the firm participated in Tribunal deliberations).

legal invocations at different stages of the process. The results indicate a strong tendency for the rate of case references in plant-level negotiations to increase for firms that have had experience at higher levels of the process. Of the firms with Labor Commissioner experience only 36.4% had a high reference to decided cases; whereas, 78.1% of the firms with appellate court experience had a high reference to decided cases. As the Weberian perspective predicted, the major threshold in case reference occurs between the presence or absence of formalized dispute resolution: the percentage difference in case references between Labor Commissioner hearings and Tribunal-appellate hearings is 34.7% (a difference of 36.4% vs. 71.1% for firms with a high case reference); while, the percentage difference in case reference between Tribunal hearings and appellate hearings is only 13.2% (a difference of 64.9% vs. 78.1% for firms with a high case reference).

Hearing Frequencies as a Durkheimian Variable. While these results tend to confirm the expectation that a positive, monotonic relationship exists between case reference and the level of specialized litigating experience, a potential rival hypothesis needs to be investigated. Given that the Tribunal system is a sequential decision process, it follows (with minor exceptions) that cases do not reach the higher levels of the decision process without passing through all the lower levels. This means that the total number of legal hearings for a firm increases as the firm moves up the appellate levels. As one might expect, our data indicate a significant increase in the mean number of legal hearings for firms that have reached different stages of the Tribunal process: the mean number of hearings is 3.7 for firms with only Labor Commissioner experience; 6.3 for firms with Tribunal but not appellate experience; and 9.1 for firms with appellate experience. This cumulative process makes it possible for firms with a large number of Labor Commissioner cases to have the same number of total hearings as a firm with a small number of appellate cases. In particular, this means that results in Table (4) confound the level of Tribunal experience with the frequency of legal hearings; and they do not differentiate between frequency and quality of experience

as rival interpretations of the relationship between court participation and the invocation of decided cases.

Since the Durkheimian perspective is primarily concerned with the structural sources of reoccurring disputes [rather than the decision processes of different legal functionaries], in explaining legal invocations it is important to control simultaneously for both the level of Tribunal experiences and the volume of Tribunal litigation. Equation D(4) summarizes those results and indicates, contrary to Durkheim, that both the level and volume of litigation are major determinants of case references. In addition, by examining the unadjusted beta coefficients for each variable, one can give a fairly precise idea of the relative effect of conciliation and adjudication on case reference. The unadjusted beta coefficients are .02 for the total number of hearings and .19 for the presence of either Tribunal or appellate awards. This means that it takes approximately eleven hearings at the Labor Commissioner stage for a firm to reach the same expected level of case references as a firm that had one hearing at the Labor Commissioner stage and one award at the Tribunal-appellate stage [$.02(11.5) + .19(0) = .23$; $.02(2) + .19(1) = .23$].

These results suggest that while both conciliation and adjudication are related to legal invocation, adjudication seems to have a greater marginal effect.

The Presence of Lawyers as a Weberian Variable. At this stage of the argument, a Durkheimian would have difficulty claiming that authority levels within the Tribunal system do not affect invocations differently. [One might be able to explain these results by suggesting that the appellate courts handle more serious disputes and offer a higher quality of norm-specification than conciliation proceedings; but within the limits of our data, there is no way to check these additional hypotheses.] The existence of authority-level effects, however, does not seriously limit Durkheimian theory as long as the empirical results confirm the existence of direct causal links from contractual complexities (Y_4) and conciliation hearings (Y_8 controlled for Y_7) to legal invocations. Since the Durkheimian perspective has been able to identify some of the structural determinants of who

goes to court, it might be willing to concede the fact that adjudicative experiences foster legal invocations as long as it is clear that legal norms are being mobilized for a wide variety of disputes outside formal adjudicative channels.

To counter this interpretation, a Weberian needs to question the viability of making direct causal links to legal invocations from contractual complexities and conciliation hearings. Such links imply that high levels of strain [plus advice the parties receive from the Labor Commissioner] provide sufficient conditions for the parties to seek legal information for their own problems. A Weberian would doubt this assumption by asserting that attention to legal information requires a specialized role for lawyers. Since lawyers generally have a monopoly on legal training and skills, a Weberian would assume that legal invocations are primarily the function of trained counsel, and not laymen.

To check this expectation, let us examine two ways of formulating the effect of lawyers on plant case references. One would be to postulate a direct causal link between normative strains and the use of legal counsel: it would suggest that situations of high normative strain are precisely those where the parties are likely to turn to legal counsel, and that the presence of these lawyers (rather than the levels of strain) is the critical intervening mechanism for introducing legal

norms. A second hypothesis would argue that, regardless of the causal links, there is an interaction relationship such that the strain variables (agreement and hearing frequencies) have no effect on case references in the absence of lawyers and a positive effect in the presence of lawyers.

Equation I(4) summarizes the evidence for the first hypothesis. The results indicate that lawyers are strongly related to the presence of Tribunal and appellate class but only minimally to the indices of normative strain: the link to hearing frequencies is not statistically significant and the link to contractual complexities is only marginally significant (the *t* probability is .05). These results suggest that lawyers are primarily used at later stages of a conflict (particularly at the higher appellate levels), but there is no indication that they are important in the early stages. In addition, while lawyers are moderately associated with contractual complexities, they do not absorb the full effects of these complexities on legal invocations. Part of the effect of these complexities is transmitted through the presence of lawyers, but a direct causal link from the number of agreements to case reference also remains.

The results of the interaction hypotheses further support a direct association between contractual complexities and legal invocations.

Table (5) summarizes these results for the

Table 5

THE EFFECT OF LAWYERS ON CASE REFERENCES: AN INTERACTION TEST

Independent Variables	Firms with No Tribunal-Appellate Awards			
	Lawyers Absent		Lawyers Present	
	Standardized Beta-Value	t - Test Probability	Standardized Beta-Value	t - Test Probability
Contractual Complexities (Y_h)	.766	< .001	.303	.042
Labor Commissioner Hearings (Y_g)	-.021	.851	.364	.015
Analysis of Variance	F(2;35) = 24.16		F(2;42) = 10.37	

effect of lawyers on case references for firms not subject to the Tribunals' formal sanctions (firms with no court experience or Labor Commissioner experience only). As expected, the strain variables have a positive effect on case reference in the presence of lawyers. The (t) probability is 0.042 for the total number of agreements and 0.015 for the total number of Labor Commissioner hearings. However, the results are mixed for the absence of lawyers. The total number of Labor Commissioner hearings is not related to variations in case reference [the (t) probability is 0.85], but the total number of agreements is strongly related to such variations. The correlation between case reference and the total number of agreements is 0.76, and the relevant (t) value is 6.82 with 35 degrees of freedom. These results suggest, contrary to Weber, that a firm's contractual complexities may be sufficient for legal invocation even in the absence of lawyers.

CONCLUSION

The main purpose of this paper has been to investigate some of the conditions under which the general public invokes legal norms in its day-to-day conduct. Based on our data from the Indian industrial tribunals (summarized in Figure 2), the invocation of legal norms is affected both by the structural need for normative guidance, stressed by Durkheim, and by the distinctive experience of parties in the formal court setting, emphasized by Weber. Structural strain occasioned by the division of labor increases the likelihood of court experience, while court experience contributes independently to the invocation of legal norms.

According to Durkheim, the law integrates a differentiated society by combining centralized policies with decentralized dispute resolution (by invoking authoritative norms, disputes can be settled without intermediate functionaries). Durkheim assumed, analogous to an economic market, that demands for state-produced norms reflect structural strains rather than activities of the state itself. In the Indian system, however, the demand for legal norms seems to be shaped both by structural strains and socializing

experiences with legal functionaries. To what extent this system can also facilitate decentralized dispute resolution remains an important question for future research.

REFERENCES

- Althausen, Robert P.
1971 "Multicollinearity and non-additive regression models." Pp. 453-72 in H. M. Blalock, Jr. (ed.), *Causal Models in the Social Sciences*. Chicago: Aldine-Atherton.
- Ashenfelter, Orley
1969 "Some statistical difficulties in using dummy dependent variables." Pps. 644-8 in William G. Bowen and T. Aldrich Finegan (eds.), *The Economics of Labor Force Participation*. Princeton: Princeton University Press.
- Black, Donald J.
1973 "The mobilization of law." *Journal of Legal Studies* 2 (January):125-49.
- Britt, David and Omer R. Galle
1972 "Industrial conflict and unionization." *American Sociological Review* 37 (February):46-57.
- Durkheim, Emile
1964 *The Division of Labor in Society*. New York: Macmillan Company and the Free Press.
- Finney, D. J.
1964 *Probit Analysis*. Cambridge: Cambridge University Press.
- Goldberger, Arthur S.
1964 *Econometric Theory*. New York: Wiley.
- Goodman, Leo A.
1972 "A modified multiple regression approach to the analysis of dichotomous variables." *American Sociological Review* 37 (February):28-46.
- Johnston, J.
1963 *Econometric Methods*. New York: McGraw-Hill Book Company.
- Kerr, Clark and A. Siegel
1954 "The interindustry propensity to strike: an international comparison." Pp. 189-212 in Kornhauser et al. (eds.), *Industrial Conflict*. New York: McGraw-Hill.
- Land, Kenneth C.
1970 "Mathematical formalization of Durkheim's theory of division of labor." Pp. 257-82 in Edgar F. Borgatta (ed.), *Sociological Methodology* 1970. San Francisco: Jossey-Bass.
- Mayhew, Leon H.
1971 "Stability and change in legal systems." Pps. 187-210 in Bernard Barber and Alex Inkeles (eds.), *Stability and Social Change*. Boston: Little, Brown and Company.
- Panday, S. M.
1966 "The Indian labor movement: growth and character." *Indian Journal of Labour Economics* 9 (April):14-34.

- Parsons, Talcott
1967 "Durkheim's contribution to the theory of integration of social systems." Pp. 3-34 in *Sociological Theory and Modern Society*. New York: The Free Press.
- Schnore, L. F.
1958 "Social morphology and human ecology." *American Journal of Sociology* 63 (May): 620-34.
- Thell, H.
1970 "On the estimation of relationships involving qualitative variables." *American Journal of Sociology* 76 (July): 103-54.
- Trubek, David M.
1972 "Max Weber on law and the rise of capitalism." *Wisconsin Law Review* 3: 720-53.
- Weber, Max
1968 *Economy and Society*. Guenther Roth and Claus Wittich (eds.). New York: Bedminster Press.

ON PHENOMENOLOGICAL SOCIOLOGY *

JAMES L. HEAP AND PHILLIP A. ROTH

University of British Columbia

American Sociological Review 1973, Vol. 38 (June): 354-367

The works of Tiryakian, Bruyn and Douglas are examined as representative of "phenomenological sociology." Radical problems are discovered in their use of key concepts in phenomenology: intention, reduction, phenomenon and essence. These problems are shown to arise out of a failure to grasp the nature of the phenomenological enterprise and its relationship to sociology. Turning back to the original formulation of this relationship by Husserl, we discover problems of transcendental intersubjectivity, of type and essence, and of objectivism. We then point out the existence of sociologies which do not share the shortcomings of what is called phenomenological sociology, yet which make use of the perspective and approach of phenomenology. We then focus on one of these sociologies, ethnomethodology in its relation to phenomenology. We find parallels in their methodology and domains of inquiry, and divergence in their approaches to intersubjectivity.

THERE is increasing interest in something called "phenomenological sociology." If this interest is to be sustained, indeed if this sub-discipline is to contribute to our knowledge of the social world, we must become clear on what phenomenological sociology is and can become. At present serious problems exist in the writings of many sociologists who have contributed to, and implicitly defined, this approach to sociology. In general, they display only a metaphorical understanding of phenomenology as a philosophy and as a set of methods. In addition, and partly as a result, they fail to understand the relationship between sociology and phenomenology. However, if we go back to Edmund Husserl's original formulation of the relationship, we once again face serious problems. Our

purpose in this essay is to explicate these problems and point out the existence of sociologies which do not share the shortcomings of what is called "phenomenological sociology," yet which make use of its perspective and approach. One of these sociologies, ethnomethodology as developed by Harold Garfinkel, will be briefly treated in our final section.

From the few authors who have contributed to phenomenological sociology we have selected the work of Tiryakian (1965), Bruyn (1966), and Douglas (1970) as representative. Tiryakian's study in the *ASR* of the affinity between phenomenology and the mainstream of sociological tradition is one of the earliest and most often quoted. Bruyn's discussion of social phenomenology appears in his book *The Human Perspective in Sociology*. Douglas' discussion of various phenomenological sociologies appears in one of the most important edited volumes in the field of ethnomethodology. In view of the availability of these statements, we take them to be important for how phenomeno-

* Revised version of a paper presented at the Pacific Sociological Association annual meetings, April 1972, Portland, Oregon. The authors wish to thank Thomas P. Wilson, John O'Neill and Richard T. Darville for their helpful comments and suggestions.

logical sociology is understood and defined by sociologists.

THE PROBLEM OF METAPHOR

The one problem Tiryakian, Bruyn, and Douglas have in common is that *they use the concepts of phenomenology "metaphorically."* That is, they use the terms as understood in everyday conversation rather than as they are meant in phenomenology. They fail to recognize these terms' intended domain of reference and thus to recognize the transformation of meaning they undergo when used to refer to an utterly different domain. In the instances of conceptual confusion to be discussed, all three authors make claims which they present as consistent with the philosophy or methods of Husserl (1962). Our criticisms have therefore been formulated solely along Husserlian lines.

Intention

The concept of intention is greatly misunderstood and misrepresented by Tiryakian. He quotes W. I. Thomas as defining attention as "the mental attitude which takes note of the outside world and manipulates it" (Tiryakian, 1965:682). Then we are told that the notion of attention "is equivalent to the phenomenological notion of 'intention'" (1965:682). In order to see how far afield Tiryakian is, it is worthwhile to cite what Thomas had to say about attention after he had defined it:

... attention does not operate alone; it is associated with habit on the one hand and crisis on the other. When the habits are running smoothly the attention is relaxed; it is not at work. But when something happens to disturb the run of habit the attention is called into play and devises a new mode of behaviour which will meet the crisis (Thomas, 1951: 218).

Phenomenologically, intention is not something that at one point is "not at work" and at another point "called into play." As Husserl said "all experiences in one way or another participate in intentionality" (1962: 222). Consciousness is fundamentally intentional: it is always consciousness of something, "*consciousness of an object whether real or ideal, whether existent or imaginary*"

(Gurwitsch, 1966:124). One of the main points of Husserl's program was that Descartes' formulation was incomplete: *ego cogito* must be expanded to *ego cogito cogitatum* (Husserl, 1962:105). The relationship between *cogito* and *cogitatum* was worked out in terms of Husserl's theory of intentionality which transcends the objectivist and subjectivist positions by dealing with the object as perceived (the *noema*) and the perception of the object (the *noesis*), or better yet, "the intended object" and the act of consciousness which intends that object, "the intentional act." Thus the concept of intention must not be confused with Thomas' attention; for intention is an essential feature of consciousness prior to the operation of attention.¹

The phenomenological concept of intention is also misunderstood by Douglas, although to a lesser degree, when he equates it with the concept of purpose. In his paper entitled "Understanding Everyday Life" he declares that

... as Schutz, following Husserl and other phenomenologists, has argued so well, it is primarily intentions at any time—our purposes at hand—that order human thought, that determine the relevance of information and ideas about the world and ourselves (1970:26).²

While it is true that intentionality can be equated with purpose, this is so only at the predicative level of experience, the level of judgment, of action in Weber's sense. However, Husserl's theory of intentionality refers also, and most significantly, to the pre-predicative level. This is the level of immediate

¹ Berger (1966) caught this misuse of "intention" but in doing so called into question whether Thomas in fact had ever used a concept called "attention." Tiryakian (1966:262) responded that Thomas had used the concept of "attention" and said that it certainly does "imply *intentional* direction of consciousness to the outside on the part of the subject." True, but not in a phenomenological sense: consciousness isn't "intentionally" directed—consciousness is intentional.

² Parenthetically he adds that "John Heeren in Chapter 2 has provided the best analysis of this intentional theory of consciousness." Examining Heeren's article (in Douglas, 1970:45-6) we find no discussion of the "intentional theory of consciousness." Rather we find a discussion of the actor's purposes at hand, his pragmatic interest in the world of daily life.

experience, of perception. (cf. Merleau-Ponty, 1962), of so-called non-meaningful behavior in Weber's sense (cf. Schutz, 1967: 54-7).

Reduction

Husserl's concept of reduction is used only metaphorically by Tiryakian and Douglas. In demonstrating that "Durkheim's sociological analysis is really phenomenological" Tiryakian discusses the "implicitly phenomenological approach" taken by Durkheim in his study of suicide.

The "surface" manifestations of suicide establish its presence as a social phenomenon; these objective quantitative factors are then "reduced" phenomenologically to underlying layers of the social structure in which the act of suicide occurs . . . (1965:681).³

We are told, however, that "Durkheim stops short of a 'transcendental reduction'" (1965:681). Tiryakian also takes the position that Simmel, "although he diverged from Husserl in some respects . . . sought to reduce manifestly different concrete forms of social phenomena to their underlying characteristics ('forms')" (1965:680).

The reduction is misunderstood here as operating in the empirical realm. While there are at least three types of reduction—eidetic, psychological, and transcendental—all treat intended objects or intentional acts within the a priori realm of possibilities.⁴ In this realm, through a method of imaginative variation, the phenomenologist can freely vary the objects or acts of consciousness. He does so to discover what is a priori,

i.e., essential to every possible appearance of the object or act within the empirical world, the realm of actualities. To secure the a priori realm the empirical world must be "bracketed." In that Tiryakian footnotes Schutz' first volume of *Collected Papers*, it is surprising that he had not grasped the radical nature of the reduction, of "putting the world in brackets," as discussed by Schutz.⁵

The phenomenologist does not deny the existence of the outer world, but for his analytical purpose he makes up his mind to suspend belief in its existence—that is, to refrain intentionally and systematically from all judgments related directly or indirectly to the existence of the outer world. . . . What we have to put into brackets is not only the existence of the outer world, along with all the things in it, inanimate and animate, including fellow-men, cultural objects, society and its institutions . . . but also the propositions of all the sciences (1962:104-5).

What is left after the initial reduction, is the intended object and the intentional act. While the transcendental reduction is more complex in that it requires bracketing one's own mundane existence as a human being within the world, we shall not take our explication further; for by now it is clear that Durkheim and Simmel were hardly engaged in phenomenological reductions. One does not reduce quantitative factors "down" to "underlying layers of social structure," for social structure itself must be bracketed. The common nuclear meanings which Simmel induced from the repetitive aspects of social life could not have been grasped via reduction, for these meanings themselves have to be bracketed.

Besides those like Tiryakian who argue that the reduction has been used unknowingly by sociologists, there are others who suggest it as a research method.⁶ Douglas declares that

³ For an "implicitly phenomenological approach" to suicide, with apropos criticisms of Durkheim's approach, see Jacobs (1967). While Jacobs seems to have tacked on the word "phenomenological" to the title of his study, a closer investigation reveals that his work satisfies Schutz' postulates of subjective interpretation and adequacy (1962:43-4). To the degree that this is the case, he can be said to be doing one form of "phenomenological sociology," or more precisely a *phenomenologically founded sociology* (cf. Schutz, 1967).

⁴ Strictly speaking, a reduction furnishes a reduced sphere which allows access to the realm of possibilities. See Lauer (1958:52). While somewhat misleading, for simplicity we shall treat the realm of possibilities as the domain of reference of all phenomenological concepts; but see Schutz (1962: 113).

⁵ Husserl uses reduction and epoché interchangeably, but some phenomenologists treat the two as different sides of the same coin (Natanson, 1962:14). The epoché is negative in that it brackets reality; whereas the reduction is positive, having to do with the character of the phenomena obtained through bracketing (Lauer, 1958:50). For simplicity we shall not distinguish between the two.

⁶ George Psathas (1971:6) is quite explicit in making such a recommendation.

... accepting presuppositions as necessary, there obviously remains that vast realm of common sense, of everyday experience that can be phenomenologically *bracketed*, that is, towards which one can take a theoretic stance and reflect upon until the basic elements and relations of the phenomenal experience are discovered (1970:22).

Not only does Douglas assume that his "theoretic stance" is a phenomenological reduction, but he also claims that "phenomenological interactionists," such as Blumer and Becker, as well as ethnomethodologists use the theoretic stance, i.e., phenomenological reduction (1970:19, 16). No sociologist brackets the existence of the world. Sociology's interests, problems, and solutions are not to be found in the realm of possibilities. While some ethnomethodologists do employ a transformed version of the reduction, they make it quite clear that theirs is not to be confused with phenomenological reduction. In the very volume which Douglas edited and within which his article appears, Zimmerman and Pollner (1970:98) state:

The term reduction is borrowed from Husserl (1962). While the notion of an occasioned corpus partakes of Husserl's program, the order of phenomena revealed by its use is by no means offered as equivalent to that which appears by virtue of the use of the phenomenological reduction.

In our final section we shall discuss the similarities and differences between phenomenological and ethnomethodological reduction. Suffice it to say that neither should be confused with methods used by symbolic interactionists.

Phenomenon

The concept of phenomenon is highly problematic. Would-be phenomenological sociologists (Psathas, 1971:2) seem drawn to Husserl's dictum "to the things themselves," by which Husserl meant a return to the phenomena as given in immediate consciousness. By phenomenon Husserl meant that "which, having been subjected to the phenomenological reduction, is purified from the reality attributed to it by naive consciousness" (Spiegelberg, 1971:722). A phenomenon qua phenomenon only becomes available when we cease to treat an object⁷

as real, and begin to treat the object as meant, as intended, as it appears.

Sociologists (Schur, 1971:124), however, understand "things" and phenomena in a strictly mundane, and therefore metaphorical manner. Arguing that Durkheim's methodology is not antithetical to an existential-phenomenological viewpoint, Tiryakian (1965:680) declares that "On the contrary, 'consider social facts as things,' has for Durkheim the same import and meaning as Husserl's dictum 'to the things themselves'." Furthermore, we are told that Durkheim's approach "is grounded in accepting social facts as *sui generis* phenomena of intersubjective consciousness, as products of social interaction" (1965:680). Social facts are hardly phenomena, for they are theoretic abstractions from what is given in experience within the empirical realm. They are not "*prior to all* 'theory,'" as Husserl (1962:95) required. They are not arrived at through phenomenological reduction.

Bruyn (1966:94-5) shares a similar problem when he suggests "phenomenological inquiry into the nature of social phenomena." By the latter he understands the referents of such sociological concepts as primary groups, social institutions, religion, society, etc. (1966:94). Since his discussion draws upon Husserl we feel warranted in inquiring how the referents of these concepts are to be phenomenologically reduced and examined. These referents transcend the immediate experience of any observer and furthermore would seem to have their existence through theoretical abstraction from immediate experience. Thus it is difficult to see how Bruyn can treat these referents as phenomena in Husserl's sense. *That is not to say that it cannot be done*, for Husserl (1962) held that all objects of consciousness can be reduced to phenomena. However it is no simple matter. Unless this point is made and understood it is almost inevitable that sociologists will read "phenomenon" in Durkheim's mundane sense of social fact (1938:14). Bruyn seems unaware of this point. Thereby he perpetuates the metaphorical misunderstandings he himself is involved in.

⁷ "Object" is to be understood in the widest sense,

as any object of consciousness, including thought objects.

While Douglas takes his theoretical stance to be a phenomenological reduction, he does not seem to realize that phenomena are made available only through reduction. For Douglas (1970:15) the reduction is simply another (though preferred) stance toward the everyday world as a phenomenon. Other stances are the absolutist and the natural (1970:13-14). All stances however are presented as sharing phenomena: the "phenomena of everyday life," which Douglas (1970:4) argues to be social action, in Weber's sense (1968:4). Social actions "must be studied and explained in terms of their situations and their meanings to the actors themselves" (1970:4).

The problems involved are multiple. First, Douglas fails to recognize the radical difference between ethnomethodology's and conventional sociology's so-called phenomena. This is the same mistake which Denzin (1970) makes and to which Zimmerman and Wieder (1970) respond in articles appearing in the very book which Douglas edited and in which his article appears. Drawing from another article in that book we can say that whereas interpretive sociology seeks to interpret social action, ethnomethodology treats "the interpretive process itself as a phenomenon for investigation" (Wilson, 1970:78).

Secondly, from within the reduction how are we as observers able to grasp the subjective meanings of the acts of others? Douglas fails to mention or recognize this problem of intersubjectivity within the reduced sphere. For Husserl this problem required a transcendental solution, which he attempted, but failed (Schutz, 1966:51-91). Douglas does not even offer a solution at the mundane level. Without explicating intersubjectivity, it is difficult to understand how, from within the reduction, we are to study and explain observed social action as phenomenon. The difficulty lies in the fact that in order to treat social action as a phenomenon we have to bracket the existence of actors qua fellow-men. The difficulty subsides, however, if we understand phenomenon and reduction metaphorically.

Essence

Finally we come to the troublesome concept of essence. After discussing the work

of the early phenomenological sociologists (cf. Martindale, 1960:267-82), Bruyn (1966:44) declares that "A conceptual contribution which European phenomenology may make to American field studies lies in the term *essence* . . . the work of the social phenomenologist becomes one of interpreting anew the meaning of essence in social theory." Thus he suggests that the term essence "may be applied to such concepts as primary group, social institutions, values, society, religion, beauty, morality, or whatever sociological phenomenon might be studied" (1966:94).

The questionable utility of the concept of essence for the sociologist will be clearer if the concept is clarified. Essence may be taken to be that intuited invariant quality without which the intended object, the phenomena, would not be what it is (Husserl, 1962:45-51). This is not to be confused with any notion of the "defining characteristics" or "necessary features" of objects in the empirical factual world. Essence has as its reference the a priori realm of possibilities which precedes that of actualities (Husserl, 1962:213). As such, it is intuited from the intended object, the object as experienced, as perceived, the object as noema.⁸ It is arrived at through the method of reduction and imaginative variation discussed above. Defining characteristics, on the other hand, are arrived at a posteriori, through logical operations (deduction, induction) based on factual knowledge about actual objects in the taken-to-be real world. Only the latter are the concern of empirical sociology, for it is in the real world that sociological problems and their solutions are to be found.

While it is not clear that Bruyn under-

⁸ As Gurwitsch (1966:132), following Husserl, has explained, "The noema is to be distinguished from the real object . . . the 'perceived tree as such' [the noema] varies according to the standpoint, the orientation, the attitude, etc., of the perceiving subject, as when for instance he looks at the tree from above, or at another time perceives it while in the garden." Whereas the real object, the tree, is known to be an oak, to have a root system, to have a more or less round trunk, to be losing its leaves, etc., the "perceived tree as such" consists of only that which I perceive, of that which is immediately given to me in my experiencing act (cf. Husserl, 1962:240-5).

stands phenomenon in a phenomenological sense, it is quite clear that he misconceives the concept of essence. After having discussed Husserl, he distinguishes between seeking the essence of a construct and the essence of a social belief: "If it is a social belief, it should not be determined by the scientist's own theoretical musings, but rather by what is inherent in the minds of those who hold it" (1966:95). Nowhere does he discuss how this is to be done phenomenologically. No theory of intersubjectivity is provided nor is any mention made of reduction or imaginative variation.

While contrasting essences with ideal types he states that "Max Weber was uneasy about essences in ideal types and feared the value judgments implied in them" (1966:95). Bruyn makes no distinction, however, between Weber's and Husserl's use of essence. Later, in discussing Weber and essences, Bruyn (1966:117-18) quotes Weber: "All expositions, for example, of the 'essence' of Christianity are ideal types enjoying only a necessarily very relative and problematic validity, when they are intended to be regarded as the historical portrayal of empirically existing facts" (Weber, 1949:97). Essences in Husserl's sense are *a priori*, formal, invariant, not relative and of problematic validity. They exist and are to be discovered within the realm of possibilities, not within the empirical realm. Bruyn does not recognize the domain of reference of the concept essence. Hence his use of that concept is metaphorical.

Given the misuse of the major concepts in phenomenology, i.e., intention, reduction, phenomenon and essence, by Tiryakian, Douglas and Bruyn, we must conclude that Tiryakian's thesis of the affinity between phenomenology and leading figures in the sociological tradition, Douglas' discussion of various phenomenological sociologies⁹

and Bruyn's suggestions for a social phenomenology, are fundamentally flawed and misleading. The problem of metaphor, however, is not simply lexical. It betrays a thoroughgoing failure to recognize and grasp the radical nature of Husserl's enterprise. Any attempt to graft phenomenological concepts onto a sociology which has not been fundamentally reconstituted can only lead to a distortion, if not perversion, of both phenomenology and sociology.

THE PROBLEM OF EIDETIC SOCIOLOGY

Sociology's realm is the empirical world. Husserl, however, held the position that parallel and prior to every empirical science there could and should be an eidetic science, e.g., an essential sociology within the realm of possibilities (1962:55-7). This essential sociology would provide a rigorous foundation for empirical study, in line with the belief that the relation of phenomenology to the social sciences is foundational.

Through the methods of reduction and imaginative variation, the essence and essential relationships of such objects of empirical sociology as society and the family, would be clarified. This essential sociology itself, however, would presuppose and be founded on an eidetic science of the structures of the life-world (cf. Kockelmans, 1967:104), the world within which empirical sociology finds its subject matter. Each and every eidetic science then would be ultimately founded on transcendental phenomenology, for only within the transcendental realm can the constitution of the world be clarified. Since all eidetic sciences would presuppose the world, this clarification is necessary. Only radical "presuppositionless," *a priori* knowledge could serve as an adequate foundation for science. Until such knowledge is attained the sciences would continue to have foundation problems¹⁰ (Husserl, 1962:19). They

⁹ Douglas (1970:32) treats Sacks and his followers as "linguistic phenomenological sociologists." This is unfortunate: (1) the compatibility of linguistic analysis and phenomenology is highly controversial (cf. Natanson, 1962:34-43; TeHennepe, 1965:133-46), (2) Sacks does not define his work as phenomenological, but instead (3) states in his dissertation that some of his work "may be viewed as somewhat analogous to the analysis of syntactic structures in formal linguistics, or componential

analysis by anthropologists" (1966:14). For a critique of semiology from a phenomenological perspective and a discussion of recent developments in the phenomenology of language see Paul Ricoeur (1967b).

¹⁰ In sociology, the abiding problem has been the appropriate model of science. For a discussion of foundational problems in sociology see Wilson (1970).

would continue to be confused as to the human meaning and significance of their findings (cf. Husserl, 1970a).

Setting aside the program of transcendental phenomenology for a moment, we can ask whether an eidetic sociology is possible. In recommending the incorporation of essence into the corpus of sociological concepts, Bruyn presupposes this possibility. In fact, such a science was attempted by students and contemporaries of Husserl, most notably Max Scheler. In terms of strict Husserlian phenomenology though, they were unsuccessful (cf. Schutz, 1962:140-2, 150-79; Neisser, 1959:210-11; Spiegelberg, 1971:266-7). They often provided a wealth of insights (cf. Scheler, 1954), but their findings were not essential, not *a priori*. Scheler in fact was forced to reverse his position on findings he once held essential (Spiegelberg, 1971:266).

The question of an eidetic sociology is complex. This complexity follows from Husserl's theory of essence (1962:45-71; Levinas, 1967; Kockelmans, 1967:77-105), the changes it underwent (Merleau-Ponty, 1964a:43-95) and the cogency of the criticisms leveled at it (Dufrenne, 1966:75-84; Merleau-Ponty, 1969:105-29; Neisser, 1959; Schutz, 1966:92-115). Space does not permit a detailed discussion, nor is it our purpose to render a final judgment. Instead we wish to point to the thorny problems sociologists must face if they wish to do an eidetic sociology.

If we wished to describe the essence of a psychological phenomenon which did not imply the existence of others, e.g., action as experienced, it would be possible. Of course, such a description would be of our own experience of action within the reduced sphere. Indeed, Schutz (1967:45-96) provided such a description. Sociology's objects, however, are social. The existence of others are presupposed. If we wish to do an eidetic description of social action, even our own, we run into Douglas' problem, discussed above. A theory of intersubjectivity is required and implied.¹¹ By definition, social action takes

account of, and is oriented to the behavior of others (Weber, 1968:4).

Within the empirical, mundane realm a theory of intersubjectivity seems possible (cf. Zaner, 1961). Husserl's approach (1970b: 89-151), unfortunately, led him into the transcendental realm, where the problem of intersubjectivity seems insoluble (Lauer, 1965:148-62; Ricoeur, 1967a:115-42). Through the use of the phenomenological-psychological reduction Husserl (1970a: 238) held that it is possible for the social scientist to understand and describe the intentional acts of his subjects. By extension, it would seem that the meaningful action of others can be grasped.

However, social scientists who wish to follow Husserl at this point are led into the problem of transcendental intersubjectivity. The social scientist must first perform the reduction on himself (1970a:253), which reveals the social scientist as the "*absolutely single ego*" (1970a:256), the transcendental Ego. In so doing, his subjects are transformed into a phenomenon. In order to account for the existence of others and a world in common, "there necessarily occurs a transformation of the phenomenological-psychological epoché and reduction into the *transcendental*" (1970a:256). The knotty problem must then be faced of how others are constituted in the social scientist (*qua* transcendental Ego) through his own intentional acts. As Schutz (1966:51-91) has skillfully argued, Husserl's theory of transcendental intersubjectivity not only fails, but is addressed to a pseudo-problem. Intersubjectivity "is not a problem of constitution which can be solved within the transcendental sphere, but is rather a datum (*Gegebenheit*) of the life-world" (Schutz, 1966:82).

If we withdraw from the transcendental sphere and treat the world and intersubjectivity as ontologically given, as do Schutz (1966:89) and Merleau-Ponty (1964b:151-81), we can still inquire as to the possibility of a strict eidetic sociology. A theory of intersubjectivity is still required, but only at the mundane level. Presupposing such a theory (cf. Schutz, 1967:97-138; Merleau-Ponty, 1962:346-65), a problem still remains.

An essence is always intuited from an "example," a phenomenon which corresponds to

¹¹ That Scheler's findings were not *a priori* may be traceable partly to his defective theory of intersubjectivity (Scheler, 1954:213-64; Schutz, 1962: 150-79) on which he based his search for essences. That theory involved the positing of a highly questionable supra-individual consciousness.

a factual or imaginary object. Husserl has said that immediately given phenomena are grasped and explicated according to types (1970a:220). That is, the life-world and all objects in it are experienced in their typicality, e.g., a typical house, a typical concert, a typical day, an atypical child (cf. Schutz, 1962:7-27). Natanson (1970:5) has called ours a "typifying consciousness." This leads to the question, though, of whether or not the essence is preconstituted by the type in terms of which we have experienced the object in the mundane world. Schutz (1966:115) argues that this is in fact the case, and that "there is indeed merely a difference of degree between type and *eidos*" (essence).

Since types are "vacillating approximations" generated in terms of actors' pragmatic purposes (cf. Schutz, 1970:63-4), they are not vehicles to be driven beyond their own pragmatic destination. Descriptions of the essence of typified social phenomena may very well go beyond how those phenomena are seen, experienced and accomplished by the actors themselves. Furthermore, since types are contingent and somewhat arbitrary, there is no guarantee that the typified object with which one begins will bear any determinate and unequivocal relationship to the phenomena being sought.

Systematic sampling offers a solution, but to opt for that solution is to vitiate the search for eidetic knowledge. Reasoning from samples leads to a posteriori knowledge. If the promise of a strict eidetic sociology is to be fulfilled only the methods of phenomenology may be used, e.g., reduction and imaginative variation. If only these methods are used, though, it is difficult to see how we can ever arrive at a priori knowledge of the essential structure of social phenomena. On the other hand, if these are our sole methods, our approach begins to take on features of the very objectivism (cf. Schutz, 1964:4) which would-be phenomenological sociologists have opposed. We may be led to make claims about "what social phenomena really are" which run counter to claims put forward by actors.¹²

¹² Koestenbaum's suggested use of phenomenology in the study of status (1966:336) seems to us, in fact, to suggest a type of result incompatible with Schutz' Weberian postulate of adequacy (1962:44).

Clearly, the search for an eidetic sociology faces many seemingly insurmountable problems. The social presupposes a theory of intersubjectivity. Husserl required and formulated such a theory at the transcendental level, but was unsuccessful. Even if we refuse his transcendental invitation and remain on mundane ground, we find that the search for *eidos* is circumscribed and prejudiced by its origin: the typified life-world. To seek for the essence beyond the type is to risk forsaking interpretative understanding with little assurance of finding a priori knowledge. In the end, even if we put aside these problems, we might well ask whether it is even wise to assume that social phenomena have a Husserlian a priori.

PHENOMENOLOGICAL SOCIOLOGIES

The foregoing would seem to suggest that even properly understood, the possibility of a phenomenological sociology in the sense envisioned by Husserl is highly questionable. However, we should realize that Husserl actually knew little of the concrete problems of the social sciences (cf. Schutz, 1962:140). In fact some sociologies which have been called phenomenological are closer to the spirit, if not the letter of phenomenology than the pertinent statements of its founder. What distinguishes these sociologies from those we have criticized is that *the following involve and invoke no claim that what is done actually is, or is the same as, Husserlian phenomenology.*

Here we wish to discuss briefly three of these sociologies in terms of their phenomenological features. A fourth type, ethnomethodology, will be elaborated in our final section. These types are neither exhaustive nor strictly drawn. Rather, they are somewhat arbitrarily drawn analytical devices offered simply to point out and distinguish between different programs in what can be called phenomenological sociology.

Hopefully, in noting and distinguishing between types of phenomenological sociologies we may prevent further confusion among critics (Schur, 1971; Neisser, 1959; Goldstein, 1963; Pivcevic, 1972). So far, they have sought to question the possibility and utility of something they have called phenomenological sociology, usually without realiz-

ing their arguments are only aimed at one type. Or, worse yet, they are aimed unknowingly and indiscriminately at different types. Finally, and unfortunately, their arguments reveal a failure to understand completely any of the types they have addressed.¹⁸

Type I can be called phenomenological in a loose sense because, whether it is realized or not, it makes use of a phenomenological philosophical perspective. This perspective is one which stresses "the primacy of consciousness and subjective meaning in the interpretation of social action" (Natanson, 1962:157). Natanson (1962:165) has identified W. I. Thomas, Cooley, Mead, and Weber with this approach; but it would not be out of order to identify this as the perspective of those working within what Wilson (1970) has called the interpretive paradigm (cf. Shearing and Petrunik, 1972). Natanson (1962:157) cautions, however, that "Obviously the label 'phenomenological' is less than satisfactory for this total approach, since it neither derives directly from the philosophy of Edmund Husserl nor is always philosophically compatible with principles of Husserlian phenomenology." Neither intersubjectivity nor the natural attitude are thematized in Type I.

Type II adopts an explicit and clarified phenomenological philosophical perspective as its foundation. This phenomenologically founded sociology follows from the studies of Alfred Schutz (1967) in which the latter clarified Weber's concept of action and his method of ideal type construction. In his studies Schutz revealed the invariant formal structures of the life-world (cf. 1966:116):

the realms of manipulation (1962:306) and of others (1962:15), and the systems of relevance and typification (1970).

This "ontology of the life-world," which Husserl had called for (1970a:173), provides a true a priori framework within which, and with reference to, sociologists necessarily discover and resolve their problematic. Though Schutz did an eidetic science, his was not an eidetic sociology, for he did not seek the essence of discipline-defined "social phenomena." Rather, he sought to elucidate the a priori structure of the world in which sociological phenomena are apprehended. Rather than seeking the essence, e.g., of corporations, the state or society, Schutz turned to the structures of the life-world, which those phenomena presuppose, e.g., a world of contemporaries beyond our reach grasped through socially distributed and pragmatically generated typifications, etc.

For Schutz these structures included mundane intersubjectivity as an ontological given. He began, but never completed, a theory of intersubjectivity (1967:97-138; Zaner, 1961). The life-world, intersubjectivity, and the natural attitude (cf. Husserl, 1962:91), that is, the attitude of naive belief in the existence of the world, were examined by Schutz through the eidetic science which he called the "constitutive phenomenology of the natural attitude" (1962:132). The type of sociology founded on Schutz's phenomenology is continued most notably by Berger and Luckman (1966) and Holzner (1968).

Type III also is phenomenologically founded on the structures of the life-world, but it uses what may be called a phenomenological approach. Traveling under the banner of reflexive sociology, Type III can be understood as a philosophically radicalized version of the type of sociology Gouldner (1970) called for (cf. Zaner, 1971). Drawing upon Husserl, Schutz, Merleau-Ponty (1964b:98-113; cf. O'Neill, 1970) and Garfinkel (1967), this first person approach is predicated on the recognition that sociology is *in* and *about* the very life-world that it studies. This leads to a rejection of the ideal of an absolute observer and requires that research be carried out in a manner which preserves the presence of the observer (cf. Darville, 1972). In so doing, this ap-

¹⁸ Schur (1971:115-36) fails to distinguish and understand the differences between Types I, II, and IV. He misunderstands the latter in a rather common fashion (cf. Denzin, 1970; Zimmerman and Wieder, 1970). Nelser (1959) criticizes the major figure of Type II (Schutz) in terms of the inappropriate criteria of strict eidetic sociology. Goldstein (1963) misconceives phenomenological sociology as being solely a first person enterprise. In so doing he confuses features of Type III with Type II, and fails to grasp how the latter develops an observer's account of social action. Pivcevic (1972) confuses Type I, II, and III. He rejects rather than disproves Type II because he uses an objectivist notion of what is social (cf. Schutz, 1964:5-6). Thus he fails to grasp the meaning of social (action) within Weber's framework.

proach seeks to warrant its claims of knowledge through explicating the grounds for those claims.

This Type is phenomenological in the sense that it

... purports nothing less than accounting for the world, its objectivity, and the unquestioned certainty of its existence in subjective terms, or to put it differently, revealing the world as a correlate and product of subjective functions, activities and operations (Gurwitsch, 1966:416).

Understood as critical in a phenomenological sense (cf. Zaner, 1970), reflexive sociology seeks "to make explicit those structures that remain merely *implicit* and taken for granted" (Zaner, 1970:82). It does so in the belief that "Unless the respondent's and researcher's decoding and encoding procedures are basic elements of the research enterprise, we cannot make sense of either the phenomena being studied or the materials labeled 'findings'" (Cicourel, 1968:3). Intersubjectivity and the natural attitude do become thematized in this approach. While there are wide differences in their work, O'Neill (1972), Smith (1972a, 1972b) and Cicourel (1968) can be identified with this approach.

PHENOMENOLOGY AND ETHNOMETHODOLOGY

Type IV, ethnomethodology, has a distinctive relationship to both sociology and phenomenology. It is a program of inquiry which combines certain phenomenological and sociological concerns while transforming them in such a way as to do violence to neither but, rather, to constitute for itself a unique and independent domain of study. Note, however, that the following exposition by no means deals with all of what currently travels under the rubric of ethnomethodology. Rather, our attention is restricted to explicating the ethnomethodological program of Harold Garfinkel (1967) as found in the writings of Pollner, Wieder, and Zimmerman (in Douglas, 1970). Using their formulations as a resource, we will now sketch some of the affinities of the ethnomethodological program with phenomenological concerns and the ways in which it transforms those concerns to establish its own distinctive domain of investigation.

To begin with, the attitude which constitutes the ethnomethodological domain differs from the "attitude of everyday life" (the natural attitude) which constitutes the domain common both to lay members and conventional sociological analysts in a manner akin to the way that the phenomenological attitude differs from the natural attitude. Under the natural attitude, the objects of the domain of everyday life are believed to exist independently of the mode of inquiry addressed to them. Both phenomenology and ethnomethodology suspend or "bracket"¹⁴ the belief that such objects are independent of the mode of inquiry used to make the objects observable. The phenomena thus made available for phenomenological and ethnomethodological inquiry differ in their constitution from the phenomenon of the natural attitude. They also differ from each other by virtue of the particular form of reduction used by each (cf. Zimmerman and Pollner, 1970:98).¹⁵ For phenomenology, objects in the "real" world are reduced to objects of immediate consciousness and are seen as constituted in and through intentional acts of consciousness. For ethnomethodology, the "objective" features of the social world are reduced to the interpretative procedures by which that world is assembled and accomplished in concrete, ongoing, social situations. For phenomenology, the foundational nexus of meaning in the world is immediate consciousness; for ethno-

¹⁴ A distinction must be made here regarding the attitude assumed by the ethnomethodologist. That attitude brackets the ontological status of any social world "external" to the directly observed social situation, but takes as given the objective reality of the observed "situated practices." To the degree that it gives the latter ontological status, it parallels the natural attitude in having a mundane, objective, intersubjectively verifiable domain. As Pollner (1970) pointed out, however, it is a precondition of all forms of inquiry, including the phenomenological variety, to have an "objective" domain. Phenomenological inquiry differs from the other two which concern us in that its domain is intersubjectively verifiable, but no claim is made for the objective reality of that domain. The very question of "reality" is bracketed, but the resulting phenomena are treated as unquestionably given. Thus, naively speaking, the domain is "objective."

¹⁵ It is perhaps useful to observe that the term "reduction" derives from the Latin compound "reducere," which means "to lead back to origins."

methodology, the foundational nexus of meaning in the social world is the immediately present, directly observed social situation.¹⁰ For both, anything transcending this nexus ("real" objects "outside" of consciousness; "objective" features of society "outside" of situations wherein their sense is recognized by members) is bracketed or "put out of play" with regard to the task of describing or accounting for the features of their respective domains. The domain of phenomenological inquiry, then, consists solely of the recognizable structures of immediate consciousness; while the domain of ethnomethodological inquiry consists solely of members' situated practices which produce for themselves and for observers the *sense* of objective social structures.

Note also that the fundamental notion of intersubjectivity receives a characteristically different placement in these domains. In the phenomenology of Schutz, intersubjectivity is viewed as an ontologically given feature of the social world, and analysis is directed toward the constitutive attitudes and beliefs that make such a viewpoint possible for members of the social world. In contrast, intersubjectivity enters the ethnomethodological domain as the *sense* of intersubjectivity contingently accomplished by members' situated practices. The transformation here involves a shift from the realm of the *a priori* to that of the contingently actual—the *a priori* becomes a problematic feature of actual accomplishment. In contradistinction to both these conceptions, however, the question of intersubjectivity never enters the domain of the natural attitude in a thematized form as a topic or object of study in its own right. Rather, intersubjectivity

resides at its foundation as an unexamined but essential presupposition.

Perhaps enough has been said to display yet another similarity between phenomenology and ethnomethodology: the structurally similar character of the misunderstandings to which both are often subject. These misunderstandings typically fail to recognize that the domains of reference and the language used to describe the objects of those domains are *radically* different in constitution (and, hence, meaning) from the domain of reference constituted by the natural attitude, i.e., the taken-for-granted reality of the commonsense world. The radical character of this difference may be indicated by the fact, paradoxical as it may seem, that the "same" question cannot be addressed to problems within these domains. This is so simply because the terms of any question (and, hence, the terms of any possible answer) undergo a thorough transformation of meaning as they pass from one domain to another. For example, under the auspices of the natural attitude a question may be asked about the manifest or latent function of some transsituational phenomena (phenomena whose existence is taken to reside outside any particular situation, i.e., objective phenomena). Under the auspices of the ethnomethodological attitude and its attendant reduction, however, a question about transsituational phenomena automatically becomes a question about the phenomenon of transsituationality (i.e., how *members* produce and sustain the sense of objective phenomena taken to exist outside the occasion where that sense is made collectively available).

Clearly the actual practitioners of the ethnomethodological program are explicitly aware of the radical character of their domain assumptions and that the order of phenomena which concerns them is entirely incommensurable with what it is usually taken to be. Thus, Zimmerman and Pollner state that "The reduction does not generate research that may be regarded as an extension, refinement, or correction of extant sociological inquiry. . . . The [ethnomethodological] reduction constitutes as its phenomenon an order of affairs that has no

¹⁰ While misleading in certain respects, the following formulations may be useful in grasping the character of the transformation of perspective involved in shifting from one attitude to another.

- (a) The natural attitude views consciousness as an "object" in the world, while the phenomenological attitude views the "world" as an object of consciousness.
- (b) The natural attitude views the features of particular social situations as products of the encompassing society, while the ethnomethodological attitude views the features of the encompassing society as products of particular social situations.

identifiable counterpart in contemporary social science" (1970:99).¹⁷

CONCLUDING REMARKS

It should be apparent that phenomenology, properly understood, can contribute to the sociological enterprises, properly understood. For this contribution to come about, and to save ourselves from the radical problems of what is called phenomenological sociology, we must make a systematic and disciplined inquiry into Husserlian phenomenologies and their derivatives (transcendental, psychological, hermeneutical and existential phenomenologies). This alone, however, is not enough; for any contribution phenomenology can make presupposes and depends on a clear understanding of what sociology is and can become. Since there are many sociologies, the task is complex. The resolution, however, is an individual problem (cf. Gouldner, 1970) not only compatible with, but complementary to, phenomenological inquiry (cf. Zaner, 1971).

REFERENCES

- Berger, Peter
1966 "On existential phenomenology and sociology (II)" *American Sociological Review* 31 (April):259-60.
- Berger, Peter and Thomas Luckman
1966 *The Social Construction of Reality*. Garden City: Doubleday and Company, Inc.
- Bruyn, Severyn
1966 *The Human Perspective in Sociology*. New Jersey: Prentice Hall, Inc.
- Cicourel, Aaron
1968 *The Social Organization of Juvenile Justice*. New York: John Wiley & Sons, Inc.
- Darville, Richard
1972 "Sociologist and person." Unpublished paper presented at the Annual Meetings of the Canadian Sociology and Anthropology Association, Montreal, Quebec.
- Denzin, Norman
1970 "Symbolic interactionism and ethnomethodology." Pp. 259-84 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Douglas, Jack
1970 "Understanding everyday life." Pp. 3-44 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Dufrenne, Mikel
1966 *The Notion of the A Priori*. Evanston: Northwestern University Press.
- Durkheim, Emile
1938 *The Rules of Sociological Method*. New York: The Free Press.
- Garfinkel, Harold
1967 *Studies in Ethnomethodology*. New Jersey: Prentice Hall, Inc.
- Goldstein, Leon
1963 "The phenomenological and the naturalistic approaches to the social." Pp. 286-301 in M. Natanson (ed.), *Philosophy of the Social Sciences*. New York: Random House.
- Gouldner, Alvin
1970 *The Coming Crisis of Western Sociology*. New York: Basic Books.
- Gurwitsch, Aron
1966 *Studies in Phenomenology and Psychology*. Evanston: Northwestern University Press.
- Heeren, John
1970 "Alfred Schutz and the sociology of commonsense knowledge." Pp. 45-56 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Holzner, Burkart
1968 *Reality Construction in Society*. Cambridge: Schenkman Publishing Company, Inc.
- Husserl, Edmund
1962 *Ideas: General Introduction to Pure Phenomenology*. New York: Collier Books.
- 1970a *The Crisis of European Sciences and Transcendental Phenomenology*. Evanston: Northwestern University Press.
- 1970b *Cartesian Meditations*. The Hague: Martinus Nijhoff.
- Jacobs, Jerry
1967 "A phenomenological study of suicide notes." *Social Problems* 15 (Summer): 60-72.
- Kockelmans, Joseph
1967 *A First Introduction to Husserl's Phenomenology*. Pittsburgh: Duquesne University Press.
- Koestenbaum, Peter
1966 "Phenomenological foundations for the behavioral sciences: the nature of facts." *Journal of Existentialism* VI (Spring): 305-41.

¹⁷ Note that Douglas (1970) is either unaware of or ignores the radical character of this transformation of meaning between the natural and ethnomethodological attitudes. Throughout his introduction he continually treats the goals of studying everyday life as establishing "objective transsituational knowledge." Yet this is precisely the sort of "knowledge" that is bracketed under the ethnomethodological attitude! In this, of course, Douglas is far from being alone, but it is curious to see such a viewpoint expressed by one who, among other things, purports to give a knowledgeable account of ethnomethodology.

- Lauer, Quentin
1965 *Phenomenology: Its Genesis and Prospect*. New York: Harper & Row, Publishers.
- Levinas, Emmanuel
1967 "Intuition of essences." Pp. 83-105 in J. Kockelmans (ed.), *Phenomenology*. Garden City: Doubleday & Company, Inc.
- Martindale, Don
1960 *The Nature and Types of Sociological Theory*. Boston: Houghton Mifflin Co.
- Merleau-Ponty, Maurice
1962 *Phenomenology of Perception*. London: Routledge and Kegan Paul.
1964a *The Primacy of Perception*. Evanston: Northwestern University Press.
1964b *Signs*. Evanston: Northwestern University Press.
1969 *The Visible and the Invisible*. Evanston: Northwestern University Press.
- Natanson, Maurice
1962 *Literature, Philosophy and the Social Sciences*. The Hague: Martinus Nijhoff.
1970 "Phenomenology and typification: a study in the philosophy of Alfred Schutz." *Social Research* 37 (Spring):1-22.
- Neisser, Hans
1959 "The phenomenological approach in social science." *Philosophy and Phenomenological Research* XX (December):198-212.
- O'Neill, John
1970 *Perception, Expression and History: The Social Phenomenology of Maurice Merleau-Ponty*. Evanston: Northwestern University Press.
1972 *Sociology as a Skin Trade: Essays Towards a Reflexive Sociology*. New York: Harper & Row, Publishers.
- Plivcevic, Edo
1972 "Can there be a phenomenological sociology?" *Sociology* 6 (September):335-49.
- Pollner, Melvin
1970 *On the Foundations of Mundane Reasoning*. Unpublished doctoral dissertation. Department of Sociology, University of California at Santa Barbara.
- Psathas, George
1971 "Phenomenological sociology." Unpublished paper presented at the Annual Meetings of the American Sociological Association, Denver, Colorado.
- Ricoeur, Paul
1967a *Husserl: An Analysis of His Phenomenology*. Evanston: Northwestern University Press.
1967b "New developments in phenomenology in France: the phenomenology of language." *Social Research* 34 (Spring):1-30.
- Sacks, Harvey
1966 *The Search For Help: No One to Turn to*. Unpublished doctoral dissertation. Department of Sociology, University of California at Berkeley.
- Scheler, Max
1954 *The Nature of Sympathy*. New Haven: Yale University Press.
- Schur, Edwin
1971 *Labeling Deviant Behavior*. New York: Harper & Row, Publishers.
- Schutz, Alfred
1962 *Collected Papers I: The Problem of Social Reality*. The Hague: Martinus Nijhoff.
1964 *Collected Papers II: Studies in Social Theory*. The Hague: Martinus Nijhoff.
1966 *Collected Papers III: Studies in Phenomenological Philosophy*. The Hague: Martinus Nijhoff.
1967 *The Phenomenology of the Social World*. Evanston: Northwestern University Press.
1970 *Reflections on the Problem of Relevance*. New Haven: Yale University Press.
- Shearing, Clifford and M. G. Petrunik
1972 "Normative and phenomenological approaches to the study of deviance." Unpublished paper presented at the Annual Meetings of the American Sociological Association, New Orleans, Louisiana.
- Smith, Dorothy
1972a "The ideological practice of sociology." Unpublished paper presented in April at the Department of Sociology, Queens University, Kingston, Ontario.
1972b "The intersubjective structuring of time." Unpublished paper presented at the Annual Meetings of the Canadian Sociology and Anthropology Association, Montreal, Quebec.
- Spiegelberg, Herbert
1971 *The Phenomenological Movement: Volumes I & II*. The Hague: Martinus Nijhoff.
- TeHennepe, Eugene
1965 "The life-world and the world of ordinary language." Pp. 133-46 in James Edie (ed.), *An Invitation to Phenomenology*. Chicago: Quadrangle Books.
- Thomas, W. I.
1951 *Social Behavior and Personality*. New York: Social Science Research Council.
- Tiryakian, Edward
1965 "Existential phenomenology and sociology." *American Sociological Review* 30 (October):674-88.
1966 "Reply to Kolaja and Berger." *American Sociological Review* 31 (April):260-4.
- Weber, Max
1949 *The Methodology of the Social Sciences*. New York: The Free Press.
1968 *Economy and Society*. New York: Bedminster Press.
- Wieder, D. Lawrence
1970 "On meaning by rule." Pp. 107-35 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Wilson, Thomas
1970 "Normative and interpretive paradigms in sociology." Pp. 57-79 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Zaner, Richard
1961 "Theory of intersubjectivity." Alfred

- Schutz." *Social Research* 28 (Spring): 71-93.
- 1970 *The Way of Phenomenology*. New York: Pegasus.
- 1971 "Solitude and sociality: the critical foundations of the social sciences." Unpublished paper presented at the Annual Meetings of the American Sociological Association, Denver, Colorado.
- Zimmerman, Donald and Melvin Pollner
1970 "The everyday world as a phenomenon." Pp. 80-103 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.
- Zimmerman, Donald and D. Lawrence Wieder
1970 "Ethnomethodology and the problem of order: comment on Denzin." Pp. 287-98 in J. Douglas (ed.), *Understanding Everyday Life*. Chicago: Aldine Publishing Company.

SOCIAL PROBLEMS, PROBLEMATIC SITUATIONS, AND QUASI-THEORIES *

JOHN P. HEWITT

University of Massachusetts, Amherst

PETER M. HALL

University of Missouri, Columbia

American Sociological Review 1973, Vol. 38 (June):367-374

This paper explores the structure, circumstances, and consequences of the use of quasi-theories in talk about socially problematic situations. Impelled by a situation within their view which they perceive as disorderly, people attempt to construct its reality by talking about it. In the course of talk, participants tentatively indicate that they are about to pursue a given line of analysis. Such moves take the form of stylized verbal expressions. If the others respond by accepting the indication as a fruitful line to pursue, agreement on a cure for the problematic situation will be forthcoming. They will then draw an inference from the cure to the basic nature of the problem; a specification of the core problem will be constructed. Participants will then build around this specification and its cure a more elaborate causal analysis containing the following elements (a) a distinction between the core problem and essentially illusory aspects of it; (b) causal generalizations that support the analysis; (c) illustrations, examples, and biographical reconstructions; and (d) widespread values and beliefs that support the analysis.

REALITY CONSTRUCTION IN PROBLEMATIC SITUATIONS

ANY collectively defined contemporary social problem represents a set of conditions regarded as both undesirable and avoidable. But the grounds on which conditions are so defined and the process of definition itself are not well understood. This essay seeks to advance that understanding by focusing on the social construction of the reality of social problems.¹ We will examine

the uses of quasi-theories in talk about problematic situations. Quasi-theories—defined as ad hoc explanations brought to problematic

builds generally on the image of man as a "reality constructionist" drawing on a social stock of knowledge (Berger and Luckmann, 1966). The emphasis on social order as an accomplishment of everyday conduct derives primarily from Garfinkel (1967) and McHugh (1968). Our view of social problems closely parallels that of Herbert Blumer (1971: 298) who views problems as "fundamentally products of a process of collective definition instead of existing independently as a set of objective social arrangements with an intrinsic makeup." This view of problems is also grounded in Hall's (1973) symbolic interactionist analysis of politics. The felt need to explain problematic situations and thus to restore order is related to the process in which accounts are rendered and received in the event of normative failures (Scott and Lyman, 1968). This conception also draws from Mills' (1940) conception of vocabularies of motives. The general approach to social problems we develop here can be viewed as an effort to apply aspects of the labeling perspective more generally to the analysis of social problems (cf. Becker, 1963; Schur, 1971).

* Revised version of a paper presented at the annual meeting of The Society for the Study of Social Problems, New Orleans, La., August 25-28, 1972. We are indebted to our colleagues Jay Demerath, Anthony Harris, Lewis Killian, Michael Lewis, Thelma McCormack, Randall Stokes, and Andrew Twaddle for their helpful comments and criticism.

¹ The concept of the quasi-theory comes from previous work by the authors which explored the political uses of a quasi-theory of communication (Hall and Hewitt, 1970). Our work is linked to several strands of research and theory. The analysis

situations to give them order and hope—have both structure and consequences. The explanations and solutions offered by quasi-theories permit situations to be perceived as meaningful in the light of common-sense notions of human behavior and social arrangements.

The concept of the problematic situation can be defined on the basis of Schur's treatment of deviant behavior:

Human behavior is deviant to the *extent* that it comes to be viewed as involving a *personally discreditable* departure from a group's normative expectations, and it *elicits* interpersonal or collective reactions that serve to "isolate," "treat," "correct," or "punish" *individuals* engaged in such behavior (Schur, 1971:24).

There are three main elements in this definition. First, it focuses on concrete behavior as socially defined, and not on alleged characteristics of the behaving person or on some objective feature of the behavior itself. Second, it emphasizes the normative standard against which the judgment of deviance is made. Third, it calls attention to collective responses to the behavior of individual deviants. These three elements are central to any general definition of problematic situations.

In their responses to the social world, people do not act in relation to discrete and objectively meaningful behavior and events. Rather, they act toward situations which contain behavior and events which are given meaning. Thus we must focus on socially problematic situations. If we fail to do so, and choose instead to deal more generally with "social problems," or with their discrete manifestations, we neglect the process of reality construction and move too quickly to its product.

The standard of judgment used in defining situations as socially problematic is a more difficult issue, and we cannot give it the full attention it deserves. The question of deviance itself is not easily resolved since the concept of normative expectations leaves much to be desired.² We suggest as an al-

ternative standard one which involves a cognitive conception of social order as its basis. While the definition of situations as deviant suggests that departures from normative expectations are significant, the standards are, in fact, far more diverse. What is noticed as a scene is surveyed and judgments made is not simply that norms are being violated, but more generally that the orderly quality of the scene is questionable. Labels of deviance that attempt to discredit and isolate individuals for their acts are responses to situations that are first perceived as extraordinary. The label of deviance is, in effect, an attempt to explain social disorder and possibly to formulate a plan of action against it.

The standards by which people judge situations orderly or disorderly ordinarily lie in the background but come to consciousness when they are violated.³ In addition to normative considerations, people's sense of the orderliness of others' acts and of the contexts in which they act is informed by judgments of the typicality of their acts, of the likelihood of observed acts and events, of causal relationships in the situation, of the technical efficiency of their acts in relation to their perceived goals, and of the substantive congruency between actors' and observers' perception of the scene. Whether as participants or onlookers, people are disposed to view a situation as orderly if they can find a basis for viewing the behavior in it as typical of its members, probable under the circumstances, applicable in the light of conditions, appropriate to the goals being sought, realistic in its direction, and morally within the norms. They view it as disorderly if the behavior seems atypical, unlikely, inexplicable, technically inappropriate, unrealistic, or morally wrong.

Behavior that departs from any *one* of these standards will be sufficient to arouse concern. If a group of professors, for example, see a colleague acting in a way they perceive as detrimental to his career, order

² A number of authors have illuminated the status of rules or norms in defining deviant behavior (Becker, 1963; Hall, 1973; Cicourel, 1972; Katz, 1972). Of these, Katz is perhaps clearest in arguing that deviance cannot be fully defined by rule-violations or by accusations of violations; instead, the

"sociological existence of deviant phenomena is constituted by the imputation of deviant ontological status to human beings" (1972:192).

³ A variety of lists of such standards could be constructed. Our discussion parallels that of Peter McHugh (1968:43-5), which is in turn an adaptation from Garfinkel (cf. 1967:76-103).

is made problematic by the technical inefficiency of his acts. If a married couple viewed by their friends as well-adjusted and happy are seen quarreling bitterly in public, order is called into question by the atypicality of their acts. In either example, all other standards of order may be met, and still the situation is judged disorderly.

If orderliness is questioned solely because of a normative violation, order may be reintroduced before participants have a chance to ponder the issue. Some normative violations are instantly noted and accounts by the violator expected. If an acceptable excuse or justification (Scott and Lyman, 1968) ensues, order is re-established. Likewise, observers of a situation may impute excuses, justifications, and acceptances to participants. Some violations are dealt with through widely shared and well-understood labels, which make it possible to construct a situation as orderly because it contains deviance and deviants. Thus, for example, a homicide is viewed as orderly because typicality, causal texture, probabilities of certain kinds of behavior, and other standards are assessed specifically with reference to a situation that contains "murder" and "murderers."

Finally, we must consider the nature of the collective response to a problematic situation. In the case of deviance, certain kinds of action are targeted and individuals discredited and handled. The individual is in effect told: you have violated norms and so must be dealt with in appropriate ways. "Appropriate" responses to deviance involve both delegating authority to control agents who deal with the deviant and his avoidance by the pure.

Deviance can be easily dealt with: A situation is perceived as problematic with respect to social order; a label is then invoked which "explains" the perceived disorder; that label discredits individuals and indicates and justifies social reactions; order is thus re-introduced. The labels available for problematic situations and their contents are highly regularized in the area of deviance, and the social mechanisms for applying them are highly formalized. But people are confronted with a range of problematic situations for which deviance labels cannot be applied, because they are unavailable or do not fit.

Where deviance labels are not used and

accounts do not suffice, explanation becomes the major feature of the collective construction of the reality of a problematic situation. Whether the explanation is in hand, anticipated, or to be sought, the nature of the problem is tied intimately to the notion that there is a solution, which in turn implies explanation. The emphasis on explanation suggests that people confront many disorderly situations with implicit optimism—if not the view that all things work out for the best, at least the assumption that things can be understood, that events have a discoverable meaning (cf. Killian, 1971). This assumption is reflected in the liberal ethos of science, progress, intelligence, and rationality (Rossides, 1972).

Let us then examine how people construct the reality of socially problematic situations other than those constructed as deviant or those in which accounts are given and readily accepted. We advance the following definition: *A situation is regarded as problematic to the extent that it comes to be viewed as involving social disorder for which an explanation is available or will be available.*

THE QUASI-THEORY OF COMMUNICATION

It is in the social context of talk that problematic situations become defined. Everyday talk about problems is clearly not conducted according to the rigorous criteria of scientific rationality. Instead, talk about problems structures their nature, and it does so because the stock of knowledge held more or less in common by the members of a society includes a stock of quasi-theories which are available when talk demands their use.

The term quasi-theory has its origins in the authors' (1970) analysis of the tactics used by the Nixon administration to manage the dissent that followed the invasion of Cambodia in the spring of 1970. In that analysis we argued that the administration had relied on a *quasi-theory of communication* to cool out students and others who protested what they perceived as a widening of the war in Indochina. During that period the administration was preoccupied with communication between it and the public. This preoccupation reflected the availability in American culture of a quasi-theory which sees problems (failures, crises, errors, con-

licts, controversies) in organizational and interpersonal affairs as due to communication breakdowns. When communications break down, the flow of information is disrupted and disagreements and misunderstandings occur. To solve such problems people must work to re-establish communication. Faced with growing dissent, the administration made effective use of this quasi-theory in attempting to redefine the political situation. By claiming that it had been basically misunderstood, the administration reasserted its goals and, at the same time, appeared to link them with the goals of protesters. Moreover, faced with an expanding anti-war movement, the administration, by striking a visible posture of listening to dissent, was able to cool out some dissenters. It defined a controversial situation in such a way as to attribute dissent either to a misunderstanding of policy or to disagreements over means and not ends. In so doing, it was leaning on an American cultural tendency to attribute curative effects to enhanced communications.

THE STRUCTURE OF QUASI-THEORIES

Many stylized, nearly ritual expressions occur in situations in which quasi-theories are invoked. "The trouble was due to a misunderstanding." "If only you'd listen to me you'd know I'm saying the same thing you are." "Don't worry about him, it's just a stage he's going through." "She just doesn't understand the rules." At some stage in problematic situations, participants by intention or chance use expressions that signal that a given line of causal analysis is to be followed. The signal, by the use of widely recognized phrases, permits, but does not require, a new turn of events and words. This signal may or may not be picked up by other participants—it may or may not be accepted as a way to define the situation. If it is, each participant has access to experiences, knowledge, explanations, and principles that permit cooperative work toward defining the situation.⁴

⁴ Experiences, knowledge, explanations, and principles may vary among participants in a context of talk. If so, agreement on a possible line of causal analysis may be more difficult to attain. Participants who differ in social class, ethnicity, religion, or other

Once a common expression is regarded as a potential basis for defining the problematic situation, the quasi-theory comes fully into play. What is essential to a quasi-theory is its logic, which is one of cause and effect, though quite disarranged temporally if viewed from a scientific viewpoint. The use of quasi-theories involves the postulation of a cure, followed by an analysis of cause and effect that supports the cure.

The stylized, common expressions that trigger the full use of quasi-theories are, in effect, announcements of an appropriate cure or resolution to a problem. In a dispute between a married couple, for example, the assertion by one that "I didn't know you felt that way!" may be seen as an opening for the use of a stylized expression, which in turn makes possible an agreement (cure) to sit down and talk about differences. Thus we might imagine the following dialogue:

HE: I didn't know you felt that way!

SHE: But I've been saying so all along!

HE: We've got to learn to communicate better.

SHE: How about right now?

In this illustration, the stylized expression and its response constitute an agreement about a cure. The agreement may be temporary or enduring, depending on the success of the cooperative work that ensues.

That work requires constructing an appropriate causal framework. Once a cure has been announced, actors draw on the social stock of knowledge to construct an analysis of cause and effect that supports the cure; to construct the nature of the problematic situation so that the core or basic problem mirrors the intended cure. This construction is accompanied by several processes: First, the inference from cure to core problem is supported by a redefinition of other aspects of the problematic situation as essentially illusory. Second, the causal analysis is extended by formulating causal generalizations—state-

important dimensions may find common ground only with difficulty. It is also important to note that some contexts of talk focus on problematic situations, but contain an implicit agreement not to come to agreement about the reality of the problem. "Bar room" talk often occurs in the context of a definition of the situation that makes continued dispute the chief goal.

ments, broad in scope, that generalize about a class of similar situations or conditions and thus promise to subsume the particular case. Third, illustrations and examples are brought to bear which help establish the validity of the causal or explanatory scheme being used. And fourth, support is sought in more general values, beliefs, and social perspectives.

The common sense distinction between that which is real and that which is merely apparent is essential to quasi-theoretical explanation. All explanation is concerned with simplification, with reducing masses of confusing detail to some orderly form. Quasi-theoretical explanation accomplishes this task by distinguishing between those aspects of problematic situations that are central and basic and those which are peripheral or apparent. Thus, for example, an attempt to deal with the problematics of urban violence in terms of a quasi-theory of protest entails viewing much observed behavior as only seemingly anti-social—violent acts are seen as efforts to protest and be heard, and not as acts really directed against society. A married couple, having agreed that talk is the cure for their problem, and that the lack of adequate communication is their problem, will redefine their substantive disagreements as trivial, exaggerated, really illusory. By creating this distinction between the real and the apparent, quasi-theoretical explanation accomplishes the task of simplification.

The data of human experience must also be ordered, and in this task an arsenal of causal generalizations stands ready. Once problem and cure have been posited and agreed on, it is not difficult for people to construct generalizations that will appear to subsume the situation in question. If problem and cure are, respectively, ineffective and improved communication, and all other aspects of the problem are merely illusory, then it is easy to generalize about the importance of communication in human affairs. "Most problems between people ultimately resolve to problems of communication" might be taken as a paradigm for such generalizations. The generalizing process is assisted by the social stock of aphorisms that members can draw on and use. The fact that each time-honored saying has its opposite number only adds to the ease of generalization.

Quasi-theoretical explanation seeks further

support for its analysis in examples and illustrations. Personal experiences and memory can usually be relied on to produce other instances of the situation under analysis. Indeed, it is likely that a good deal of past experience will be reinterpreted in the light of the new "insight" that has been reached—events and incidents from the past will rapidly be recalled and fall into place as the explanatory power of the quasi-theory is explored. This ability of the quasi-theory to explain past as well as present situations adds to an actor's conviction that they have adequately explained the situation.

Finally, underlying various phases of quasi-theoretical explanation are a variety of values and beliefs that can be drawn on as needed to buttress points and support the explanation under construction. We pointed out in the analysis of the quasi-theory of communication and the management of dissent, for example, that its interpretation of events was reinforced by a prevalent American belief in common values. Frequent reference in the American context to the existence of basic goals for the society and basic standards of Americanism (though these are rarely spelled out in concrete terms) means that members of the political body are prepared to believe that consensus on values is a basic characteristic of the society, so that any conflict *must* be more apparent than real.

THE QUASI-THEORY OF TIME

The utility of our analysis may be more broadly indicated by discussing a quasi-theory of time. The natural passage of time often assumes a central place in people's efforts to make sense of problematic situations. Time is central to many common sense conceptions of socialization: growing up is viewed as a natural process, in which one stage follows another in an order not susceptible to intervention, shortening, or rearrangement. Thus it is common for adults to explain the problematic behavior of their children with the image of "going through a stage." The "terrible twos" and the "traumas of adolescence" illustrate the assumption that time itself creates and dissolves problems.

Time is often thought essential to resolving human conflicts. Some crises in interpersonal relationships are seen as far less significant

than they appear at the time. The passage of time will "heal all wounds." In time, the heat of the moment will dissipate, cooler heads will prevail, and disagreements can be put in their "proper" perspective.

The quasi-theory of time operates in the same manner as other quasi-theories: An initial stylized expression indicates that a line of analysis is about to be opened. The initial symmetry of cure and core problem leads to efforts to validate the analysis. This involves, first, the statement of causal generalizations. In the first illustration above, statements would be made about the inevitability of certain behavioral problems during adolescence; in the second, we would find generalizations about lovers' quarrels. Then, participants would cite examples and experiences that "confirm" the applicability of the constructed analysis. Moreover, support for the quasi-theory of time will be sought in more basic values and beliefs. Some beliefs suggest the value of inaction. Advice to "let things well enough alone" and to "let sleeping dogs lie" suggests this value. Moreover, certain unpleasant experiences may be positively valued. Conflicts, problems children have with their peers, and interpersonal difficulties, by the twists of logic that characterize social life, can easily be imbued with positive connotations: Children learn from their difficulties and unpleasant experiences, it is said; and lovers know each other better if they have fought creatively. Once a given quasi-theory has been adopted, an endless store of ammunition can be mustered in its defense.

OTHER QUASI-THEORIES

Other examples of quasi-theories abound, and each lends itself to our form of analysis. A *quasi-theory of unconscious motives* (grounded in popularized Freud) interprets untoward and strange actions in terms of a pattern of meaning that stems from motives of which the actor is held to be unaware. A *quasi-theory of ultimately discernable meaning* holds that some disorderly situations are explainable, but that temporary obstacles prevent their meaning from being known. These two quasi-theories afford a last-chance opportunity to introduce order into problematic situations, since they lean even more

heavily than other quasi-theories on assumption and distant facts.

These and other quasi-theories we have discussed are relatively benign in their implications—they take the basic line that the problems people observe are less serious and threatening than they appear. Such quasi-theories assume that people participate in a community of values and interests. Given community, problematic situations are interpreted as avoidable and unfortunate breakdowns or apparent lapses in community.

Some quasi-theories do not rest on a benign assumption of community, but on a suspicion that common values are not held by all. Such theories treat social disorder, not as less threatening than it seems, but as the result of genuine efforts to disrupt. Thus, for example, there is a *quasi-theory of outside agitation*, which postulates the cause of troubles, such as protests against racial inequality, to be outside agitation and the cure their removal. Similarly, quite a wide range of situations that are problematic from one point of view or another—"free love," the use of fluoridation in public water supplies, theatres that show "X-rated" films—are interpreted in terms of a *quasi-theory of anti-social conspiracy*, though the presumed goals and identities of the conspirators vary widely.

Any problematic situation can be defined in terms of almost any available quasi-theory. Thus, for example, some may apply the quasi-theory of outside agitation to urban violence associated with racial issues; while others will resort to the quasi-theory of deprivation which sees violence as inevitably flowing from poverty and the only solution ameliorating the conditions of the poor. Still others may advance a quasi-theory of protest, in which violent acts are viewed essentially in terms of their communicative content. Still others may attend to urban violence with the quasi-theory of communication, viewing it as evidence of discontent fostered by misunderstandings and lack of contact between groups.

THE USE OF QUASI-THEORIES IN PROXIMATE AND DISTANT SITUATIONS

The conditions under which quasi-theoretical explanation takes place can be differ-

entiated by a distinction between proximate and distant problematic situations. The distinction turns on how a situation is known to those who talk about it. A problematic situation is *proximate* if a person's knowledge of it is direct and unmediated by mass media or by third parties; and the situation is *distant* if knowledge is mediated. In proximate situations the reality construction tends to call for explicit action on the part of the attending actor; in distant situations, the attending actor engages in reality construction only.

The significance of the distinction is that members of contemporary society are increasingly affected by situations far removed from direct view for which they receive information in sketchy form through the media. The issue, then, is how an individual can make sense of and order his world, when the events are significant and far away and the information is incomplete. The distance of the events makes them no less a perceived threat to order. Quasi-theories provide a basis for reintroducing a sense of order.

Quasi-theories are less likely to be used in proximate situations because actors tend to be known in more detail and because such situations surround rather than confront the individual. The complex presentations of selves, motives, concerns, and qualities of live actors make it more difficult for them to be treated in quasi-theoretical form. Acting within situations, individuals are likely to respond to others in normative ways, with the result that calls for accounts are the usual method of dealing with disorder. (If accounts fail, however, quasi-theoretical explanation is likely to come into play.)

Quasi-theories thus find a major use in putting distant problematic situations to rest by rendering them orderly, and thus less threatening to world-views. The constructed order is both subjective and collective. Subjectively, the quasi-theory puts the distant problematic situation in its proper perspective—i.e., it is really understandable and there is hope—so that the everyday routines of the individuals are not subject to the threat of disruption. Collectively, quasi-theoretical explanations provide the basis for groups and collectivities to take action toward such distant situations should action be required.

Initial public responses to many problematic situations—the urban disorders of the 1960s, for example, or the Vietnam war protests—take the form of quasi-theoretical reasoning. Such reasoning goes on in countless private talk contexts as well as in the larger context of interaction between the public and government mediated by the press. When problematic situations are repetitive, their ad hoc explanations become stabilized and thus readily available for reference when the situation recurs. “Another riot” or “another demonstration” are phrases that signal such a stabilizing of public definition.

Yet, of course, the stabilizing of a public definition of a problem (or of competing definitions held by different publics) does not mean the explanation will succeed over the long run. Factual events challenging to the theory may occur. Although quasi-theories enable people to perceive situations in a highly selective way, selective perception has its limits. While a quasi-theory of communications implies in the realm of politics and national policies that contending parties will see their unity of purpose and recognize the artificiality of their conflicts, real differences of interest and point of view may persist. In the long run, constant emphasis on the value of talk wears thin; people are ultimately convinced that they have communicated with others by deeds.

Therein lies a major paradox of quasi-theories as well as a major sociological use of the concept. Quasi-theories are used to explain disorder, and thus to put matters in orderly perspective; but in the long run, by serving as the basis on which problems are defined, they help set the terms by which people judge progress made in eliminating problems. The major use of the concept thus lies in its identification of expectations about the future course of social problems. Exploring the content of quasi-theories invoked in a given area of public debate and policy about problems ought to help reveal those aspects of problems perceived and emphasized as well as those ignored; the causal generalizations advanced by various publics; the values invoked to support explanations; the biographical and historical memories evoked by the debate; and the expectations

of various groups and collectivities about the future.

A more fully developed theory of quasi-theories would specify the conditions under which particular quasi-theories are likely to be accepted by various publics as the basis for defining problematic situations. Such analysis would consider the conditions under which solutions and explanations are generated and found convincing, as well as the competition and conflict among various publics with respect to definitions of problems. The overall thrust of such analysis would be toward increased empirical study of the collective behavior processes in which social problems are defined.

REFERENCES

- Altheide, David L. and Robert P. Gilmore
1972 "The credibility of protest." *American Sociological Review* 37 (February):99-108.
- Becker, Howard S.
1963 *Outsiders: Studies in the Sociology of Deviance*. New York: Free Press.
- Berger, Peter L. and Thomas Luckmann
1966 *The Social Construction of Reality*. New York: Doubleday.
- Blumer, Herbert
1969 *Symbolic Interactionism: Perspective and Method*. Englewood-Cliffs, N.J.: Prentice-Hall.
1971 "Social problems as collective behavior." *Social Problems* 18 (Winter):298-306.
- Cicourel, Aaron V.
1970 "Basic and normative rules in the negotiation of status and role." Pp. 4-45 in Hans Peter Dreyzel (ed.), *Recent Sociology* No. 2. New York: Macmillan.
- Garfinkel, Harold
1967 *Studies in Ethnomethodology*. Englewood-Cliffs, N.J.: Prentice-Hall.
- Hall, Peter M.
Forth- "A symbolic interactionist analysis of politics." *Sociological Inquiry*.
- Hall, Peter M. and John P. Hewitt
1970 "The quasi-theory of communication and the management of dissent." *Social Problems* 18 (Summer):17-27.
- Katz, Jack
1972 "Deviance, charisma, and rule-defined behavior." *Social Problems* 20 (Fall):186-202.
- Killian, Lewis M.
1971 "Optimism and pessimism in sociological analysis." *The American Sociologist* 6 (November):281-6.
- Matza, David
1969 *Becoming Deviant*. Englewood-Cliffs, N.J.: Prentice-Hall.
- McHugh, Peter
1968 *Defining the Situation: The Organization of Meaning in Social Interaction*. New York: Bobbs-Merrill.
- Mills, C. Wright
1940 "Situating actions and vocabularies of motive." *American Sociological Review* 5 (October): 904-13.
- Rossides, Daniel W.
Forth- "The legacy of Max Weber: a nonmetaphysical politics." *Sociological Inquiry*.
- Schur, Edwin M.
1971 *Labeling Deviant Behavior: Its Sociological Implications*. New York: Harper and Row.
- Scott, Marvin B. and Stanford M. Lyman
1968 "Accounts." *American Sociological Review* 33 (February):46-62.
- Turner, Ralph H.
1969 "The public perception of protest." *American Sociological Review* 34 (December): 815-31.

MINISTERIAL ROLES AND SOCIAL ACTIONIST STANCE: PROTESTANT CLERGY AND PROTEST IN THE SIXTIES *

HART M. NELSEN, RAYTHA L. YOKLEY AND THOMAS W. MADRON

Western Kentucky University

American Sociological Review 1973, Vol. 38 (June):375-386

The results of a factor analysis of the responses by a sample of Protestant clergymen (serving churches located in five major U. S. cities) to twenty-three items on clergy role are reported. The community problem solving role is shown to include an orientation toward social action. Of the five roles present, two—traditional and community problem solving—are especially related to protest orientation. Clergy roles, theological and political viewpoints, education, and age are used as predictors of protest in Multiple Classification Analysis. From most to least important as predictors were: political viewpoint, community problem solving role, theological position, age, and education. The role of clergyman as community problem solver is briefly discussed.

A DECADE and a half have passed since seminal work was done on ministers' roles and involvement in civil rights activities (see Blizzard, 1958a; Campbell and Pettigrew, 1959). Yet, those somewhat parallel studies have not been directly linked through the collection and analysis of hard data on roles and stance toward social action. In this article we report the findings of an analysis of data collected by mailed questionnaire from a random sample of Protestant clergymen serving churches located in Boston, Pittsburgh, Minneapolis, Atlanta, and Los Angeles. The data included ministers' behaviors and attitudes of a protest nature (generally involving civil rights).

The Blizzard and Campbell-Pettigrew Clergy Studies

Blizzard developed a theoretical framework for viewing the work and stance of the Protestant clergyman. A brief review of his work is necessary in order to understand a parenthetical critique of his work made by Campbell and Pettigrew (1959b:515).

Blizzard conceived of Protestant clergy roles as involving at least three levels for analysis: means (or practitioner roles), goals

(or integrative roles), and conception of the ministry as distinctive from other occupations in the eyes of the individual clergyman (or master roles). Six practitioner roles were described by Blizzard (1956a)—administrator, organizer, pastor, preacher, priest, and teacher. Of interest to us is his description (p. 508) of the organizer role which "involves leadership, participation, and planning in local church associations and community organizations."¹ Blizzard (1958a) observed that the "practitioner role" is "performed . . . as a means to an end (or goal)"; and he gave (p. 375) as an example:

Ministers may preach for different purposes. The intent of one minister in preaching (a means oriented practitioner role) may be to be persuasive to the non-believer, or to evangelize (a goal oriented integrative role). Another may preach to instruct the believer, or to edify or to educate; another may preach to bring judgment to the community, or to be prophetic.

The fourteen integrative roles concerned "the end toward which he [the minister] is working in his professional relationship with parishioners, church associations, community

* This study was completed under the support of a grant from NIMH of the National Institute of Health (No. 1 RO1 MH 16573). The authors also thank Samuel Blizzard and Ernest Q. Campbell who served as consultants at critical points in the larger study.

¹ Blizzard (1956a:509) reported how his respondents ranked the six roles in terms of importance, effectiveness, and enjoyment. He concluded that the "minister's dilemma" lies in the fact that he is urged to allocate more of his energies in administering programs than would be seemingly demanded in terms of training and his own and his parishioners' feelings (which place more stress on the traditional duties of preacher and pastor).

groups, and the general public" (Blizzard, 1958a:374).² Of special interest is his "community problem solver" integrative role in which "the minister conceives his interest and skill as an *organizer* extending out to community, national, and international issues" (p. 378, italics added). The clergyman with this role will liken himself to an Old Testament prophet or will be a "crusader with a social welfare orientation."

The clergyman's "master role" (Blizzard, 1958b) is the way the minister sees himself vis à vis the social system, the culture, and other occupations. For example, the minister may define himself as a mediator between God and man or, instead, as a servant of Christ. Or, he may have a more functional viewpoint, with a social service orientation or he may view himself as an inspiring example for parishioners. The minister might hold two or more of these views simultaneously.³

Campbell and Pettigrew's focus on the clergy role was more specific: the clergyman who faced the "moral dilemma" of racial crisis. They (1959a:85-108, 1959b) drew on three reference systems to reconcile clergy attitude and behavior in the Little Rock, Arkansas racial crisis of 1957. The minister's stand on the crisis was affected by the pro-

fessional, membership, and self-reference systems. The self-reference system included the possibility of guilt from not acting in a way consistent with one's own racial attitude. According to the authors (1959b:515), the role structure works against a social actionist orientation: "the minister is required to be a cohesive force, . . . to show a progressive increase in the membership of his church . . . and to encourage maximum annual giving and to plan for the improvement and expansion of the plant." The minister is compelled "to base his self-image . . . on his success in managing his church."

Campbell and Pettigrew (1959b:515) criticized the Blizzard article (1956a) in which the six practitioner roles were presented on the grounds that "Blizzard [did] not find a 'community reformer' or 'social critic' role in the ministry. . . ." It is unfortunate that they were publishing at about the same time as Blizzard, because they were explicitly interested in the normative system encouraging or discouraging a social actionist stance on the part of the Little Rock clergyman. The pertinent article by Blizzard on this subject was that on integrative (1958a) rather than practitioner (1956a) roles. As already noted, the community problem solving role as seen by Blizzard included a social actionist orientation and particularly "organizer" means. That their interests overlapped is evidenced from one of Campbell and Pettigrew's concluding hypotheses (1959a:124): "The minister whose orientation is primarily to his parish is less likely to support desegregation than the minister who is oriented to the community at large" (italics deleted). They basically identified (1959b:515; 1958a, b) the work of the minister as social change agent as residing in an educating role geared to gradual change.

In summary, Campbell and Pettigrew portray a powerful image of the constraints acting on ministers in their attempts to help implement the faith in the daily lives of adherents. Their important study documents the workings of the reference system in the social action conduct of ministers. Yet the question lingers: Why are there so many avowed social actionists?

A partial answer, we felt, would be found in Blizzard's work. His framework included the community problem solving role, and he

² These roles included general practitioner (three or more integrative roles held with approximately the same intensity), believer-saint, scholar, evangelist, liturgist, father-shepherd, interpersonal relations specialist, parish promoter, community problem solver, educator, sub-cultural specialist, "lay" minister, representative of the church-at-large, and church politician.

³ Two other articles by Blizzard are of less importance to our study. Blizzard (1956b) discussed role conflicts facing some urban clergymen, including potential conflict between believer-saint and prophet, or that "the minister feels that he is expected to be a man of belief, but that ethical judgments that he derives from his understanding of the Christian faith are at times challenged." In other words, an individual clergyman might find himself feeling tension over the attraction of sometimes contradictory integrative roles, here being the believer-saint and the community problem solving roles. Finally, Blizzard (1959) discussed similarity as well as uniqueness in demands made on the minister across community settings. For example, "metropolitan-urban located ministers stressed race, ethnic, and civil rights issues [while] non-metropolitan clergy emphasized taking stands on puritan ideals of conduct."

explicitly indicated that the minister with this orientation would assume either a prophetic or social welfare stance.

Our strategy was to develop an instrument to measure clergy role orientation by using factor analysis to identify roles of an integrative nature. An examination of these roles might indicate a minister's social actionist orientation. The clergy roles could also be related to attitudes and behaviors of a protest nature (especially including civil rights). We would expect the community problem solving (or social actionist) role to be positively related to receptivity to protest. Finally, other variables can be included in the analysis (for reasons to be explained later): political viewpoint, theological position, age, and education of the minister. Our attention, then, is focused on predicting protest behavior and attitude of Protestant clergymen.

The Data and the Measure of Receptivity Toward Protest

From the church section of the Yellow Pages of the telephone books for the five cities already named, one thousand churches were selected (two hundred per city). Churches were numbered, the interval distances calculated, and random starts selected. Of the thousand churches selected, forty were ineligible due to a lack of sufficient address or (rarely) for other reasons (for example, an error in numbering which caused a church executive rather than a church to be selected). Of the 960 Protestant clergymen sent questionnaires (in the name and address of the local church), 443 returned completed questionnaires (46.1 percent completion rate). Included with the questionnaire was a postcard to be returned at the same time, signifying identification of those who had cooperated, but not permitting the respondent to be linked with the specific questionnaire returned.

The cities were selected to give a wide range of denominations. An analysis of completion rate by city indicated that from most to least cooperative were ministers of: Minneapolis, Boston, Pittsburgh, Atlanta, and Los Angeles. The percentages ranged from 60.0 to 32.1 for completion rate by city. An analysis of completion rate by denomination indicated that the lowest percentage rate

was 40.1 (Baptist), while the highest was 64.0 (Lutheran).

Finally, it might be noted that using the Yellow Pages for sampling would give results more representative than one might think. Glock and Stark (1966:220) indicated that most of even the smallest sectarian congregations appeared in the church section of the Yellow Pages in their four-county California sample. The questionnaires in our study were mailed in November, 1971; and one followup letter was employed.

The major dependent variable in this study was receptivity toward protest, and four items were used to construct an index to measure protest. Two items were behavioral and two attitudinal:

- (1) Have you ever practiced civil disobedience (risked arrest to symbolize protest) as part of a civil rights, peace, or other movement?
- (2) How would you characterize your participation in social action events (political activities, demonstrations, civil rights protest, etc.) over the past six or seven years or so?
- (3) Do you approve or disapprove of clergymen participating in civil rights protests?
- (4) Looking back on the decade of the 1960's, do you wish that the church had been: more involved in the Civil Rights Movement, about as involved . . . as it was, or less involved. . . .⁴

⁴ Without collapsed categories, the inter-item correlations for the responses to these items ranged from +.35 through +.63; and the item-total coefficients ranged from +.57 through +.90. The value for coefficient alpha was .73. (In addition, a factor analysis of the data gave only one factor, with loadings in excess of +.65.) The index was therefore assessed as having internal reliability.

The collapsing of responses in summing the score was: Yes (1 point, for 14.3 percent of the respondents who were assigned scores) versus No (0 points); Very or somewhat active (40.4 percent of the respondents) versus not very active; Strongly approve or approve (63.4 percent) versus undecided, disapprove or strongly disapprove; and More involved (with 41.2 percent of the clergymen so responding) versus about as involved as it was and less involved than it was. Thirteen respondents replied that they had too recently entered the ministry to answer the second question. These thirteen and respondents not replying to one or more questions were not assigned scores. Of the 443 respondents re-

Factor Analysis of Clergy Role Items

Fifty-six items were included in the questionnaire to tap the ministers' role orientations. Most of the items were written by the first author who, as he prepared them, kept the Blizzard article (1958a) on integrative roles firmly in mind. This article included several short quotations of ministers' responses selected by Blizzard as typical of the integrative roles. The items were then reviewed by the three authors together with a consultant (Blizzard) and modified. The fourteen roles were not represented by the same number of items.

The responses to the fifty-six items were subjected to a principal component solution factor analysis with orthogonal rotation. A lower limit of 1.00 was used for the appropriate eigen-values in order to terminate the extraction of factors. Items with factor loadings less than .40 were then discarded. In addition, the first factor included seventeen items loading .40 and above. An examination of the first factor indicated that it consisted of items traditionally associated with the ministry, and therefore only six items were selected (two each for Blizzard's believer-saint, evangelist, and father-shepherd) for inclusion in the reanalysis. The responses to twenty-three items were then again subjected to factor analysis, and the results appear summarized in Table 1.

The first six items comprise the traditional role. The sixth item best fits under the first factor—the traditional role—but it also loads under the community problem solver role. (A factor analysis of a second set of data collected in June, 1972, and using the twenty-three items on clergy roles gave results very similar to those shown in Table 1, except that this sixth item clearly loaded under both Factors I and IV. The second set of data were

collected from a nationwide sample of United Presbyterian ministers.)

Items 7 through 11 have in common a counseling role, or the interpersonal relations specialist role in Blizzard's terminology. The tenth item also loads under Factor IV—the community problem solving role, which would include series of conferences with people with crucial problems (probably of a social rather than personal nature).

Items 12 through 17 comprise the third factor which is a church administration orientation, or in Blizzard's phrase, the "parish promoter." (The sixteenth item shifted to the fifth factor, or the educating role, for the Presbyterian data). The seventeenth item cuts across several factors; it is included in the first factor (the traditional role would include encouraging a comforted congregation). It was placed under the third factor in part because it was written to represent that orientation. (For the Presbyterian data, it loads heaviest under the third factor). It also loads (-.36) under the fourth factor, with the community problem solver apparently seeing some advantage in conflict in a congregation, since conflict might sometimes be a necessary ingredient for discussing and solving social problems.

Items 18 through 21 are interpreted as representing the community problem solving role described by Blizzard. It can be seen that this role includes social action. An examination of the items reveals that social criticism of the community and society are part of this role.

Finally, items 22 and 23 make up the educator role. Remember that at least one earlier item (16) had a measurable loading (.37) on this factor.⁵

⁵ A brief comment might be made about Blizzard's (1958a) assessment of the distribution of his respondents across the fourteen integrative roles. We have already stated that our traditional role included his father-shepherd, believer-saint, and evangelist integrative roles (these comprise 36.7 percent for his sample). His interpersonal relations specialist role can be equated with our counseling factor (16.7 percent), and his parish promoter integrative roles is an almost direct match for our administrator role (14.3 percent). The community problem solver roles are virtually identical and comprise 16.7 percent of his sample. Finally, the educator role of his sample constitutes 4.0 percent. He notes that the general practitioner role consists

turning questionnaires, 413 were assigned protest scores.

Since the final index score is composed simply of the number of items the respondent marked "protest," the total score (though of a restricted range) is an interval level measure in much the same sense that an achievement test is at the interval level of measurement. Separate analyses of individual protest items as well as subindices (behavioral and attitudinal) gave results very similar to the findings reported in this article.

Table 1. Ministerial Role Items, With Factor Loadings

Ministerial Role Items	Factor Loadings ¹				
	I	II	III	IV	V
1. I try to be a person who preaches the Word to save souls.	.84	.04	.09	-.15	.17
2. The minister should be prayerful in all things.	.82	.06	.02	-.05	.03
3. My ministry is especially concerned with the salvation of souls.	.82	.10	.08	-.18	.15
4. I am the servant of Christ, a shepherd to my church members.	.73	.07	.12	-.05	.06
5. The minister is an example of the fruits of the Christian life.	.70	.18	.22	-.06	.07
6. I try to fight for what is right and help my members overcome evil.	.68	-.02	.01	.30	.04
7. I find that counseling people is the most important function of the Church.	.08	.79	-.04	.10	.16
8. I am willing to devote most of my time to counseling.	.11	.79	-.08	-.05	.21
9. I get closest to the spiritual concerns of my people when I'm counseling with them.	.19	.75	-.00	-.01	.05
10. My ministry tends to be a series of conferences with people with crucial problems.	-.00	.57	.07	.37	-.15
11. I think of myself as a lay psychiatrist.	-.04	.48	.34	.08	.22
12. I am an organization man and I make no bones about it.	.04	-.12	.74	.02	.11
13. I like to run my church as a smooth, well-oiled organization.	.32	.01	.69	-.17	.15
14. I feel rewarded when attendance is high, membership is expanding, and the budget is oversubscribed.	.08	-.01	.68	.03	-.28

Note that for the remainder of the paper we have assigned role scores to respondents in two ways. From the factor analysis we produced factor scores for each factor.⁶ These

of individuals loading equally heavy on three or more roles; and this role constitutes 7.1 percent of his sample. Six roles delineated by Blizzard are unaccounted for by our instrument, but our five roles account for 87.4 percent of his respondents (for his general practitioner role we have allocated 7.1 percent times our subtotal). In conclusion, these twenty-three items permit the categorization of a significant portion of the respondents; and the brevity of this instrument is one of its positive features.

⁶ The factor analysis used a principal component solution with an orthogonal rotation of the factor

scores were then transformed to a set of scores with a mean of 50 and a standard deviation of 10. This transformation was executed solely for processing convenience and in no way altered the shape of the distribution. These scores were used when clergy roles were utilized as dependent variables. The transformed scores were also collapsed with four categories for each role, with the

matrix. A regression estimate of factor scores was computed using the formula: $S = ZR^{-1}F$, where S = the matrix of factor scores; Z = the standardized data matrix; R = the inverse of the correlation matrix; and F = the rotated factor loading matrix. For a discussion of this approach, see Rummel (1970:437-41).

Table 1--Continued

Ministerial Role Items	Factor Loadings ¹				
	I	II	III	IV	V
15. My major tasks are the making of plans, the recruiting of leaders, and the supervising of groups.	.05	.09	.68	.11	.15
16. One of my major tasks is the recruitment of personnel for the church (Sunday) school.	.20	.19	.45	.07	.37
17. I try to keep conflict at a minimum in my congregation.	.50	.09	.42	-.36	.11
18. I feel called by God to speak out on social ills.	.04	.05	-.06	.80	.02
19. I continually seek ways to strike harder at problems like war and racism.	-.16	.08	.10	.76	-.15
20. I am a servant of the community who identifies problems that Christians can work toward solving.	.01	.23	-.02	.68	.13
21. I find that I sometimes think of myself as an Old Testament prophet, a social critic to the community.	-.14	-.08	.03	.63	.19
22. The major goal of the church should be religious education.	.12	.12	.16	.04	.74
23. I most enjoy Christian Education, whether it is with youth or adults.	.25	.25	.05	.10	.65

¹The five factors are identified as involving the following roles:

- I. Traditional
- II. Counseling
- III. Administrator
- IV. Community Problem Solving
- V. Christian Education

N=435. The responses by eight additional individuals were not subjected to analysis because they did not respond to three or more items. Other "no responses" were assigned to the middle (third) response; these were infrequent and were distributed fairly evenly over the 23 items.

quartile values as the cutting points. These scores were used when role orientations were predictor variables.

Clergy Roles Related to Theological and Political Views

In comparing positions taken by liberal, neo-orthodox, and conservative ministers on six public issues, Johnson (1967:441) concluded that while "theology is a good predictor of the political attitudes and behavior of ascetic Protestant pastors and laymen" there are "grounds for rejecting theology as the sole or even the major source of these norms." Johnson observed the heterogeneity in beliefs among modern theologies, and he

concluded that "it seems plausible that the major source of the political views of liberal religious leaders is simply their common commitment to humanistic social values and their common interest in translating these values into policy positions on specific issues."

The ministers in our study were asked to rate themselves on both their theological and political positions.⁷ The two variables were

⁷ Stark and Foster (1970:390-2) report the analysis of the relationship between ministers' theological self-conception (fundamentalist, conservative, neo-orthodox, and liberal) and their index for orthodoxy (including items for belief in a personal God, divinity of Jesus, authenticity of Biblical miracles, and existence of the Devil). They report (p. 391)

positively correlated ($r = .44$), with ministers who rated themselves theologically liberal also tending to rate themselves politically liberal. This relationship supports Johnson's conclusion that theological outlook is a good predictor of political viewpoint.

Using Multiple Classification Analysis (on MCA see Andrews et al., 1967),⁸ with theological and political views as predictor variables and each clergy role as the dependent variable (in five separate analyses), we tested the validity of our two major clergy roles: traditional and community problem solving. Prior to the analysis we predicted that the traditional role especially should have a theological base, whereas the community problem solving role should be more related to political outlook. As expected, ministers scoring high on the traditional role tended to be conservative theologically, and ministers scoring high on the problem solving

role were politically liberal. In comparing eta and beta coefficients, one may observe that the data bear out our expectations: in predicting the traditional role, the beta value for the theological variable remains at a high level, whereas the beta value for political outlook is greatly diminished. In predicting community problem solving scores, the beta value of the political variable remains at a high level, while the beta value for the theological stance is greatly diminished. The results from these five analyses are shown in Table 2. In conclusion, our belief that the traditional role would be especially linked with a conservative theological base is confirmed. Similarly, we have shown that the community problem solving role is linked with a liberal political outlook.

Clergy Roles Related to Protest Orientation

Now we turn to the relationships between clergy roles and receptivity to protest. In Table 3 we show the eta and beta coefficients for the effects of the five clergy roles on protest orientation. The beta values do not especially differ from the eta values for each clergy role (you will recall that the role scores were assigned through factor scaling with rotation; while the data are not shown, separate analyses indicated no significant relationships among clergy roles). From Table 3 it can be observed that two roles especially are associated with protest orientation. The traditionally oriented minister (who tends to be theologically conservative) is likely to score low on protest, while the community problem solving oriented clergyman (who tends to be politically liberal) is likely to evidence high protest orientation. This is as predicted, since without the introduction of other predictor variables, clergy roles (with theological and political bases) were expected to be related to protest orientation.

Next we introduce other predictors in our analysis of the data. The relationship of each clergy role is assessed in separate analyses; each analysis includes other (non-role) predictor variables. The results from these additional twenty analyses are shown in Tables 4 and 5. Table 4 shows the effects of the clergy roles on protest, with additional predictors being theological view (analysis 1), political view (analysis 2), theological and

a high degree of agreement between ministerial self-conception of theological position and scoring on the orthodoxy index, with liberal clergymen being least likely to endorse an orthodox position. Therefore, we have relied on clergy self-assignment for the measurement of theological position.

⁸ Multiple Classification Analysis is an analogue of multiple regression analysis. It permits examining the interrelationships among several predictor variables and a dependent variable, assuming an additive model. Predictors can be at less than ordinal measurement; the dependent variable should be at the interval level of measurement or should be dichotomous. From the output the researcher can see the effects of each predictor on the dependent variable, before and after adjusting for the effects of the other predictors.

The MCA program computes eta (a zero-order correlation ratio) and beta coefficients (the latter is a partial coefficient explaining variation in the dependent variable after the effects of all other predictors are adjusted). The reader can compare the size of the eta and beta coefficients for a given variable and thereby determine the effect of that predictor on the dependent variable, before and after adjusting for the effects of other predictors included in the analysis. From a visual inspection of the means across categories of a predictor, the sign of the coefficient can be determined. Finally, the program produces a multiple (R) correlation coefficient, as well as sums of squares for computing significance levels.

The reader desiring more information on MCA as well as seeing it used with other data should see, in addition to the Andrews et al. (1967) volume, Blau and Duncan (1967:128-40), Bachman (1970: 62-71), Olsen (1970:682-97), Nelsen (1972:230-1), and Yancey et al. (1972:338-59).

Table 2. Effects of Theological and Political Views on Clergy Roles¹

Dependent Variable: Clergy Role	Predictor Variables				R
	<u>Theological View</u>		<u>Political View</u>		
	eta	beta	eta	beta	
Traditional	-.59 ^c	-.48 ^c	-.49 ^c	-.18 ^c	.600
Counseling	.06 ^a	.06 ^a	.10 ^a	.11 ^a	.057
Administrator	-.12 ^a	-.08 ^a	-.16 ^b	-.16 ^b	.124
Problem Solving	+.33 ^c	+.15 ^c	+.44 ^c	+.41 ^c	.444
Christian Education	.19 ^c	-.23 ^c	.18 ^c	.18 ^c	.226

Using appropriate F-test: ^ap>.05 ^bp<.05 ^cp<.01

¹Where possible, directions have been assigned to the coefficients from an examination of means across categories. The table shows the results of five separate MCA analyses. Self-reporting of theological position included four positions: fundamentalist, conservative, neo-orthodox, and liberal. Political categories were: very conservative, somewhat conservative, middle-of-the-road, somewhat liberal, and very liberal.

N=409

political views (analysis 3), and theological and political views together with education and age (analysis 4). This strategy was repeated separately for each clergy role as a predictor of protest (for a total of twenty analyses). While the relationships of education and age with the clergy roles will be reported elsewhere, note that age was included since younger ministers are more pro-

test-oriented (see Campbell and Pettigrew, 1959:123) and since ministers with low levels of education (generally serving sectarian bodies) are less protest-oriented.

Our attention is especially focused on the effects of the traditional and community problem solving roles. With the introduction of theological view as a predictor along with the traditional role, the effect of the tradi-

Table 3. Effects of Clergy Roles on Protest Orientation

Predictor Variables (Clergy Roles)	Unadjusted Protest Means by Role Scale ¹				Coefficients	
	Low	Mod. Low	Mod. High	High	eta	beta
Traditional	2.36	1.59	1.42	.80	.41 ^c	.40 ^c
Counseling	1.40	1.76	1.74	1.42	.13 ^a	.12 ^b
Administrator	2.12	1.32	1.47	1.44	.23 ^c	.15 ^c
Problem Solving	.74	1.34	1.62	2.66	.52 ^c	.51 ^c
Christian Education ²	1.64	1.54	1.65	1.47	.06 ^a	.03 ^a

Using appropriate F-test: ^ap>.05 ^bp<.05 ^cp<.01

¹Role scores resulted from factor scaling (with rotation). Here categories of low, moderately low, moderately high, and high have been assigned using quartile values as cutting points. Shown are means on the protest index; the mean values are not adjusted for the effects of other predictors (the remaining four clergy roles).

²There is possible interaction with race here. The means for whites only are 1.48, 1.40, 1.63, and 1.44. The means for blacks are 2.89, 3.00, 2.71, and 1.47.

N=406 R=.675

Table 4. Effects of Clergy Roles (and Additional Predictors) on Protest Orientation (Beta Coefficients)¹

Clergy Roles	Effect of Role (Introduced Singularly) on Protest, With Following Additional Predictors:			
	Theological View	Political View	Theol. & Pol. Views	Theol. & Pol. Views, Education, and Age ²
Traditional	-.14 ^b	-.15 ^c	-.08 ^a	-.05 ^a
Counseling	.11 ^a	.09 ^a	.08 ^a	.08 ^a
Administrator	-.15 ^c	-.12 ^c	-.11 ^b	-.09 ^a
Problem Solving	+.42 ^c	+.30 ^c	+.31 ^c	+.31 ^c
Christian Education	.07 ^a	.04 ^a	.06 ^a	.04 ^a
Using appropriate F-test:	^a p>.05	^b p<.05	^c p<.01	

¹Where possible, directions have been assigned to the coefficients from an examination of means across categories. Values for eta coefficient can be seen in Table 3. Values for eta and beta coefficients for other predictors utilized in these analyses can be seen in Table 5.

²Values for R, in order (by roles), are: .727, .730, .731, .778, and .726.

tional role drops from -.41 (eta coefficient, which can be seen in Table 3) to -.14 (beta coefficient, which can be observed in Table 4). A similar drop occurs for the traditional role with the introduction of political position. With the introduction of both theological and political viewpoints, the ability of the traditional role to predict protest orientation falls to a level perhaps due to chance alone ($P > .05$).

With the introduction of the theological variable along with the community problem solving variable, the effect of the community problem solving variable on protest is only slightly diminished, with eta = +.52 and beta = +.42. There is a greater decline in coefficient, however, with the introduction of the political variable, with beta = +.30. This level remains for the ability of the community problem solving role to predict protest, when both theological and political viewpoints are used as predictors (and also with education and age added as additional variables). As Table 4 shows, the only clergy role which is significantly related to protest, with theological and political viewpoints, education, and age used as additional predictors, is the community problem solving role.

Table 5 is really a continuance of Table 4 in that it indicates the effects of theological and political viewpoints as well as education and age on protest orientation. The beta co-

efficients which are reported for these variables are modal values since there were five separate analyses employing this set of predictors. Only the values for political outlook evidenced variability depending on the predictors used in the analysis. The modal value for the effect of political view (with the introduction of the other predictors) was +.53; but in the analysis employing the community problem solving role the value was +.42. In other words, both the community problem solving role and the political position were important variables in predicting protest orientation.

Conclusions

An analysis of the items comprising the five roles of the Protestant clergy that we identified and a comparison with the description by Blizzard of his fourteen roles led us to conclude that almost nine-tenths of Blizzard's respondents would have been measured by our instrument. Together with theological and political viewpoints, age, and education, the five clergy roles were used as predictors of protest orientation. With theological and political viewpoints, age, and education used together with a single clergy role, only community problem solving among the clergy roles was significantly related to protest. With the effects of other predictors not con-

Table 5. Effects of Additional Predictors (See Table 4) on Protest¹

Additional Predictor ²	Eta Coefficient for All Analyses	Beta Coefficient (Modal Value) for All Role Analyses ³
Education ⁴	.34	.13
Age	-.24	-.17
Theological View	+.56	+.19
Political View	+.69	+.53

¹These coefficients result from the five analyses (utilizing each role in singular fashion) using role, theological and political views, education, and age. The effects of role on protest, as determined by these analyses, are shown in the extreme right column of Table 4. All coefficients shown in Table 5 are significant at the .01 level, as determined by the appropriate F-test.

²Education was coded as (1) some seminary, but degree not completed, or less; (2) 3 or 4-year seminary degree; (3) graduate work, but no advanced degree; and (4) master's degree or more. Age was collapsed into four categories, with the three breaking points at 40, 50, and 60.

³The values for all beta coefficients did not vary more than .02 from these four modal values, with the exception of the value for political view which was .42 for the analysis with community problem solving utilized for clergy role.

⁴From a visual inspection of means on the protest index across education categories, it was observed that individuals at the two middle levels on education were more protest-oriented than those who were either high or low on education.

trolled, both community problem solving and the traditional roles were related to protest. Traditional ministers tended to score low on protest, while ministers scoring high on community problem solving tended to score high on protest. Clergy endorsing a protest orientation tended to be theologically and politically liberal. From most to least important as predictors were: political viewpoint, community problem solving role, theological position, age (with younger respondents being more protest oriented), and education (the more protest-oriented clergy were at the two middle levels of education).

In summary, Blizzard's community problem solving role was demonstrated to include social action. Campbell and Pettigrew had expressed surprise that some Little Rock ministers would be active integrationists even though their reference system included powerful deterrents for such action. We have located support for such a social action role as stemming, at least in part, from the self-reference system: the clergyman's own clergy role orientation as well as his political viewpoint. The influence of clergy role might also be assigned to the professional reference sys-

tem in that clergymen acquire role orientations in part through socialization in seminary training and through contact with fellow clergymen (for an examination of structural effects of seminaries in socialization, see Carroll, 1971).

At least in reference to one public issue (civil rights), Johnson's finding that both theological and political perspectives would be related to outlook on public issues, but that the former would not be the "major source" of public affairs outlook, was also supported by the findings of our study.

Johnson observed (p. 441) that black denominations tend to be theologically traditionalist, but that the black vote tends to be Democratic; and thus "theology has not alone been responsible for the political norms of these Protestant groups." While only forty-six of our respondents identified themselves as black (and 386 as white), analyses of the data by race indicated that black clergymen scored higher on both the traditional and community problem solving roles and higher on protest as well. Our analysis would indicate that the crucial variables for predicting protest are political outlook and

orientation to the community problem solving role. Johnson's statement and our findings are, then, in accordance.

We suggest that clergy preference for a community problem solving role can be identified at least in part as a commitment to a humanistic Christianity, with a concomitant desire to identify those social and political problems at the community and societal level which can be changed for the better. This orientation exists to some degree apart from political outlook. The acquisition of such an orientation remains to be explored.

Johnson has reiterated (p. 440) the observation that liberalism within the Church has attempted to shift the emphasis, especially within Protestantism, away from the older, more conservative concerns with individual salvation, and toward a greater concern for modifying the present conditions of life. He also suggested that Protestant conservatives tend not to impute religious significance to social issues and, by implication at least, that religious liberals tend to identify certain social issues as religious and moral questions. We might presume, therefore, that political liberalism might be a political result of the social concerns already noted, while the community problem solving role might be the occupational outgrowth of the same concerns. One's theological position might provide a rationale for both political liberalism and the community problem solving role.

The entire question might be placed in a slightly different perspective by suggesting that (religious) conservatives might well tend to identify "religion" itself with theology, while (religious) liberals might tend to identify it with ethics (Hadden [1969:98] and others have taken a similar position, relating the "new theology" with concern for social action in this world rather than for otherworldly religious doctrine). Thus, we might even suggest that for the liberal pastor the significant fact about his vocation is that it is one institutional arrangement (among others) which allows him to work out his ethical objectives. Had the liberal pastor perceived some other institutional arrangement (early in his career) as more effectively fostering those ethical objectives, it seems reasonable that he might have followed other

career possibilities (Jud et al. [1970:51, 72] report that one-fourth of the United Church of Christ ex-pastors in their study left "to work in greener pastures because . . . [of being] disillusioned or angry about the church's irrelevance" and that ex-pastors were more likely than pastors to report having enjoyed "giving leadership to the community on crucial social issues"). For the conservative pastor the one special institutional arrangement that can satisfy his quest for personal salvation (for himself and others) is the Church.

Other analysts have suggested that religious liberals and conservatives also perceive the character of sin (and hence, of salvation) somewhat differently, for liberals tend to identify "sin" with societal problems (war, segregation, etc.); while conservatives tend to identify it with personal problems (lying, swearing, drinking, etc.). H. Richard Niebuhr (1929) has described the character of the religious movement as being more strongly related to its definition of sin than to its concept of salvation. We therefore suggest that the problem solving role orientation and political liberalism stem from a common ethical concern and that the pastor's theological position represents an effort to provide an "explanation" for his ethical concerns within a religious context (we should note the similarity between theological position as explanation and Blizzard's conception of the master role).

Whether any given individual would enter the ministry (rather than social work, for example) to satisfy his ethical concerns would undoubtedly be a function of his earlier socialization experiences. His decision to remain in the ministry could then be construed to be at least a partial function of the extent to which the Church actually provided an arena in which those ethical concerns could come to some fruition (for an analysis of support for such action vis à vis denominational polity, see Wood, 1970). There is, then, a convergence of evidence that the problem solving role and political and theological liberalism are substantively related to protest orientation and that these variables explain a substantial amount of the variance in rates of protest.

REFERENCES

- Andrews, Frank, James Morgan and John Sonquist
1967 *Multiple Classification Analysis*. Ann Arbor: University of Michigan, Institute for Social Research.
- Bachman, Jerald G.
1970 *Youth in Transition, Volume II, The Impact of Family Background and Intelligence on Tenth-Grade Boys*. Ann Arbor: University of Michigan, Institute for Social Research.
- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: John Wiley & Sons.
- Blizzard, Samuel W.
1956a "The minister's dilemma." *Christian Century* 73 (April 25):508-9.
1956b "Role conflicts of the urban Protestant parish ministers." *The City Church* 7 (September):13-15.
1958a "The Protestant parish minister's integrating roles." *Religious Education* 53 (July-August):374-80.
1958b "The parish minister's self-image of his master role." *Pastoral Psychology* 9 (December):25-32.
1959 "The parish minister's self-image and variability in community culture." *Pastoral Psychology* 10 (October):27-36.
- Campbell, Ernest Q. and Thomas F. Pettigrew
1958a "Men of God in racial crisis." *Christian Century* 75 (June 4):663-5.
1958b "Vignettes from Little Rock." *Christianity and Crisis* 18 (September 29):128-36.
1959a *Christians in Racial Crisis*. Washington, D.C.: Public Affairs Press.
1959b "Racial and moral crisis: the role of Little Rock ministers." *American Journal of Sociology* 64 (March):509-16.
- Carroll, Jackson W.
1971 "Structural effects of professional schools on professional socialization: the case of Protestant clergymen." *Social Forces* 50 (September):61-74.
- Glock, Charles Y. and Rodney Stark
1966 *Christian Beliefs and Anti-Semitism*. New York: Harper & Row.
- Hadden, Jeffrey K.
1969 *The Gathering Storm in the Churches*. Garden City, N.Y.: Doubleday.
- Johnson, Benton
1967 "Theology and the position of pastors on public issues." *American Sociological Review* 32 (June):433-42.
- Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
1970 *Ex-Pastors*. Philadelphia: Pilgrim Press.
- Nelsen, Hart M.
1972 "Sectarianism, world view, and anomie." *Social Forces* 51 (December):226-33.
- Niebuhr, H. Richard
1929 *The Social Sources of Denominationalism*. New York: Henry Holt.
- Olsen, Marvin E.
1970 "Social and political participation of blacks." *American Sociological Review* 35 (August): 682-97.
- Rummel, R. J.
1970 *Applied Factor Analysis*. Evanston, Ill.: Northwestern University Press.
- Stark, Rodney and Bruce D. Foster
1970 "In defense of orthodoxy: note on the validity of an index." *Social Forces* 48 (March):383-93.
- Wood, James R.
1970 "Authority and controversial policy: the churches and civil rights." *American Sociological Review* 35 (December):1057-69.
- Yancey, William L., Leo Rigsby and John D. McCarthy
1972 "Social position and self-evaluation: the relative importance of race." *American Journal of Sociology* 78 (September):338-59.

COMMENTS

ON HUMMON'S MATHEMATICAL FORMULATION OF BLAU'S THEORY OF DIFFERENTIATION IN ORGANIZATIONS

The purpose of this paper is to investigate Hummon's (1971:297-303) mathematical formulation of Blau's (1970:201-18) axioms concerning the effect of size on differentiation in organizations.¹ It will elaborate on Hummon's "new" proposition that states "with increasing organizational size, the rate of increase in the size of the average component itself increases" (1971:300). Hummon claims that this proposition follows from his mathematical model of Blau's axioms, and Blau (1971:305) uses a numerical example (which shall be formalized in this paper) "to show that the axiom implies the very opposite of his new proposition.

Whenever one sets out to build a mathematical model of any physical or social phenomena, one makes certain assumptions that tend to "close" the system under investigation to eliminate variables that contribute negligible information. Hence, one simplifies the mathematics and the model. Hummon's model assumes that the size of an organization can be taken as the "main independent variable" (Hummon, 1971:298) influencing the extent of differentiation within the organization. [Mayhew, James and Childers (1972:750) consider a model involving another variable, "p=the ratio of the force of unification to the force of diversification . . ."]. In addition to this sociological assumption Hummon makes a tacit mathematical assumption that leads him to his "new" proposition.

To illuminate Hummon's error, we briefly sketch his model of Blau's first axioms. Using Hummon's notation, Blau's first axiom is:

V1.0 "Increasing size generates structural differentiation in organizations along various dimensions at decelerating rates."

"Let S = size of organization in number of members."

"Let D = differentiation" (by this we must mean the number of differentiated occupations in the organization).

Here is Hummon's mathematical translation.

If D is a function of S , write $D = f(S)$, and f is twice differentiable, Hummon writes:

$$\begin{aligned} \text{M1.0} \quad & \frac{df}{dS} > 0 \\ & \frac{d^2f}{dS^2} < 0. \end{aligned}$$

The second of Blau's axioms that Hummon models is:

V1.2 "The larger an organization is, the larger the average size of its structural components."

Again, using Hummon's notation, let C denote the average size of a component of the organization and write

$$C = \frac{S}{D} = \frac{S}{f(S)}.$$

Then Blau's axiom V1.2 translates into Hummon's

$$\text{M1.2} \quad \frac{dC}{dS} > 0.$$

Now Hummon's new proposition enters the scene. The mathematical formulation is Hummon's

$$\text{M1.7} \quad \frac{d^2C}{dS^2} > 0, \text{ if } \frac{d^2f}{dS^2} < -\frac{f(S)}{2S^2}.$$

Hummon (1971:300) gives the verbal translation of M1.7 to be:

V1.7 "In practical terms it states that with increasing organizational size, the rate of increase in the size of the average component itself increases."

We can now see that Hummon neglects to mention the mathematical hypothesis in his verbal translation. Hummon's proposition should read: With increasing organizational size, the rate of increase in the size of the average component itself increases if

$$\text{1.0} \quad \frac{d^2f}{dS^2} < -\frac{f(S)}{2S^2}.$$

¹ We wish to thank Professor R. J. Greechie for calling these two papers to our attention.

In other words, Hummon is tacitly assuming that 1.0 always holds. Hummon's argument to support M1.7 is mathematically correct, so the pertinent question is: Do functions exist which could be reasonably utilized in this model, relating S and D in such a way that 1.0 does not hold; i.e.

$$\frac{d^2f}{dS^2} \geq -\frac{f(S)}{2S^2}?$$

Before continuing the mathematics, we offer a verbal translation of 1.0. Essentially, it goes like this: Increasing organizational size generates structural differentiation at a rate that is less than the negative of the number of differentiated classes divided by twice the square of the size of the organization. In view of this, it seems that hypothesis 1.0 of M1.7 was motivated more by mathematical insight than sociological evidence.

To return to our original question, we begin with a mathematicized version of Blau's (1971: 304-7) example.

BLAU'S COUNTEREXAMPLE

Suppose one considers organizational sizes of 100, 200, 400, and 800 employees, (these are the values of S), with 5, 10, 20, and 40 components respectively (these are the values of D). One identifies the function f , relating S and D to be linear in this example. In fact, we can explicitly write the rule for the function that relates these two variables. Define f by

$$f(S) = \frac{S}{20}.$$

Then, for example, when $S=100$, we have that

$$f(S) = D = \frac{100}{20} = 5, \text{ etc.}$$

Calculating the desired derivatives of this function we find

$$\frac{df}{dS} = \frac{1}{20} > 0,$$

satisfying M1.0(a). However

$$\frac{d^2f}{dS^2} = 0 \text{ and } -\frac{f(S)}{2S^2} < 0,$$

(for all $S > 0$, which is obviously the case). Since zero is larger than any negative number, Hummon's assumption 1.0 stating that

$$\frac{d^2f}{dS^2} < -\frac{f(S)}{2S^2}$$

clearly does not hold.

The fact that

$$\frac{d^2f}{dS^2} = 0$$

for this example suggests that one may wish to reword V1.0 slightly to read: Increasing size generates structural differentiation in organizations along various dimensions at *non-increasing* rates. Then, making the corresponding change in M1.0(b), we have

$$\frac{d^2f}{dS^2} \leq 0.$$

This change is necessary for mathematical consistency. To write "<" in M1.0(b) means the rate of change in structural differentiation *must* change. To write " \leq " allows for the case when this rate of change is constant, (which is the case in Blau's example).

With this minor modification we may continue to get

$$C = \frac{S}{D} = \frac{S}{f(S)} = \frac{S}{\frac{S}{20}} = 20.$$

So, we have that

$$\frac{dC}{dS} = 0, \text{ and } \frac{d^2C}{dS^2} = 0.$$

This indicates that one might rewrite Hummon's M1.2 to read

$$\frac{dC}{dS} \geq 0.$$

Here again, by writing " \leq " we allow for the case when the rate of change in the average size of a component with respect to organizational size remains constant.

Furthermore, if one writes Hummon's M1.7 to agree with Blau's example (and a model should agree with the facts known about the system), we would have the exact opposite:

$$\frac{d^2C}{dS^2} \leq 0.$$

Verbally this says that the average size of a component is non-decreasing at a non-increasing rate. It is well to note that this relationship enters the model as a conjecture based on empirical evidence analyzed by Blau. It is *not* forced on us by the mathematics. It is also worth no-

ting that by calculating the appropriate first and second differences (which roughly corresponds to calculating the first and second derivatives for continuous data) from the data collected by Childers et al. (1971:825) we find that one may slightly modify this conjecture to say: The average size of a component increases with increasing size, but the rate of the increase tends to stabilize with increasing size. This is supported mathematically by the fact that some second differences of average size of a component (second difference in C) divided by the corresponding second differences in organizational size, (second differences in S) may, in fact, be negative or positive, but in either case these numbers tend to be small (usually of the

order $\frac{1}{100}$ or less) in absolute value.

A NON-LINEAR COUNTEREXAMPLE

We now demonstrate that Hummon's "new" proposition is untenable for a non-linear example that is not, in a mathematical sense, a borderline case. We consider the function f relating D and S to be

$$D = f(S) = 2\sqrt{S}.$$

Then, for organizational sizes, (values of S), of 100, 200, 400, and 800, the functional values, (values of $D = f(S)$) are 20, 28, 40, and 57, rounding to the nearest integer.

For this function, we have

$$\frac{df}{dS} = \frac{\sqrt{S}}{S} > 0$$

for $S > 0$ (which is always the case), and

$$\frac{d^2f}{dS^2} = -\frac{\sqrt{S}}{2S^2} < 0,$$

as desired to satisfy M1.0(a) and M1.0(b). Verbally, these two inequalities say that as organizational size increases so does the number of differentiated classes in the organization, and that this increase occurs at a decreasing rate.

CONTINUING THE CALCULATIONS

$$C = \frac{S}{D} = \frac{S}{f(S)} = \frac{S}{2\sqrt{S}} = \frac{\sqrt{S}}{2}.$$

So

$$\frac{dC}{dS} = \frac{\sqrt{S}}{4S},$$

satisfying Hummon's M1.2, and indicating that the average size of a component increases with increasing size.

However,

$$\frac{d^2C}{dS^2} = -\frac{\sqrt{S}}{8S^2} < 0,$$

contrary to the conclusion of Hummon's M1.7, which is the mathematical statement of his "new" proposition. It is important to notice that we also do not have

$$\frac{d^2f}{dS^2} < -\frac{f(S)}{2S^2}.$$

That is, the assumption 1.0 of M1.7 does not hold. This becomes clear when we assume that the assumption 1.0 does hold. Then we have

$$\frac{-\sqrt{S}}{2S^2} < -\frac{2\sqrt{S}}{2S^2}$$

or, multiplying by $-2S^2/\sqrt{S}$,

$$1 > 2,$$

which clearly is not the case!

SUMMARY AND CONCLUSIONS

The mathematical statement of Hummon's new proposition V1.7 is actually

$$\frac{d^2C}{dS^2} > 0.$$

Hummon shows that

$$\frac{d^2C}{dS^2} > 0 \text{ if } \frac{d^2f}{dS^2} < \frac{-f}{2S^2}.$$

We have shown, by example that it is not always true that

$$\frac{d^2f}{dS^2} < \frac{-f}{2S^2}.$$

That is, the hypothesis 1.0 of M1.7 does not always hold. Hence one cannot conclude that

$$\frac{d^2C}{dS^2} > 0,$$

and its verbal counterpart always holds. It was also demonstrated that 1.0 has a quite awkward verbal interpretation. Therefore, Hummon's new proposition is not mathematically invalid but it is sociologically irrelevant.

Blau's example suggests that the exact opposite is true, that is,

$$\frac{d^2C}{dS^2} \leq 0.$$

However, keeping in mind that both Blau's example and the non-linear example are mathematically "perfect" and noticing that in both examples we have that

$$\left| \frac{d^2C}{dS^2} \right|$$

gets small with increasing organizational size, (in fact this number is zero for Blau's example and .008 when $S=100$ for the non-linear example). We are thus led to the following conjecture: The average size of a component increases with increasing size but the rate of this increase tends to stabilize (become constant) with increasing size. This conjecture is also supported by the following facts. First, Childers et al. data shows that appropriate second differences (corresponding to d^2C/dS^2) may be either positive or negative, but always small in absolute value. Secondly, we have shown that

$$\frac{d^2C}{dS^2} \geq 0$$

is untenable. But, assuming that

$$\frac{d^2C}{dS^2} \leq 0,$$

we are logically forced into the converse of Hummon's hypothesis 1.0, that is, it must be that

$$\frac{d^2f}{dS^2} > -\frac{f(S)}{2S^2},$$

which yields a complicated and unexplainable verbal translation analogous to 1.0. So, perhaps, the question of whether

$$\frac{d^2C}{dS^2}$$

is positive or negative is not so important as the question concerning its magnitude.

FOSTER G. DIECKHOFF
Kansas State University
Department of Mathematics

REFERENCES

Blau, Peter M.

1970 "A formal theory of differentiation in or-

ganizations." *American Sociological Review* 35 (April):201-18.

1971 "Comments on two mathematical formulations of the theory of differentiation in organizations." *American Sociological Review* 36 (April):304-7.

Childers, G. W., B. H. Mayhew, Jr. and L. N. Gray

1971 "System size and structural differentiation in military organizations: testing a baseline model of the division of labor." *American Journal of Sociology* 76 (March):813-31.

Hummon, Norman P.

1971 "A mathematical theory of differentiation in organizations." *American Sociological Review* 36 (April):297-303.

Mayhew, B. H., Jr., T. F. James and G. W. Childers

1972 "System size and structural differentiation in military organizations: testing a harmonic series model of the division of labor." 77 (January):750-65.

Simon, H. A.

1957 *Models of Man*. New York: Wiley.

COMMENT ON WILLIAMS' "THE ECOLOGICAL APPROACH IN MEASURING COMMUNITY POWER CONCENTRATION"*

I wish I could be as enthusiastic in defending the MPO ratio as Mr. Williams is in attacking it. For me it was simply the best approximation to the operationalization of a concept of power distribution within a system that I could find at the time. The use of data for a purpose not intended by the producer always leaves something to be desired. But the principle I tried to represent with the MPO ratio is still a useful one. So I will speak to the principle rather than to the ratio as such.

Evidently the author gave my paper a very hasty reading, for he seems to have acquired some rather distorted notions about it. In the first place, the ratio is constructed not of the "total" number of managers, proprietors and officials, but only those "not elsewhere classified." Omitted were technical personnel who appeared not to occupy policy positions. The census year used for the construction of the ratios was 1950, which is one of the reasons for my dissatisfaction with the measure. One departure from this basis of calculation occurred in connection with the comparison of cities of 15,000 to 50,000 population with cities of 50,000 population and over. MPO's "not elsewhere

* Williams, James M.

1973 "The ecological approach in measuring community power concentration: an analysis of Hawley's MPO ratio." *American Sociological Review* 38 (April):230-42.

classified" were not reported separately for cities of less than 50,000 population. In that one instance all MPO's were used. Everywhere else in the study MPO's "not elsewhere classified," as reported in 1950, were used, Mr. Williams' belief notwithstanding.

Second, there is no "logical flaw" in the interpretation of the ratio as a measure of power distribution. True it is that the ratio does not capture all conceivable variations of distribution, nor did I make any such extravagant claim. To infer that the distribution is more concentrated where the MPO ratio is small than where it is large, given the assumption that the amount of power is equal in similar systems, may be a descriptive fault, but it is not a logical one. The author should consult his dictionary.

Third, my use of the power concentration hypothesis was limited to innovative programs. Expressly excluded were firmly established components of community structure, such as is manifested in the welfare sector. Presumably the latter is a resolution of an earlier exercise of system power.

Mr. Williams bases his criticism of my approach to power on the results of correlations of the MPO ratio with case-study findings. It is interesting to note that where he obtains some slightly significant results which show a direct relationship he questions the quality of the data. He suggests that, since seven different researchers produced the data, there is probably some lack of standardization among them. Where, however, he found results to his taste, using data from fifty-five case-studies by almost as many different researchers, the data problem doesn't disturb him. There is no point in my engaging in an evaluation of case-study data. Every educated social scientist is familiar with the problem. It is gratuitous to observe that by contrast survey or census data are vastly superior for quantitative purposes.

The author moves on to a comparison of "decentralization scores," obtained by Terry Clark from his NORC study of fifty-one communities, with the respective MPO ratios. The correlation is found to be negative and at a significant level. But this does not warrant a conclusion that the MPO ratio fails to measure power concentration. The question of which measures what remains open, at least until a third, independent, and theoretically acceptable indicator is produced. It is of more than passing interest to note, however, that Clark's "decentralization scores" show a negligible relation with his measure of output-urban renewal expenditures, whereas the MPO ratios proved to have a much stronger and statistically significant relation to output.

The inconclusiveness of the correlation is conceded by the author. He then does look for independent evidence of power concentration. Since no one else has provided him with a useful alternative, he turns to the results of other studies that employ the same reputational-sociometric method of identifying power. Unfortunately the number of communities that occur in two or more studies is so small and the results of comparisons so inconsistent that no conclusion can be supported. This does not prevent the author from making very confident assertions. Mr. Williams is obviously a man of strong convictions.

I have come to the opinion after trying to follow the strange reasoning in this paper that the concept of power as a system property is so foreign to the author's social psychological predilections that he is unable to grasp its meaning. Although I thought I had made my interpretation of the MPO ratio clear, he persists in regarding it as an indication of a "covert elite." He is also unable to recognize the basis on which I assume that similar systems represent equal amounts of power. Incidentally, he quotes me correctly in the text on this point, and then misrepresents my remarks in his footnote number 4. It is entirely possible, of course, that I have failed in my efforts to communicate.

I am somewhat puzzled over what might be the intent of Mr. Williams' paper. If it is to submit the MPO ratio to careful examination, he should have looked for better contrary evidence and have given more thought to the theoretical issues. Indeed if his purpose is simply to demolish the concept, the more circum-spect approach would have been effective enough. In either case his criticism would have been much stronger had he been able to argue it from a strong theoretical position.

AMOS H. HAWLEY
University of North Carolina

REPLY TO HAWLEY

Although the belated clarification of the two ways in which Hawley originally computed the MPO ratio may be of some historical interest, it changes nothing. MPO ratios were lowest in "execution stage" cities under 50,000 as well as over, a point that Hawley used to bolster his theory. Strangely, his original article fails to mention that two methods were used, and his "Reply to Straits" (1965) flatly denies it. In any case, the issue would be crucial only if the two methods produced ratios that varied independently. I would be very surprised if MPO ratios

computed by the two methods for the same cities did not correlate at a level of .9 or above.

I fail to see the logic of the argument that the *apportionment* of managerial functions among the subsystems of a community system can be determined by the ratio of all these functions to all other functions in the total system. The only way this can be determined is by *identifying* the subsystems and then computing the proportion of all managerial functions that are located within each subsystem. If all subsystems contained the same proportion of managerial functions, there would be no concentration of power, according to Hawley's theory. Any departure from this condition of "perfect" equality would represent some concentration. Hawley's MPO ratio, since it ignores subsystems, cannot possibly reveal the kind of functional concentration implied by his theory.

Hawley neglects to comment on the two most significant questions raised by the paper. Why should MPO ratios vary inversely with concentration of power? My explanation is highly speculative, and the link between the MPO ratio, middle class composition, governmental structure and power concentration is by no means clear. Secondly, the implications of the findings for a theory of community innovation need to be spelled out in detail. Aiken and Alford (1970) have developed an alternative model in which initial innovation is seen as occurring within the context of a decentralized system of power relationships, while performance becomes the responsibility of a community decision organization specially created for the task. If this is correct, it means that Hawley's postulated link between innovation and power concentration is correct if applied at the level of the subsystem. Stated otherwise, community innovations require a diffuse power system in order to get accepted, but the subsystem responsible for their execution will perform more effectively if it has a concentrated power arrangement. This theory is consistent with Hawley's original finding that low MPO's are found in cities that have reached the execution stage of urban renewal, since we now know that a low MPO is associated with a diffuse community power structure. Thus it seems that Hawley's theory was correct, but for a different level of community organization and a different stage of the innovation process. The empirical relationship between the MPO and innovation is also correct, but for a different reason.

JAMES M. WILLIAMS
University of Wisconsin-Eau Claire

REFERENCES

- Aiken, Michael and Robert R. Alford
1970 "Community Structure and Innovation: The Case of Urban Renewal." *American Sociological Review* 35 (August):650-65.
Hawley, Amos
1965 "Reply to Straits." *American Journal of Sociology* 71 (July):82-4.

COMMENTS ON "HALL'S PROFESSIONALISM SCALE: AN EMPIRICAL REASSESSMENT"

In the February 1972 issue of the ASR, William Snizek employed factor analysis in his assessment of Hall's Professionalism Scale. Unfortunately, certain information crucial to an adequate assessment of the article was omitted. Snizek failed to specify what type of factor analysis model (e.g., component factor analysis, common factor analysis, image factor analysis) was used to analyze the data (Rummel, 1970: 101-32). Knowing which factor analysis model was used or which estimates of communality (e.g., unity or squared multiple correlation) were used would have shown whether the five factors being considered represented the basic dimensions of the *total* variance in the fifty items of Hall's scale, or the basic dimensions of just the *common* variance in the fifty items.

Snizek did not show what percent of the total variance of Hall's scale was accounted for by the five factors. Our calculations, based on the orthogonally rotated factor matrices given in Snizek's Table 1, indicate that the five factors accounted for 34.34% of the total variance for Hall's data and 30.87% of the total variance for Snizek's data. This information, in the absence of any information concerning the eigenvalues of the unrotated principal axes factors, suggests that other major sources of variance in Hall's scale were not accounted for by the five factors. Whether these other sources of variance are additional factors, specific variance or error variance remains unanswered in Snizek's article.

While Snizek correctly extracted the first five principal axes factors from the two sets of data to reflect empirically the five theoretical dimensions underlying Hall's Professionalism Scale, he rotated them to an orthogonal varimax solution without indicating whether he had checked for an oblique factor structure within the two sets of data. Without checking for such a structure, one cannot argue that the scale's underlying dimensions are unidimensional or independent, since orthogonality is a *necessary* outcome of a varimax factor matrix rotation

(Cartwright, 1965). Even in checks for an oblique factor structure, inter-factor correlations are often influenced by the estimates of communality used in the analysis. For example, oblique component factors generally have smaller inter-factor correlations than oblique common factors.

The inter-dimension product moment correlations which Snizek calculated as a check on the unidimensionality of each of the revised dimensions of Hall's scale (five items summated per dimension) indicate that some of the revised dimensions within each set of data are interrelated (e.g., $r = .373$ for dimensions 1 and 4 in Hall's data and $r = .219$ for dimensions 2 and 4 in Snizek's data). These inter-dimension correlations also suggest a different structure of relationships among the dimensions of Hall's scale across the two sets of data (e.g., dimensions 1 and 5 correlate .256 in Hall's data but only .084 in Snizek's data).

On the basis of his factor analyses, Snizek (1972:111) commented that, "approximately half of the fifty items formulated by Hall have less than an acceptable (varimax factor loading on their appropriate theoretical dimension." Some items had little empirical fit with the theoretical dimensions, others were multidimensional, while still others had their highest absolute factor loadings on inappropriate theoretical dimensions (Snizek, 1972:111-12). Since these findings involved a number of items in both sets of data, Snizek, in hopes of improving Hall's scale for immediate research needs, deleted several items from Hall's scale. Yet, some of the remaining "better" items, as indicated in Snizek's Table 1, appear little better than the items deleted. Several of the "better" items have their highest factor loadings on inappropriate theoretical dimensions (e.g., items 6, 8, 16, and 36 for Snizek's data; items 14 and 39 for Hall's data); still other "better" items remain multidimensional (e.g., items 26 and 47 for Snizek's data; items 24 and 49 for Hall's data). The lack of an explicit criterion for "acceptable" factor loadings with respect to the magnitude and pattern of the items' factor loadings across the five dimensions of Hall's scale may have contributed to this problem.

Snizek attempted to define the best twenty-five items of Hall's Professionalism Scale (five items per dimension) on the basis of the rotated varimax factor loadings for the fifty items in both sets of data. We would caution that deleting half of the items from Hall's scale may change the overall pattern of relationships among the remaining variables enough to alter the results of a subsequent common factor

analysis of the remaining twenty-five items (Rummel, 1970:103-4). Whether a common factor analysis of just the twenty-five "better" items in Hall's scale would show a clearer empirical fit of those items to the underlying theoretical dimensions than Snizek's factor analysis of the fifty original items remains an empirical question.

We suggest that the results of a factor analysis may be unique to the interaction of the items and population under consideration at a given time (Cattell, 1952:323). There may be differences between Hall's and Snizek's data sources great enough to invalidate Snizek's assessment across both sets of data. Snizek (1972:112) questions the scalability of many items and is "led to believe that the scale's difficulties lie with its items, rather than its sampling idiosyncracies." While we agree that Hall's scale could benefit from item content modification and other revisions, its fifty items did not vary for Hall's or Snizek's data; while the occupational composition of the two samples did. Hall's (1968:96) sample of 328 respondents included fifteen engineers, while Snizek's (1972:110) sample of 566 consisted primarily of engineers. Any differences between the factors and factor matrices of Hall's and Snizek's data could be a function of the differences between the occupational composition of the samples as well as problems considered inherent in the items.

To investigate the endogenous similarities and differences between the factors and factor matrices of Hall's and Snizek's data, a topic Snizek almost completely omitted from his article, we orthogonally rotated the factor matrix of Snizek's data to its best least squares fit with the factor matrix of Hall's data (Schonemann and Carroll, 1970).¹ Table 1 gives the residual difference between the factor matrix of Hall's data and that of Snizek's data orthogonally rotated to its best least squares fit. The mean of the squared residuals is .03, showing that the absolute mean deviation between the corresponding factor loadings in the two factor matrices was .17.²

¹ The contraction parameter was 1.0, and the central dilation parameter for each of the five factors was 0.0.

² The normalized symmetric error was equal to .03. The mean of the squared residuals may have differed had we orthogonally rotated the factor matrix of Hall's data to its best least squares fit with the factor matrix of Snizek's data. Since Hall's data appeared more heterogeneous in occupational composition, we rotated the factor matrix of Snizek's data to the factor matrix of Hall's data.

An examination of Table 1 shows that the distribution of the residuals across the five factors differs. The mean of the squared residuals (X^2 Sq Resd) for factor I (Using the Professional Organization as a Major Referent) is .06, while the mean of the squared residuals for factor V (Autonomy) is .01.³

To assess the degree of similarity between Hall's five factors and Snizek's factors orthogonally rotated to their best least squares fit with Hall's factors, we calculated the coefficients of congruence and the root square mean coefficients between all factors from both sets of data (Harman, 1967:269-71; Rummel, 1970:460-3). The coefficients of congruence given in Table 2 are equal to the cosines of the angles between the factors in the common variable space of the fifty items. These coefficients, at best, indicate a moderate degree of similarity between factors II, III, and IV for Hall's and Snizek's data. Factor V (Autonomy) is very similar for the two sets of data, while factor I (Using the Professional Organization as a Major Referent) is clearly the most dissimilar. Whether the difference between factor I for the two sets of data is a result of inter-occupational attitudinal differences or variations in respondents' occupational settings cannot be determined by our analysis.

The coefficients of congruence in Table 2 also indicate a common pattern for the five factor loadings in both sets of data. Factors I, II, IV, and V are dominated by positive factor loadings, while factor III (Belief in Self-Regulation) is dominated by negative loadings. A plausible explanation for this occurrence may be that the items with the larger factor loadings on factor III suggest not a belief in the self-regulation of a profession but rather a belief that one has no opportunity to evaluate the work of others in the profession (e.g., items 18, 33, 43, and 48 in both Hall's and Snizek's data).

Table 3 gives the root square mean coefficients between the five factors of Hall's and Snizek's data. These coefficients are a stringent measure of the factors' degree of similarity in that they measure any deviation in the magnitude or pattern of the factor loadings of two factors. Since root square mean coefficients are proportional to the Euclidean distance between the factors, the more these coefficients deviate from zero, the poorer the correspondence between the factors.

Table 1. Residual Matrix R

Theoretical Dimensions		Empirical Dimensions				
Item No.		I	II	III	IV	V
I	* 1	-.02	.03	-.11	.04	.20
	* 6	.20	-.11	.15	.07	.14
	11	.18	-.04	-.09	.07	.05
	* 16	.19	-.04	.18	.01	-.03
	21	.16	.11	.02	-.12	-.04
	* 26	-.02	.07	.06	-.42	-.01
	31	-.27	.22	-.02	-.06	.03
	* 36	-.04	-.08	-.06	-.08	.08
II	41	.14	.36	.06	-.18	.03
	46	-.27	.29	-.09	.15	.09
	* 2	-.43	.14	-.15	.15	.07
	* 7	-.19	.09	-.21	.03	-.04
	* 12	-.28	.09	-.03	.15	-.07
	* 17	-.16	.10	-.06	.01	.05
	22	-.30	-.04	.04	.00	.08
	27	-.16	.01	-.04	-.04	.08
III	32	-.39	.13	-.34	-.14	.06
	37	-.35	.05	.01	.03	.06
	42	-.09	.17	.02	-.14	.22
	* 47	-.08	.37	-.05	-.06	.02
	3	-.04	.17	-.10	-.12	.08
	* 8	-.13	-.04	-.08	.08	.04
	13	.14	.23	-.08	-.24	-.11
	* 18	-.07	-.01	-.09	.15	-.04
IV	23	.39	.36	-.47	-.54	.03
	28	.17	.19	-.09	-.27	.05
	* 33	-.34	.01	-.17	.07	-.11
	38	.27	.11	-.02	-.08	.03
	* 43	-.42	-.01	-.21	.04	.00
	* 48	-.27	-.11	-.12	.17	-.06
	4	.02	-.05	-.14	.06	.02
	* 9	.32	-.12	.00	.03	-.06
V	* 14	.44	-.21	.16	.16	-.08
	19	.04	.17	-.54	.62	-.08
	* 24	.41	-.22	.24	.01	-.01
	29	.54	-.44	.30	.22	.03
	34	.11	-.21	-.12	.14	.07
	* 39	.32	-.01	.09	.06	-.08
	44	-.04	.28	.01	.06	-.03
	* 49	.33	-.05	.17	.05	-.08
	* 5	.05	-.11	.07	.00	.02
	10	-.14	.05	-.07	.03	.06
	* 15	.09	.07	.04	.07	.01
	20	.10	-.10	-.09	-.12	-.04
	* 25	-.01	.28	-.14	-.03	.07
	30	-.20	.12	-.05	.16	.28
	35	.18	-.19	.10	-.01	-.05
	* 40	.11	-.04	.01	-.01	.00
	* 45	.05	-.03	.10	.09	-.03
	* 50	.24	.05	.11	-.09	-.03
X Sq Resd		.06	.03	.03	.03	.01

³ An index of deviation for each of the fifty items across the two sets of data can be calculated by summing the squared residuals for each item in Hall's scale (Rummel, 1970:468).

* Items selected by Snizek as best empirically corresponding to their proper theoretical dimension.

Table 2. Coefficients of Congruence

Hall's Factors	Snizek's Factors				
	I	II	III	IV	V
I	.586	.339	-.442	.284	.258
II	.331	.735	-.018	.305	.163
III	-.442	-.018	.820	-.156	-.203
IV	.311	.341	-.171	.786	.072
V	.258	.167	-.204	.066	.953

The root square mean coefficients given in Table 3 indicate the same general findings as the coefficients of congruence in Table 2 except that they show more precisely the problematic nature of factor I in Snizek's data. While this factor most closely resembles Hall's factor I, it also approximates factors II and IV in Hall's data, suggesting that Snizek's factor I may be a more general dimension of professionalism than Hall's. Lacking information on the eigenvalues of the unrotated principal axes factors in both sets of data, we can only speculate on the question of whether less than five factors might equally well describe (empirically) the extracted variance in Hall's and Snizek's data.

While factor analysis may be an elegant and powerful tool for analyzing scales, its potential can only be realized after careful thought has been given to its intended objectives. This process must involve a consideration of the more general theoretical and methodological issues underlying factor analysis, as well as the issues underlying a particular scale.

In summary, we suggest that more empirical research across a broader spectrum of occupations and occupational structures be undertaken to improve Hall's Professionalism Scale. We

need such information to refine and develop Hall's scale and the dimensions of professionalism before we can realize the scale's potential to reflect the dimensions of professionalism under varying conditions.

JOHN W. FOX

Western Michigan University

JOHN A. VONK

University of Northern Colorado

REFERENCES

- Cartwright, Desmond S.
1965 "A misapplication of factor analysis." *American Sociological Review* 30 (April): 249-51.
- Cattell, Raymond B.
1952 *Factor Analysis*. New York: Harper and Brothers.
- Hall, Richard H.
1968 "Professionalism and bureaucratization." *American Sociological Review* 33 (February): 92-104.
- Harman, Harry H.
1967 *Modern Factor Analysis*. Illinois: University of Chicago Press.
- Rummel, R. J.
1970 *Applied Factor Analysis*. Evanston, Illinois: Northwestern University Press.
- Schonemann, Peter H. and Robert M. Carroll
1970 "Fitting one matrix to another under choice of a central dilation and a rigid motion." *Psychometrika* 35 (June): 245-55.
- Snizek, William
1972 "Hall's Professionalism scale: an empirical reassessment." *American Sociological Review* 37 (February): 109-14.

REPLY TO FOX AND VONK *

Fox and Vonk raise several interesting and provocative questions, the majority of which are procedural in nature and warrant only a brief reply, while others require some elaboration since they involve basic differences in methodological approach. I welcome the opportunity to clarify questions of the first type in order to address some general issues of greater import.

As to the procedural questions, I used a principal-axis factor analysis with squared multiple correlations placed in the diagonal of the 50 x 50 correlation matrix, thereby estimating the communality of each variable. Having first checked for an oblique factor solution and found it not to be present to any discernible degree, I rotated both sets of data for two to ten factors

* I wish to express my appreciation to Lawrence Mayer and David Klemmack for their valuable advice and comments on an earlier draft of this reply.

Table 3. Root Square Mean Coefficients

Hall's Factors	Snizek's Factors				
	I	II	III	IV	V
I	.239	.285	.445	.319	.319
II	.286	.169	.353	.297	.319
III	.445	.353	.158	.405	.406
IV	.298	.275	.389	.169	.346
V	.319	.319	.406	.363	.082

using normal Kaiser Varimax criteria. Upon analysis, I chose a five factor orthogonal solution as yielding a preferable description of the correlation matrix due to: (1) the marked drop in total unit variance extracted once five factors had been surpassed; (2) the fact that a five factor solution appeared to divide the item loadings in a more discernible (patterned) way than any other rotations attempted; (3) theoretical considerations, in that Hall had constructed the items so as to tap five theoretically distinct dimensions of professionalism! Having selected a five factor solution, I again analyzed both sets of data after twenty-five items had been deleted with little or no alteration in the factor solution. I hope this clarification is sufficient to explicate the procedures I employed. Such clarification, however, in no way modifies my original findings.

In reacting to Fox and Vonk's remarks concerning my "lack of an explicit criterion for 'acceptable' factor loadings," I would only answer that I know of no such "explicit" criterion; and they do not present one. I would argue that one must evaluate the "acceptability" of a given factor loading vis-à-vis others in both horizontal and vertical proximity to it. Furthermore, I find Fox and Vonk's conclusions regarding the interrelated nature of scale dimensions based on their comparisons of inter-dimensional product moment correlations (Table 2 in the original article) to indicate a basic misunderstanding of the article's intent. In nine of ten pairings of both Hall's data and my own, the inter-dimensional product moment correlations were shown to be diminished using five as opposed to ten items. The important point is the relative improvement effected by the revised, in contrast to the original number of scale items for each of Hall's five dimensions. Albeit a few of the correlations between dimensions are higher than one would like; but the vast majority were diminished, a fact suggesting improved unidimensionality in the dimensions of Hall's original scale. Fox and Vonk apparently choose to disregard relative improvement in favor of absolute precision, when in fact the goal of my analysis, and of the article, was to offer suggestions for an immediate and "near term" modification of Hall's original scale.

Perhaps the clearest and most basic indication of Fox and Vonk's misunderstanding of the

intent of my article can be seen in their investigation into the "endogenous similarities and differences between the factors and factor matrices of Hall's and Snizek's data, a topic Snizek almost completely omitted from his article." Obviously such a "topic" was purposely omitted from my article since I recognized that the factors extracted from Hall's data and my own could not possibly be the same due to the interaction between the factor solutions and the occupational composition of those surveyed. In short, no attempt was made to "compare" factors extracted from both sets of data. Instead, I analyzed the two data sets for replicative, rather than comparative purposes to gain a validity check on item deletions. Of particular interest to me in this connection is why Fox and Vonk would indicate their awareness of the fact that the factors extracted from both sets of data could not possibly be identical, and then proceed to rotate my factor solution on Hall's to prove they are in fact not identical. Such logic suggests either a misunderstanding of what I did in my analysis, what they did in theirs, or the shopworn "straw man" stratagem.

In conclusion, I am sure both Hall and I would concur with Fox and Vonk in advocating, "more empirical research across a broader spectrum of occupations and occupational structures be undertaken to improve Hall's Professionalism Scale." Yet with its methodological shortcomings, I would like to underscore the enormous contribution made by Hall in constructing a device for measuring professionalism, lest this contribution be buried amid a myriad of co-efficients.

WILLIAM E. SNIZEK
*Virginia Polytechnic Institute
and State University*

ERRATUM

The last sentence of footnote 3 on page 744 of Professor Theodore D. Kemper's article, "The Division of Labor: A Post-Durkheimian Analytical View," which appeared in the December, 1972, issue of ASR, should read as follows: "Thus, in Figure 1, networks B, D, F, and G are trees, while A, C, and E are not."

ITEMS (Continued)

lem of collective definitions of social issues and problems. His collaborator, **Peter M. Hall**, is currently professor and chairman of the Department of Sociology at the University of Missouri, Columbia. His substantive interests are in political sociology, collective behavior and social movements, and social psychology. He is also engaged in integration of perspectives in analyzing U.S. and Canadian societies, with collaborator Dalton Kehoe, a former colleague at York University.

■ **Hart M. Nelsen, Raytha L. Yokley, and Thomas W. Madron**, are respectively, Associate Professor and Professor of Sociology and Professor of Government at Western Kentucky University. The trio has been investigating politicization and

socialization within black and white churches. They have recently undertaken a national study of clergy focussing on role and career orientations. Nelsen's interests are in sociology and religion and minority groups (and the intersection of the two). Nelsen and Yokley are co-editors of *The Black Church in America*. Yokley's other interests include race and culture and ideologies. Madron's publications are in methodology and political parties as well as in sociology of religion.

■ Have a good summer. We hope to see and greet many of you who have so greatly aided the *ASR* enterprise at the ASA meetings in "fun city."

J. F. S.

University of Chicago—Graduate School of Education
Summer Professional Development Program

MULTIVARIATE ANALYSIS OF QUALITATIVE DATA TRAINING SESSION

August 23-26, 1973

Study in this intensive four-day session will focus on two powerful, user-oriented computer programs for the analysis of qualitative data by log-linear models. The session will include lectures, study groups, and computer laboratory work. Participants are encouraged to bring their own pre-punched data for analysis. Lecturers for the session are R. Darrell Bock of the University of Chicago and Stephen E. Fienberg of the University of Minnesota. For additional information, please write:

R. Darrell Bock
University of Chicago
Data Analysis Training Session
5835 South Kimbark Avenue
Chicago, Illinois 60637

SOCIOMETRY

A Journal of Research
in Social Psychology

Genuinely interdisciplinary in the presentation of works by both sociologists and psychologists.

Recent issues have dealt with:

The Development of Trust and Mistrust in Mixed-Motive Games

Language, Society and Subjective Experience

Task and Social-Emotional Leadership Role Performance

Using Balanced Scales to Control Acquisition

Effects of Sex, Response Order and Expertise in Conformity: A Dispositional Approach

Effects of Observing Athletic Contests on Hostility

Compensatory Reactions to Spatial Intrusion

Dogmatism Attitudes Towards the Vietnam War

Evaluations and Expectations for Performance

Scapegoating: An Alternative to Role Differentiation

Influence Structures

Social Class and Self-Derogation: A Conditional Relationship

\$10 for 4 issues \$14 for institutions

Special rate to members
of the ASA—\$6.00

Order from

THE
AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W.
Washington, D.C. 20036

BLACKER THAN THOU:

THE STRUGGLE
FOR CAMPUS UNITY



by George Napper
Preface by Troy Duster

A black administrator at the University of California during the Berkeley riots, George Napper subsequently interviewed 40 of the students involved as a basis for this new and vital study into the complex sociology of the black student.

Coming through clearly in this well-researched volume are the tensions of the black student who is caught between white middle class values and emerging black identity. Constituting a device rather than a unifying force, contends Napper, is the "blacker than thou" attitude espoused by the black militants.

No "Uncle Tom," Napper contends the black community needs the input of educated blacks; black students, then, must get on with the business at hand—education. 128 pages.

Paper, \$2.45; Cloth, \$4.95

Also recommended:

FOR BLACKS ONLY

Black Strategies for Change in America by Sterling Tucker

"... should be read by everyone concerned with these issues."

—Library Journal

Paper, \$2.95; Cloth, \$4.95

WM. B. EERDMANS
PUBLISHING CO.
Grand Rapids, Michigan 49502



AMERICAN SOCIOLOGICAL REVIEW
Volume 38 **Number 4**

Pope		Parsons' Interpretation of Durkheim	
Movahedi Ogles	Axiomatic Theory	Mayhew	System Size and Ruling Elites
Cole	Functional Alternatives Development	vonBroembsen Gray	System Growth and Power Structures
Jacobson Kendrick	Education and Mobility	Specht	System Size, Structural Differentiation
Ritterband Silberstein	Disorders in Public Schools	Kelley	The Socioeconomic Career
Comments			

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit three copies, and retain a copy for your own files.* Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—doublespaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for *Ibid.*, *op. cit.*, or *loc. cit.*; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated within reference. ["... have claimed that this is so (but see Jones, 1952:99 for conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semi-colons. ["... as many have noted (Johnson, 1942; Perry, 1947; Linquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

- Davis K.
1963a "The theory of change and response in modern demographic history." *Population Index* 29 (October):345–66.
1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.
- Goode, W. J.
1967 "The protection of the inept." *American Sociological Review* 32 (February):5–19.
- Moore, Wilbert E., and Arnold S. Feldman.
1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt (ed.)
1962 *The American College*. New York: Wiley.

* Manuscripts will not be returned unless accompanied by a self-addressed, stamped envelope.

AMERICAN SOCIOLOGICAL REVIEW

AUGUST, 1973

VOLUME 38, No. 4

CLASSIC ON CLASSIC: PARSONS' INTERPRETATION OF DURKHEIM *

WHITNEY POPE

Indiana University

American Sociological Review 1973, Vol. 38 (August):399-415

Parsons' commentary on Emile Durkheim's work, first set forth in The Structure of Social Action and later developed is undoubtedly the most influential interpretation yet to appear in English. Parsons attempts to document Durkheim's shift from positivism to the voluntaristic theory of action and to idealism. Neither attempt is successful. Parsons' interpretation often seems as much a function of his own perspectives as of Durkheim's. Examination of Durkheim's theory in light of his intentions and assumptions reveals serious errors in Parsons' analysis.

TALCOTT PARSONS' *The Structure of Social Action* (1949: first published, 1937, hereafter cited as *The Structure*) is perhaps the most influential book produced in American sociology during the twenties and thirties. Asserting that it "precipitated the sociological outlook that had been implicit in the most interesting of the empirical inquiries" and that it "laid out the main lines of the concrete sociological outlook that has come forward in academic study," Edward Shils (1961:1406-7) calls the appearance of *The Structure* a turning

point in the development of sociology. *The Structure* attempts to document the emergence of the voluntaristic theory of action from three great traditions of Western European thought: utilitarianism, positivism, and idealism. Action entails an actor in a situation oriented toward some goal where selection of means is normatively regulated (Parsons, 1949:44-5). Parsons (1949:46) stresses that the action frame of reference is subjective in that "it deals with phenomena, with things and events as *they appear from the point of view of the actor* whose action is being analyzed and considered."

The Structure (304) argues that "in Durkheim . . . there is a fundamental change, from one set of sharply formulated ideas to another."¹ Parsons (1949:304) identifies four main stages and locates Durkheim's most important works within these stages. His argument may be summarized as follows: During the earliest stage, Durkheim was still attempting to formulate his fundamental problems (see *The Division of Labor*

*This work has been supported by a Public Health Service Predoctoral Fellowship from the National Institute of Mental Health and by National Science Foundation grant number GS 30953. I would like to express my gratitude to the following for their reactions to ideas included in this paper: Reinhard Bendix, Nick Danigelis, Martha McMurry, Garth Massey, Hallowell Pope, Neil Smelser, Edward Tiryakian, and Ruth Wallace. I am especially indebted to Dain Oliver, Jere Cohen, Larry Hazelrigg, and Anthony Giddens for detailed critiques. Lucille Hake has provided valuable research assistance and Carolyn Mullins editing assistance. I would like to acknowledge my great intellectual debt to Barclay Johnson with whom I spent one summer discussing Durkheim. My own interpretation, especially of *Suicide* upon which most of our exchanges focused, owes much to these discussions.

¹ Though Parsons emphasizes change and development in Durkheim, he does not deny the existence of basic continuities. *The Structure* (308) observes that *The Division of Labor in Society* "contains, in germ, almost all the essential elements of Durkheim's later theoretical development."

in Society; hereafter referred to as *Division*). He viewed the actor as constrained by an external reality, both social and nonsocial, toward which he assumed a morally neutral attitude. This perspective implies that the actor's primary motivation is to avoid the imposition of negative sanctions. In the second stage, Durkheim achieved an early theoretical synthesis (*The Rules of Sociological Method*, hereafter cited as *The Rules*, and *Suicide*). The decisive advance represented by this stage is the recognition that the content of the collective conscience is as important as its strength. In the third stage (*Sociology and Philosophy* and *Moral Education*) Durkheim discovered internalization. This represents a radical break with the positivism of the first stage as Durkheim, in coming to view individual behavior as guided by social norms which become internalized to constitute an integral part of the actor's personality, comes to see that in order to explain human behavior the scientist must focus on the subjective states of individual actors. A truly voluntaristic theory must recognize human effort and allow for the very important creative, active, nondeterministic element of free choice in human behavior (Parsons, 1935:282-92; see also Scott, 1963:719-24, 732; Devereux, 1961:19-20). Morality implies an irreducible element of free will for the individual. Hence in coming to recognize the importance of the internalized moral component, Durkheim comes to embrace not only a theory of action but the voluntaristic version thereof. Stage three, however, proved to be transitional. It opened up a vast new range of problems leading to the empirical investigations of the fourth and final stage (*The Elementary Forms of the Religious Life*, hereafter referred to as *The Elementary Forms*). Durkheim, however, was unable to develop this final idealistic phase to the point of achieving a new general theoretical synthesis.

Parsons (1949:305, 441) feels that Durkheim began as a positivist. The underlying thesis of *The Structure* (714, 719-20) is that Durkheim, Max Weber, Vilfredo Pareto, and Alfred Marshall (an economist) converged on the voluntaristic theory of action. In his analysis of Durkheim, however, Parsons (1949:445) holds that, in leaving positivism,

he passed over voluntarism to embrace idealism. But as an idealist Durkheim presumably would be no closer to the voluntaristic theory of action than as a positivist. Elsewhere Parsons (1949:713) prefers the argument that, in the final stage of Durkheim's thought, idealism and voluntarism were locked in an uneasy struggle for supremacy. The latter view predominates.

The argument of this paper is that Durkheim was never a positivist. Durkheim's thought does not undergo the changes identified by Parsons. At no point did he embrace any version of the theory of action. Parsons' interpretation often seems to reflect his own theoretical stance rather than Durkheim's.

FROM POSITIVISM TO VOLUNTARISM

The Actor as Scientist

(Stage one)

The Structure (421; see also 61) defines positivism "as the doctrine that positive science is man's sole significant cognitive relation to external reality." Thus positivists treat "the actor . . . as if he were a scientific investigator" (Parsons, 1949:61). Parsons feels that this is precisely the attitude which the early Durkheim imputes to the actor (Parsons, 1949:380-1, 396-7, 414, 438-9, 468, 709).

At no point, however, does Durkheim even begin to postulate the existence of actors guided by a "scientific" attitude; and, significantly, *The Structure* cites no such passages. *Division* (4) defines a moral rule as "an obligatory means of acting; which is to say, withdrawn from individual discretion." This view is consistent with that found in Durkheim's major late work *The Elementary Forms* (238) where he says that the ascendancy of moral rules is such that they exclude "all idea of deliberation or calculation." Indeed, moral rules automatically cause or inhibit action, "without regard for any consideration relative to their useful or injurious effects" (Durkheim, 1961a:237).

To Parsons (1949:406) Durkheim's reference to the importance of negative sanctioning implies "the avoidance of sanctions as the dominant motive of conformity . . ." But rather than linking negative sanctioning with the attitudes identified by Parsons, Durkheim simply identifies it as one conformity-producing mechanism. Parsons

PARSONS' INTERPRETATION OF DURKHEIM

wants to attribute the attitude of the scientific observer implied in Durkheim's approach to those Durkheim proposes to study. But since Durkheim's theory never postulated the existence of a rational, calculating, scientific attitude on the part of the actor, he was never, in Parsons' terms, a positivist.

Biological Reductionism in Division (Stage one)

A second aspect of Parsons' argument is based on Durkheim's alleged biological reductionism. Commenting on the theory of social change contained in *Division*, Parsons (1949:322-3) argues that, in the last analysis, social differentiation results from population pressure which is "not in any analytical sense a social element at all, but essentially biological." Hence, Durkheim's appeal to population pressure represents "the breakdown of utilitarianism into radical positivism, in this case the 'biologizing' of social theory." Even though this claim that Durkheim's theory has ultimate recourse to a biological factor puts him in the good company of such astute interpreters as Sorokin (1928:480), Benoit-Smullyan (1948:508) and, albeit equivocally, Alpert (1961:91) and Merton (1965:110), careful consideration should put this long standing charge to rest.

Durkheim's theory explains the evolution of mechanical into organic society (Durkheim, 1960a:223-350, esp. 256-82). *Division* (257) treats the rate of social interaction (dynamic or moral density), a pre-eminently social variable, as the key independent variable. Parsons' reference (1949:323) to what Durkheim "ends up with" suggests his attempt to trace Durkheim's causal chain back as far as possible at which point, presumably, a biological variable is encountered. His specific reference (Parsons, 1949:322) to "the increase of numbers" in society, which he equates with "population pressure," indicates that this factor is the crucial one in the biologizing of social theory. But is sheer appeal to the importance of numbers in social life really a form of biological reductionism? Durkheim (1960a:257-60) identifies three principal ways in which "the progressive condensation of societies in historical development is produced": (1) population concentration, (2) formation of cities, and (3)

improved means of transportation and communication. Clearly he is not treating increased numbers as biologically caused.

Perhaps the strongest possible case for Parsons' interpretation would attribute increased numbers not to any of the above social factors but rather to the "biological" factor of higher birth rates. Whether such rates constitute a biological variable depends on the conceptual framework used, as Parsons himself implicitly notes (1949:6-42, esp. 41-2). Durkheim never refers to birth rates as a biological factor; and given his social realism, there is no reason to suppose he thought of them as such. On the contrary, he may have felt it unnecessary to state explicitly that birth rates can be treated as a social factor. Certainly, *Suicide* illustrates how one phenomenon—death by suicide—which is biological from one perspective and psychological or individual from another, is eminently social from yet a third. Furthermore, as early as 1888—well before either *Division* or *Suicide*—Durkheim (1888) maintained that variations in the birth rate were socially caused (Foskett, 1939:119-20). Since Parsons makes no reference to this work, he was apparently unfamiliar with it. In any case, Durkheim (1938:8; 1960a:433n) makes similar statements in works known to Parsons.

Durkheim's view of birth rates is entirely consistent with his portrayal of mechanical society. In this setting in which the process of social differentiation is initiated, the collective conscience is all-powerful and may be presumed to control such important aspects of human behavior as procreation. Whether or not Parsons' reference to increased numbers contains an implicit reference to birth rates, the central point remains: Durkheim treats both the causes and the effects of increased numbers—heightened socially regulated competition and struggle—not as biological but as social phenomena. Far from manifesting any form of biological reductionism, then, the explanation of social differentiation in *Division* follows Durkheim's injunction (1938:110) to explain social facts sociologically.

The argument for biological reductionism is a curious one. *The Structure* (307n, 448) downgrades Durkheim's concern with and development of a theory of social change,

and Parsons never argues that population pressure is integral to any other aspect of Durkheim's theory. Clearly it is not part of the theory of social integration which Parsons treats as central to Durkheim. The analyst is left wondering why a minor part of the overall theoretical structure should merit being judged as evidence that, at this stage in his development, Durkheim was a radical positivist (Parsons, 1949:323).

In terms of his four-stage hypothesis, Parsons' concern is to document the change between *Division* and *Suicide*. "In . . . *Suicide* no further use [is] made of the population factor; indeed it drops out of his work altogether" (Parsons, 1949:327). But nothing in *Suicide* indicates that Durkheim's thinking on the role of numbers in causing the division of labor had changed. *Suicide's* dropping of the "population factor" is a function of the book's focus, not of any change in Durkheim's position. Furthermore, biological factors play an integral role in *Suicide's* explanatory structure. Durkheim (1951:272, 384-5) appeals to biologically determined differences between men and women to explain why the latter consistently display lower suicide rates even when both live in the same "societies" or social conditions. Comparison of the social factor, numbers, in *Division* with this appeal in *Suicide* reveals movement in Durkheim's thought opposite to that postulated by Parsons.

Value Content in Suicide (Stage two)

In *Suicide*, a key work in the hypothesized second stage of Durkheim's thought, Parsons argues that Durkheim comes to recognize the importance of value content. This is important to Parsons because it permits him to interpret Durkheim as coming to acknowledge the importance of an aspect of the social factor which, in a latter stage of his development, Durkheim will come to see as internalized.

Integration and regulation are the two independent variables in terms of which Durkheim (1951) attempts to explain variations in social suicide rates. One of the persistently vexing problems of interpretation posed by *Suicide* concerns the difference between these two variables. Parsons (1949:330-7) argues that integration refers to the

content of the collective conscience; regulation, to its strength. It is his attempt to read value content into Durkheim that is at question. Durkheim calls the state of low integration egoism and high integration altruism. Parsons argues that integration refers to the value placed on the individual and his interests as opposed to that placed on the group and its interests. Parsons and Durkheim agree that in altruism the individual is subordinate to the group. Parsons holds that this is not a question of the strength of integration but rather of values. Durkheim (1951:220-1) holds otherwise: ² "For the individual to occupy so little place in collective life he must be almost completely absorbed in the group and the latter, accordingly, very highly integrated."

Parsons misses *Suicide's* (226-7, 387) contention that belief and value systems are derived phenomena. If they directly reflect the anatomical constitution of society (Durkheim, 1951:387), they also reflect the all-

² Parsons (1949:330) also misconstrues the nature of the individual's subordination to the group in altruism. Comparing *Division* and *Suicide* he observes that "the altruistic factor in suicide is . . . on essentially the same theoretical plane as mechanical solidarity. . . . [But] there is a slight shift of emphasis. It is no longer similarity which is the central point, but subordination of individuality to the group." Parsons is mistaken on both counts. Durkheim's position concerning the lack of individuality in the primitive, mechanical, or altruistic setting remains consistent (Durkheim, 1960a:130, 194; 1951:220, 238n, 336). Further, similarity or commonalities remain central (Durkheim, 1951:170, 202, 302). Parsons (1949:330) cites the army, a group which is highly integrated but nonetheless differentiated, as a counter-example. In doing so, however, he fails to notice that Durkheim (1951:234, 236, 238) does not speak of integrative mechanisms specific to or contingent on differentiation. Rather, he stresses the lack of individuality in the army and notes the primitive society-army analogy, integration in primitive society being based not on differences or individuality but precisely on all pervasive commonalities. Parsons' example undermines his own argument.

The reason commonalities are so crucial for Durkheim becomes especially clear in *The Elementary Forms* where Durkheim stresses that only similar individual representations can fuse to form collective representations.

By denying that individuality and extent of commonalities are linked in a constant, determinate way with integration, Parsons is able to deny that they are the crucial variable aspects of integration, thereby paving the way for interpreting the latter as really a matter of value content.

important strength of social control. The low valuation placed on things individual—individuality, individual personality, even the survival of the individual—is, in Durkheim's view, an inevitable reflection of that social reality in which the group is everything and individuality and individual personality are virtually non-existent. High levels of integration "cause . . . this feeble individuation," one offshoot of which is the low valuation placed on the individual (Durkheim, 1951: 220-1; see also 227). Subtracting what is central for Durkheim—strong integration—and omitting any reference to low valuation of individuality as a reflection of the existing social reality, Parsons is left with value content which thereby comes to occupy the central place in his discussion. Thus, Parsons maintains that value content is crucial even though Durkheim makes clear that the value content on which Parsons focuses is caused by the very factor (high levels of integration) whose importance Parsons must deny.

Egoism is the second point at which Parsons attempts to discern value content. Contrasting altruistic and egoistic suicide, Durkheim (1951:221) says: "Whereas the latter [egoistic suicide] is due to excessive individuation, the former [altruistic suicide] is caused by too rudimentary individuation." Altruism refers to a condition of strict social control and rudimentary individuation, whereas egoism identifies a state of excessive individuation and weak social control.

Nonetheless, Parsons persists in attempting to discern value content as the decisive variable. One of the comparisons most important to both authors is that between Protestants and Catholics. Protestants, Durkheim claims, display the higher suicide rates. Since these two groups are also known to subscribe to somewhat different religious values, the question of their relevance naturally arises. The difference between them "lies in the different *content* of the different value systems" (Parsons, 1949:333). Durkheim's explanation (1951:157-8), however, is quite different. "The only essential difference between Catholicism and Protestantism is that the second permits free inquiry to a far greater degree than the first." But "free inquiry itself is only the effect of another cause," namely, "the overthrow of traditional beliefs." The overthrow of traditional be-

liefs denotes a weakening of the collective conscience and free inquiry develops to fill the void thereby created (Durkheim, 1951: 158-9). In contrast to Parsons, who holds that the spirit of free inquiry represents a shared sentiment, an element of the collective conscience, Durkheim specifically says it arises as a consequence of the breakdown of traditional beliefs, i.e., a weakening of the collective conscience (see also Johnson, 1965: 885; for later statements by Parsons see 1960:147 and 1968:316). Parsons' reinterpretation may be contrasted with Durkheim's (1951:159) own "conclusion that the superiority of Protestantism with respect to suicide results from its being a less strongly integrated church than the Catholic church." Parsons (1949:332) acknowledges that "in one sense the difference" between Protestants and Catholics "consists in the fact that the Catholic is subjected to a group authority from which the Protestant is exempt." Had he chosen to emphasize this theme in his interpretation, there would be no grounds for disagreement.

Durkheim could scarcely have been expected to identify, only to deny, the importance of all beliefs or attitudes which various commentators might see as constituting a decisive difference between Protestant and Catholic value systems. He does, however, recognize the existence of such differences; and insofar as he explicitly considers them, he denies that they are causes of differential suicide rates. Whereas Parsons focuses on attitudes concerning religious freedom and individual responsibility, Durkheim implies that the attitude which, a priori, would appear to have the greatest impact is that concerning suicide itself. Denying the importance of differential attitudes, he notes (1951:157) that Protestantism and Catholicism "prohibit suicide with equal emphasis." Consequently, "if Protestantism is less unfavorable to the development of suicide, it is not because of a different attitude from that of Catholicism." Clinching the argument, he observes (1951: 170) that "the religion with least inclination to suicide, Judaism, is the very one not formally proscribing it . . ." Insofar as *Suicide* explicitly considers them, different values and attitudes are ruled out as marking an important difference between Protes-

tants and Catholics or between either of them and Jews.

Durkheim's analysis does not end with religious society. Rather, to validate his hypothesis that integration and suicide vary inversely, he tries (1951:152-216) to demonstrate that it applies equally to religious, political, and domestic society. He then (1951:208-9) concludes that the moderating influence of these various types of societies cannot be due "to special characteristics of each" but must be due to "a characteristic common to all The only quality satisfying this condition is that they are all strongly integrated social groups." By contrast, Parsons focuses on a single (Protestant-Catholic) comparison, a procedure which ignores the rationale underlying Durkheim's approach and leaves his hypothesis potentially sustained, *not* by the cumulative impact of successive comparisons in religious, domestic, and political society but by a *single* comparison in one type of society. Parsons thereby shears Durkheim's argument of most of its empirical base⁸ and most of its force.

Furthermore, even were Parsons' account of Protestant-Catholic differences acceptable, his general conclusion that value content is the key factor in egoism could not be accepted because he would still have to show that this same factor also explains variations in suicide rates in domestic and political society (not to mention additional religious society comparisons involving Jews). His thesis concerning value content notwithstanding, Parsons makes no attempt to demonstrate its importance in Durkheim's analysis of domestic society. To the contrary, he (1949:331) acknowledges that social control is the central consideration at this point: "Egoism seems to exist as a factor in suicide so far as people are freed from . . . group control . . ." Devoting but one paragraph to domestic society, he ignores political society completely.

The Structure (333) also refers to "the more general phenomenon of which the Prot-

estant version of religious freedom and responsibility is a special case," namely, "the view that the leading common moral sentiment of our society is an ethical valuation of individual personality as such." Parsons includes only a single reference, citing Chapter I of Book III in *Le Suicide*. Since the chapter cited does not treat the ideas referred to, there is no way of ascertaining with certainty which section he has in mind.

The most interesting issue suggested by his discussion concerns a theoretical paradox posed by what in *Division* and *Suicide* is variously referred to as the cult or religion: of personal or individual dignity, individual or human personality, the individual, personality, man or humanity (Durkheim, 1960a:172, 400, 407; 1951: 240, 334, 336, 363-4; see also Durkheim, 1969). Durkheim's general position is that the strength of the collective conscience and individuality vary inversely. But what happens when the collective conscience stresses the value, dignity, and importance of man generally and the personality and individuality of each man? The stronger such shared sentiments, the stronger the collective conscience but also, presumably, the greater the development of individuality. *Suicide* struggles with this problem as Durkheim's somewhat inconsistent statements suggest. In particular, he seems to be having a hard time deciding whether the cult of man leads to egoistic suicide through its stress on individualism. On the one hand he (1951:336) denies that the cult is associated with egoistic suicide. Here Durkheim's premise that shared sentiments constitute a fundamental bond of solidarity asserts itself. Elsewhere in *Suicide* (363-4) Durkheim holds that the cult of man encourages individualism and, ultimately, egoistic suicide. Although the denial that this shared sentiment represents a bond of social solidarity implicitly acknowledges the importance of value content, it must be recognized that this denial is only a brief reference occurring late in the book. Further, value content is never directly used to explain given variations in social suicide rates. This reference cannot be read back into the systematic contrasts that Durkheim draws between more and less integrated groups as he analyzes the consequences of varying levels of integration in religious, political, and

⁸ Since Parsons evidences little systematic concern with the adequacy of the evidence offered by Durkheim to support his theory, this shearing of its empirical base presumably comes easily enough. In contrast, Durkheim was vitally concerned with empirically validating his theory.

domestic society. His inconsistent statements concerning the cult of man are an insufficient foundation on which to reinterpret integration as basically a question of value content (see also Johnson, 1965:885-6).

Since Parsons (1949:333-4) implies that perhaps Durkheim's views had undergone some change from those expressed earlier in his book, it may be noted that toward the very end of *Suicide* Durkheim restates his position concerning the relationship between the integration of religious society and suicide. Here he once again discounts the importance of the specific content of religious beliefs, while identifying "the powerful and scrupulous discipline to which" religion subjects thought and conduct as its source of immunity against suicide (Durkheim, 1951:376).

Commenting further on the change from *Division* to *Suicide*, Parsons (1949:336) observes that "instead of the *conscience collective* being contrasted with organic solidarity, there now are two types of influence of the *conscience collective*, and set over against *both* of them the state where its disciplining influence is weak . . ." Durkheim does attempt to distinguish between integration and regulation and hence between egoism, (the state of weak integration), and anomie (the state of weak regulation). Parsons fails, however, to appreciate the difficulty he encounters in so doing. Certainly the difference does not lie in value content, and it is instructive that the difference Parsons attempts to read into Durkheim has nothing to do with any of the distinctions which Durkheim himself attempts to draw, most of which fail to identify any strictly sociological distinction (Durkheim, 1951:241, 258, 283-7, 382; see also Johnson, 1965:884). Parsons (1949:336-7) argues that anomie is explicitly set "over against egoism" so that the "freedom from collective control . . . in the cases of egoism and of *anomie* are on quite different levels."

Suicide, however, reflects Durkheim's awareness of their basic similarity at the sociological (as opposed to psychological) level of analysis. *Suicide* (1951:258) asserts that both egoism and anomie "spring from society's insufficient presence in individuals."⁴ Noting their "peculiar affinity"

Durkheim (1951:288) observes that they "are usually merely two different aspects of one social state." Again, he (1951:382) refers to the "identical cause" producing them. Mention is also made of the "hypercivilization which breeds the anomic tendency and the egoistic tendency . . ." (Durkheim, 1951:323). Following through, Durkheim (1951:382) indicates that each requires the same remedy, namely the establishment of occupational groups. *Suicide* (288) explicitly asserts that the egoist is likely to be anomic and vice versa (but see also 258). Far from seeing a sharp distinction between them, Durkheim is aware of their basic similarity.

Value content cannot be used to distinguish regulation and integration. Indeed, the reading of value content into Durkheim rests largely on the attempt to find it at one or two specific points in but one work (but see also Parsons, 1949:333). For Durkheim the strength of social control remains basic whether that be identified in terms of the strength of the collective conscience or solidarity as in *Division*, or in terms of integration and regulation, as in *Suicide*.

Internalization and Freedom of Choice (Stage three)

During the third stage, Durkheim, according to Parsons (1949:382), takes the decisive step away from positivism toward voluntarism. This occurs when Durkheim discovers the internalization of values. Recognizing that he did not use the term internalization, Parsons holds that the concept, absent from early Durkheim, becomes central later (Parsons, 1949:378-90; see also 1956:9-10; 1960a:105-6). In *Suicide* (212, 258, 287, however, was present in Durkheim from the very beginning.

As early as 1887, well before *Division*, Durkheim observes that moral "constraint does not consist in an exterior and mechanical pressure; it has a more intimate and psychological character" (quoted in Wallwork, 1972:38; see also 40-1n). Durkheim continues to use the concept in *Division* (1960a: 73-82, 97-101, 196). The social factor is defined in terms of what is common to all

egoistic and anomic suicide. Since, however, the types of suicide are named after the social conditions causing them, the statement also characterizes egoism and anomie.

⁴Durkheim's immediate reference here is to

individuals, and this factor largely exhausts personality in the mechanical setting (Durkheim, 1960a:129-30). Durkheim represents the collective conscience as an entity which, in becoming an integral part of the individual's conscience and personality, operates as a social force controlling him. Indeed, society is possible only on the basis of this penetration, which represents an all important mechanism of social control (Durkheim, 1960a:105-6). In *Suicide* (212, 258, 287, 320, 335) Durkheim continues to use the concept as he does in *The Elementary Forms*. Interestingly enough, in the latter work Durkheim (1961a:256n) refers the reader back to *Division* for further amplification of the idea.

Beyond the question of its alleged introduction into Durkheim's theory at some point subsequent to *Division* and *Suicide*, Parsons misrepresents the meaning of internalization by associating it with freedom of choice. Durkheim does quite the opposite by linking it with social control. This link follows from his conception of the opposition between society and the unsocialized individual. In *The Elementary Forms* Durkheim (237) notes that given ends and interests contrary to those of the individual, society "requires that, forgetful of our own interest, we make ourselves its servitors, and it submits us to every sort of inconvenience, privation and sacrifice, without which social life would be impossible." Consequently, individuals find themselves subjected to "rules of conduct and of thought which we have neither made nor desired, and which are sometimes even contrary to our most fundamental inclinations and instincts" (see also 298). This conception of an individual-social opposition underlies Durkheim's basic explanatory structure and may be found in *Suicide* (319), *Division* (130) and elsewhere in his work (e.g., 1960b).

Conceiving of the individual and the social as opposed forces, Durkheim feels that the greater the internalization of the social component the greater the control it exercises over the individual and the less his freedom of choice. Perhaps the most vivid illustration of this is to be found in his account of the mechanical (*Division*), altruistic (*Suicide*) or primitive (*The Elementary Forms*) setting where personality is composed almost

wholly of the internalized social factor, and "the collective conscience chains us to our group and shackles the liberty of our movements" (Durkheim, 1960a:304). In modern society personality is less completely taken up by the internalized component, social control is weaker, and the individual is freer from social control.

Does it follow, then, that internalization varies inversely with freedom for the individual? Durkheim's answer is most elaborately spelled out in *Suicide*. In the altruistic settings of primitive society and the modern military, men are not free, being subject to high levels of social control. Outside the military context many groups and social conditions in modern society are relatively egoistic or anomic. Failing to fulfill the socially generated need to find meaning in life, the egoist is driven toward suicide (Durkheim, 1951:208-16). In a state of anomie (Durkheim, 1951:241-76) men's goals, desires, passions, or appetites structure infinitely expandable needs. Consequently, unless restrained they outstrip the means available to attain them. As social products these needs can be restrained only by the moral power of the group; to the extent that this restraint is lacking, a means-needs disjunction arises and creates the unhappiness that brings man to suicide. Thus, Durkheim holds that the more the individual is subject to the moral control of the group, the more scaled down are his needs, the more needs and means exist in a state of equilibrium, the happier man is and the less likely he is to commit suicide; hence regulation and suicide vary inversely. As man is freed from social restraint he experiences an unfulfilled need to find meaning in life or a means-needs disequilibrium resulting from the insatiable nature of his own needs, passions, and desires. Completely divorced from society he is subject to the "blind and amoral forces of nature" (Durkheim, 1953: 55). Either way, the individual does not exist in the realm of freedom.

Morals and Freedom of Choice (Stage three)

The internalized element is, of course, the moral or social factor (the common or collective conscience of *Division*, the collective sentiments of *Suicide*, or the collective representations of *The Elementary Forms*). Par-

sons' coupling of internalization with freedom of choice is based on a misunderstanding of the meaning of "moral" in Durkheim's theory. According to Parsons (1949:382-8), Durkheim eventually came to see that individuals respect internalized social norms. This respect (Parsons, 1949:383-4) is important in eliciting compliance from individuals. "The essence of constraint is the moral obligation to obey a rule—the voluntary adherence to it as a duty. . . . Some even would say that it is not constraint at all, since it involves voluntary adherence to a rule, which is precisely the opposite of constraint." Parsons (1949:384) concludes by noting that adherence to moral norms has both voluntary and binding elements, the latter resulting from moral obligation. For Parsons, then, Durkheim postulates an important, irreducible element of voluntary compliance with moral rules.

Durkheim's theoretical works, however, reflect a different view. *The Elementary Forms* (298) observes that as the respect inspired by society "is naturally extended to all that comes from it, its imperative rules of conduct are invested, by reason of their origin," with authority and dignity. Thus society is able to force moral ideas on the individual. Durkheim goes on to characterize society as a "voice which makes itself heard only to give us orders and establish prohibitions." In similar vein (1961a:237-8) he observes that "an object . . . inspires respect when the representation expressing it in the mind is gifted with such a force that it automatically causes or inhibits actions, . . ." Respect, then, "is the emotion which we experience when we feel this . . . pressure operating upon us."

Similar views are expressed throughout Durkheim's works. *Division* (425-7) defines moral rules as sanctioned rules of conduct. *Suicide* (335) observes that by "the resistance offered by our egoism to" the renunciations occasioned by the requirement to act in conformity with moral rules, "we readily see that they are forced from us by a power to which we have submitted" (see also Durkheim, 1938:2). What distinguishes moral rules is their obligatory character (Durkheim, 1960a:425). Taking constraint as one defining characteristic, *The Rules* (3) characterizes social (moral) facts as those "ways

of acting, thinking, and feeling, external to the individual, and endowed with a power of coercion, by reason of which they control him."

"Moral" is commonly understood to have the meaning Parsons interprets Durkheim as attributing to it. Thus moral behavior is often understood as guided in part by voluntary compliance with ethical standards. Unless the individual remains free to choose, his behavior cannot be meaningfully judged in terms of its morality. But for Durkheim moral behavior is behavior which is *determined* by moral rules. It consists solely in the commandment and in nothing else so that to the "extent that *any* other element enters into conduct, to that extent it *loses* its moral character" (Durkheim, 1961b:30-1; emphasis added, see also 1960a:197). Moral rules acquire their power and authority from their commonality; embodying the force of the social factor, they compel compliance. Society is both source and object of morality (Durkheim, 1961b:86; 1953:59). Moral regulations express "needs that society alone can feel" (Durkheim, 1960a:5) so that "to act morally is to act in terms of the collective interest" (Durkheim, 1961b:59). Given the individual and society as opposed forces, the greater the morality, the less the control exercised by the individual over his own behavior. It is when he is freed from social control that he acts in an immoral or, at best, amoral fashion. Thus anomie is the contradiction of all morality (Durkheim, 1960a:431n). What Durkheim says of weak regulation applies equally to states of weak integration or solidarity. The absence of social control is the very contradiction of morality; strong social control, its source. In short, Durkheim does not link morality with freedom of choice; indeed, freedom from social control is basically subversive of morality.

Durkheim presents additional views which make the preceding appear one-sided. *Moral Education* (17-126) identifies duty or discipline, group attachment, individual autonomy or self-determination, the good or desirable, and understanding as elements of morality; while "The Determination of Moral Facts" (Durkheim, 1953:35-62) identifies the elements of duty and desirability as its basic elements. Synthesizing these two statements, an attempt may be

made to state Durkheim's position as follows: Man in society has a basic "need to be constrained, bounded, [and] restricted" (Durkheim, 1961b:113) by society. Furthermore, only through society can the individual realize his own potential as a human being (Durkheim, 1953:55). Society far surpasses the individual and represents the end of his moral behavior (Durkheim, 1953:44-5, 59; 1961b:86). Understanding leads us to see the reasons for societal commands, why they are useful, inevitable, and necessary, and how they enable us to realize the best in ourselves (Durkheim, 1961b:116-20). This coupled with the inherently good and superior nature of society makes it desirable to the individual (Durkheim, 1953:54-6; see also 1961a:243, 243n; 1938:liv_n). Understanding of the basically beneficial and necessary nature of the social commands embodied in moral rules leads to voluntary compliance which therefore remains compatible with the autonomy and self-determination of the individual (Durkheim, 1961b:116-20).

Initial appearances to the contrary, these views do not require us to reject the prior argument that Durkheim's social theory does not link morality with an element of freedom of choice. The great French master was both social theorist and social philosopher. He valued rationality and freedom. As a social philosopher he worked hard to reconcile the view of morality as duty, discipline, obligation, constraint, coercion, and commandment with that of morality as understanding, self-determination, autonomy, freedom, and desirability. How successfully he did so is open to question. Even in his more social philosophical writings (Durkheim, 1961b:17-126; 1953:35-62), it might be said that if the individual chooses, he does not choose freely. But whatever the case in these works, the more important point is that in his statement of sociological method (*The Rules*) and in his major works on the theory of social integration (*Division*, *Suicide* and *The Elementary Forms*), there is no doubt the predominant view excludes any irreducible element of freedom of choice.

Durkheim himself addresses the free will-determinism issue. In *The Rules* (141) he says that sociology "needs to embrace free will no more than determinism." A long footnote in *Suicide* (325n) characterizes the issue

itself as metaphysical. But if the hypothesis of free will is metaphysical, presumably Durkheim would not want to incorporate it into his sociology. He goes on to say that his approach simply adds social forces to physical, chemical, biological and psychological forces. There is nothing voluntaristic about his conception of these forces which "act upon men from without." As for desirability as an element of morality, Durkheim's formulation in *Professional Ethics and Civil Morals* (61) seems to remain true both to his theory of social integration and to his attempt to reconcile desirability and duty as elements of morality: Every society is despotic. This despotism is both natural and necessary. Yet it is no way intolerable, and individuals do not experience its weight any more than they feel the weight of the atmosphere on their shoulders. Having been raised by the collectivity, the individual "will naturally desire what it desires and accept without difficulty the state of subjection to which he finds himself reduced" (see also Durkheim, 1938:6).

The Action Frame of Reference

Parsons' basic thesis is that Durkheim gradually came to, or came close to, embracing the theory of action. He does express some reservations, however. For example, he notes (1949:710) that "little of Durkheim's attention was centered on the intrinsic means-end schema as such . . ." Durkheim makes some use of a means-ends schema in *Suicide*, particularly in the accounts of anomic (241-76) and, secondarily, egoistic (208-16) suicide. Nevertheless, he generally tried (1951:43) to avoid using it because "an act cannot be defined by the end sought by the actor, for an identical system of behavior may be adjustable to too many different ends without altering its nature." Subsequent to *Suicide* Durkheim tended to remain true to this rejection of a means-ends schema. In this sense the general direction of his theoretical development after *Suicide* was not toward but rather away from the theory of action. Regardless of whether (1) Durkheim largely rejected a means-ends schema or (2) as Parsons (1949:710-11) argues, it remained latent in his work, on either ground we can question whether Durkheim converged on the theory of action.

A more general question concerns the status of subjective variables in Durkheim's theory. In *Suicide* Durkheim considers the possibility of framing an explanation in terms of the subjective states of individual actors. He (1951:43) refuses to define suicide as intentional self-homicide on the grounds that "intent is too intimate a thing to be more than approximately interpreted by another. It even escapes self-observation" as individuals delude themselves into thinking that acts "due to petty feelings or blind routine" are inspired by "generous passions or lofty considerations." Later (1951:148-9) he observes that "human volition is the most complex of all phenomena" adding that it would be difficult to ascertain the various motives underlying individual instances of suicide. Furthermore, such motives do not constitute the true causes of suicide. Durkheim describes at length the subjective states associated with various types of suicide, noting that such states may even appear to be the cause of suicide. In reality, however, they are merely the prolongations of social causes inside the individual (Durkheim, 1951:287, see also 300).

Here as elsewhere Durkheim, ever the social realist, rejects explanations of human behavior which employ subjective variables as independent variables. In what might stand as one hallmark of his approach, *The Rules* (95) asserts that all questions of human "intention are too subjective to allow of scientific treatment." It is in keeping with this assessment that when, as in *Suicide* or *The Elementary Forms*, Durkheim does consider the subjective states of individual actors, he always treats these states as the result of objective, exterior (to the nonsocial component of personality), social factors and causes and never as themselves truly causal variables. Parsons' theory of action, in contrast, is predicated on the necessity of treating the subjective states of individuals actors as important variables in their own right. Far from embracing the radically subjective theory of action, then, Durkheim emphatically rejects all subjective explanations of social man.

FROM POSITIVISM TO IDEALISM

The Structure (405) maintains that the outcome of the third stage of "Durkheim's

development has been the emergence in outline" of the theory of action. But in the fourth stage (*The Elementary Forms*) he goes beyond this to embrace idealism (Parsons, 1949:445). Somewhat ironically in light of his general convergence thesis, Parsons' comparison of the third and fourth stages in Durkheim's thought reveals movements *away from* rather than toward the voluntaristic theory of action.

Clearly Durkheim is not an idealist as that term is normally understood (Parsons, 1949:473-87) nor did he think of himself as such. Instead, Parsons' characterization rests heavily on his analysis of Durkheim's conception of society. For Durkheim "society has become the thing the idealist philosophers are talking about. It consists as he [Durkheim] says 'exclusively of ideas and sentiments' and . . . of *the Idea* . . ." (Parsons, 1949:444). Parsons (1949:442n, 444) places considerable stock in this passage (he quotes it twice in the space of three pages). In fact, however, Durkheim's referent is not to society but rather to collective forces (Durkheim, 1960c:521; Durkheim, 1961a:408). Certainly Durkheim often speaks of society as a sui generis reality consisting of the collective representations resulting from the fusion of individual representations. Overall, however, his portrayal of society is not so monolithically psychic as Parsons suggests. In concluding *The Elementary Forms* Durkheim (1961a:470) includes individuals, the ground they occupy, the things they use, and the movements they perform as parts of society. The penultimate paragraph of the book (Durkheim, 1961a:495) refers to the "richness of different materials" constituting society and identifies physical forces as one of its constituent elements.

Consistent with his emphasis on the radical change in Durkheim's thought, Parsons (1949:307) holds that "idealistic strains appear in Durkheim's thought, but only at the latest stage of its development." If stress on the ultimately psychic nature of social phenomena generally and society in particular is one criterion, it must be observed that idealistic strains can be found in Durkheim (1960a:109, 129) from the beginning. For instance, *Division* (80) refers to mechanical society as "the psychical type of society." Even in reference to more differentiated

organic society Durkheim (1960a:80) says, "Judicial, governmental, scientific, industrial, in short, all special functions are of a psychic nature, since they consist in systems of representations and actions." A similar emphasis on the psychic nature of social facts and social life may be found in *The Rules* (see Parsons, 1949:356) and *Suicide* (312). And in *Moral Education* (277; see also 59, 69), a work from the (hypothesized) third stage of development when Durkheim supposedly came closest to the voluntaristic theory of action, Durkheim characterizes society in terms which are just as "idealistic" as anything to be found in *The Elementary Forms*: "Society . . . is a complex of ideas and sentiments, of ways of seeing and of feeling, a certain intellectual and moral framework distinctive of the entire group. Society is above all a consciousness of the whole" (see also Durkheim, 1953:96). In short, contrary to Parsons' hypothesis of change, Durkheim's conception of the social factor generally and society in particular reflected a powerful idealistic component from the beginning (e.g., Durkheim, 1965:2).

Parsons (1949:448-9) links Durkheim's idealism to the absence of a theory of social change and then explains this lacuna. "It became increasingly evident that Durkheim was thinking of the social element as a system of eternal objects. Now the very essence of such objects is timelessness. Hence the concept of process, of change, is meaningless as applied to them in themselves." This passage overlooks Durkheim's concern with the interplay between the collective substratum (or morphological base) and collective representations. The latter do change, and those of the Australian aborigines analyzed in *The Elementary Forms* are not those of modern society. Far from portraying these representations as eternal objects, Durkheim (1961a: 386-92) says that they have whatever place individuals give them in their own minds disappearing altogether when the members of society no longer think of them. Durkheim is not thinking of eternal, timeless objects or forms either in the Platonic mode, as Parsons (1949:444) implies, or otherwise (Durkheim, 1960b:331-4).

A trend in Durkheim's thought can be discerned. In *Division* he (256-62, 336-41) lends the morphological base causal priority

vis-à-vis the development of the division of labor, ideals and psychic life, and the progress of civilization generally. *Suicide* (226-7, 387) says that the anatomical constitution of society determines its collective ideas. By the time he wrote "Individual and Collective Representations" (1898), Durkheim (1953:30-1) was perceiving collective representations as partly dependent and partly independent of the collective substratum. *The Elementary Forms* expresses a similar view, though there is a clear shift in the direction of emphasizing the relative autonomy (not to mention importance) of collective representations. Durkheim (1961a: 471) notes that, once born, collective representations obey laws all their own. With respect to Durkheim's thought, there is some movement away from materialism toward idealism. Equally important, Durkheim stresses the interplay between material foundation and collective consciousness. This stress parallels that in *The Elementary Forms* itself on the interplay between collective religious beliefs and the rites. In sum, Parsons' attempt to portray Durkheim as having shifted from positivism to idealism⁵ is no more convincing than the earlier at-

⁵ Parsons' own analysis (1949:446) seems to entail some inconsistency. The problem with idealism is that in its overemphasis on values and ideas it "eliminates the reality of the obstacles to the realization of values Hence the central feature of the category of *action*, its voluntaristic character, the elements of will, of effort, have no place in such a scheme." This may be contrasted with his earlier assertions (Parsons, 1949:437; see also 412). Durkheim tends "to think of ritual as the primary element of religion and religious ideas as secondary rationalizations, explanations, justifications of ritual." Further, "Durkheim's view of the functions of ritual implies the necessity of . . . will or effort. So far from being automatic, the realization of ultimate values is a matter of active energy, of will, of effort . . ." (Parsons, 1949:440). These conflicting interpretations are consistent with Parsons' position that, in the fourth stage, voluntarism and idealism were struggling for supremacy. It does, however, call Parsons' attempt to portray Durkheim as an idealist into question.

Parson (1949:444) also asserts that, even in his fourth stage, "Durkheim will not let go of his positivism." Thus, according to Parsons, he is simultaneously in some measure positivist, voluntarist, and idealist. However, the analysis in this paper shows that Durkheim is, in fact, none of these; Parsons' image of Durkheim as a morass of conflicting tendencies is more artificial than real.

PARSONS' INTERPRETATION OF DURKHEIM

tempt to document his journey from positivism to voluntarism.

THE FUNCTIONALIST INTERPRETATION OF DURKHEIM

Durkheim is often considered the most important progenitor of the contemporary functionalist approach in anthropology and sociology, and Parsons himself is widely acknowledged to be the most influential functionalist theorist of our time. It might be anticipated, then, that *The Structure* would have devoted considerable attention to those very functionalist strands with which Parsons is now identified until, of course, it is recalled that the author of *The Structure* was not at that time a functionalist. Indeed, the reader of Parsons' book would have no reason to suspect that functionalism pervades Durkheim's thought.⁸ More recently, of course, many sociologists and anthropologists have taken note of Durkheim's functionalism and its influence on the development of this perspective within both these disciplines.

In addition to ignoring this vital aspect of Durkheim, *The Structure* minimizes Durkheim's concern with social change. Asserting that he has no clear-cut theory of social change, Parsons (1949:448) holds that in all his later thought (with one exception), the problem of social change "is altogether outside his field of interest."

During the fifties and sixties functionalism came under heavy attack for its alleged static implications. No doubt partly as a response, functionalists increasingly turned their attention to social change (Demerath and Peterson, 1967). One apparent outgrowth of this was a re-interpretation of Durkheim. In 1959 Bellah published "Durkheim and History," in which he quotes Durkheim's assertion in *The Rules* that "*one cannot explain a social fact of any complexity except by following its complete development through all social species*" (Bellah, 1959: 452; Durkheim's emphasis; see also 450; see also Durkheim, 1960b:325, one of Durk-

heim's last essays written during that period when, according to Parsons, he should, if anything, have been least concerned with change). In addition, Durkheim practiced what he preached, so most of his own researches are "organized in an historical framework. This is true, for example, of his sociology of the family, his treatment of the division of labor, his theory of punishment, his discussion of property and contract, his sociology of education, his sociology of religion, of his study of socialism" (Bellah, 1959:448-9). Finally, Durkheim's concern with change bore theoretical fruit. He "went quite far in the direction of developing a theory of social change," a theory which is "organized around the master idea of structural differentiation" (Bellah, 1959:452, 455). But "Parsons and Smelser have . . . pushed the theory of structural differentiation considerably forward" (Bellah, 1959: 452n). It appears that Parsons and Smelser have pushed forward a theory of social change in Durkheim which Parsons denied Durkheim ever had.

The famous opening paragraph of *The Structure* (3) begins with a quote from Crane Brinton: "Who now reads Spencer? . . . He was the intimate confidant of a strange and rather unsatisfactory God, whom he called the principle of Evolution. His God has betrayed him. We have evolved beyond Spencer." Parsons comments: "We must agree with the verdict. Spencer is dead." Given Parsons' embrace of evolutionism in the 1960's (Parsons, 1964; 1966), it is understandable that this section of *The Structure* should often be cited to document the change in his own thought. His most recent discussion of Durkheim may be considered in the context of Bellah's documenting of Durkheim's concern with change, Parsons' own heightened concern with change, and his evolutionism. He (1968:318) notes Durkheim's "exceedingly valuable conception . . . of processes of structural differentiation." Further, "Durkheim provided the groundwork for a major theory of developmental change in societies and . . . made important direct contributions to it himself" (Parsons, 1968:318). Parsons' article also makes repeated and favorable reference to Durkheim's evolutionism. It expresses one regret: "The contributions he did make to an under-

⁸ Compare Lopreato's (1964:639-46) analysis of Pareto as a functionalist. He observes on the one hand (1964:639) that Pareto's sociology "reveals a highly developed functionalism" and on the other (1964:639-40) that though Parsons is himself an illustrious functionalist, he "sees no functional approach at all in Pareto's sociology."

standing of the process of change seem not to have been understood as fully as his more 'static' analysis" (Parsons, 1968:318). Parsons does not undertake to assess the role of *The Structure* in this development.⁷ Bellah observes that "Durkheim's insistence on taking the total society as his theoretical frame of reference was one of his major contributions, as Talcott Parsons has . . . pointed out . . ." (Bellah, 1959:451n; see also Parsons, 1960). This observation is particularly ironic because it is Parsons' inattention to Durkheim's concept of society as a *sui generis* reality that underlies *The Structure's* tendency to all but ignore Durkheim's functionalism, his concern with change, and his evolutionism. To paraphrase a commonplace, Durkheim has remained the same; only the interpretation has changed.

DURKHEIM'S THEORY IN LIGHT OF HIS INTENTIONS AND ASSUMPTIONS

It could be held that any adequate critique of Parsons' interpretation should, in addition to identifying points of disagreement, suggest an alternative interpretation. Unfortunately, only the bare foundations of such an interpretation can be suggested here.

Durkheim's theory is best understood in relation to his intentions and underlying assumptions. His primary objective was to establish sociology as a legitimate science on a footing equal to that enjoyed by such recognized disciplines as physics or biology. Science itself is the study of reality. All reality is a system of forces. In order to validate its claim to legitimacy, any scientific discipline must identify its own distinctive reality to study. Reality itself is proportional to and hence measurable by the power and irreducibility of the system of forces embodied in it. Forces are measured by their

effects. The greater the effects, the more powerful the forces and the greater their reality. Second, forces are real to the extent that specific effects can be uniquely attributed to them, i.e., that the forces in question have their own and not a derived, subordinate, or borrowed power. These perspectives underlie three pervasive motifs in Durkheim.

All social facts are forces, and the forces embodied in social phenomena are just as powerful and real as those embodied in the phenomena studied by other scientific disciplines. "Actually, our conception merely adds to physical, chemical, biological and psychological forces, social forces which like these act upon men from without" (Durkheim, 1951:325n). These social forces "may be measured, their relative sizes compared, as is done with the intensity of electric currents or luminous foci" (Durkheim, 1951:310). And "though purely ideal . . . they determine the conduct of men with the same degree of necessity as physical forces" (Durkheim, 1961a:260). Durkheim emphasizes that he does not hesitate to compare social or moral forces with physical forces. At one point he suggests that perhaps they are even more powerful: "A society is the most powerful combination of physical and moral forces of which nature offers us an example" (Durkheim, 1961a:495; see also 1953:97). As explicitly noted in the concluding paragraph of the last of his books published during his lifetime, all his work is based on the assumption that "when it is recognized that above the individual there is society, and that this is not a nominal being created by reason, but a system of active forces, *a new manner of explaining men becomes possible*" (Durkheim, 1961a:495; emphasis added). Sociology is born of the recognition of the existence of these forces which determine the conduct of men in society.

Second, it is not enough that society represents a powerful system of forces. They must possess their own reality. Durkheim emphasizes the necessity of explaining social facts in terms of other social facts. "*The determining cause of a social fact should be sought among the social facts preceding it . . .*" (Durkheim, 1938:110). He justifies his stance by appealing to the doctrine of emergence which finds two somewhat different ap-

⁷ In both his most recent treatment of Durkheim (Parsons, 1968) and the account of his own intellectual development (Parsons, 1970), Parsons makes favorable mention of Durkheim's evolutionism. He refrains, however, from acknowledging Durkheim's influence on his own evolutionist perspectives, mentioning Weber in this regard (Parsons, 1970:851-3, 874). This seems curious since (1) Durkheim is more nearly a prototypical evolutionist than Weber, and (2) Parsons' own perspectives on the structural differentiation of social systems are much more congruent with Durkheim's evolutionism than with Weber's.

plications in his work. On the one hand, the interaction or combination of phenomena at one order of nature gives rise to emergent phenomena at the next higher order. On the other, the interaction or interconnectedness of parts gives rise to wholes with emergent properties. Social phenomena exhibit emergent characteristics vis-à-vis the phenomena at lower orders of nature from which they arise; society is an emergent phenomenon relative to its parts. Here, then, is Durkheim's ever present stress on the sui generis nature of social reality generally and society in particular.

It is in this context that some of his more extreme statements concerning one aspect of the relationship between individuals and society can be understood. In the chapter in *Suicide* on "How to Determine Social Causes and Social Types," he proposes to proceed by "disregarding the individual as such, his motives and his ideas" in order to "seek directly the states of the various social environments (religious confessions, family, political society, occupational groups, etc.), in terms of which the variations of suicide occur" (Durkheim, 1951:151). *The Rules* (102) observes that "when the individual has been eliminated, society alone remains. We must, then, seek the explanation of social life in the nature of society itself." Without denying that Durkheim struggled to provide an interpretation of his assertion that society exists independently of those who compose it, the thrust of his attempt to treat society as a reality distinct from individuals is unmistakable.

In postulating the existence of emergent phenomena Durkheim is not thinking only in terms of analytic realities. The scientist does not create different realities by thinking of the same phenomena in different ways. Rather, the realities come first and our ideas and concepts are simply more or less accurate reflections of the reality to which they correspond and which they ultimately represent. Durkheim is not a nominalist but rather a realist.

Finally, these views are intimately linked to the second key component of his understanding of the nature of the relationship between the individual and society. To the degree that individual and social forces are similar and tend in the same direction it be-

comes difficult, if not impossible, to attribute given effects to social as opposed to individual forces. But to the degree that such a situation prevails, individual explanations of social phenomena become possible, thereby undermining the legitimacy of sociology's claim to recognition as an independent scientific discipline. If, on the other hand, individual and social forces are different, nay, if they are opposed, antagonistic forces, then it becomes clear whether given effects are due to individual or social forces. Durkheim stresses the radical heterogeneity of the individual and the social throughout his works by everywhere contrasting the social, sacred, ideal, spiritual, soul, reason, moral, higher, superior, and complex with the individual, profane, material, organic, lower, body, appetites, inferior, utilitarian, and the simple. This radical heterogeneity reduces the possibility that individual and social forces might be confused. In this context his contention that the way in which social forces constrain men constitutes proof of their reality becomes most understandable.

CONCLUSION

Having focused primarily on four European theorists, Parsons' first conclusion (1949:719-20) is that, in their works, "there has appeared the outline of what *in all essentials*, is the *same* system of generalized social theory, the structural aspect of what has been called the voluntaristic theory of action." He (1949:722) recognizes that "it is, of course, conceivable that the convergence does not exist at all, but that its appearance in this study is the result of an accumulation of errors of interpretation by the present author." Even today, though, Parsons evidences no uncertainty about the validity of his demonstration (see Parsons, 1970:829, 831, 833). It is precisely the thesis of this paper, however, that Parsons' interpretation of Durkheim is based on just such an accumulation of errors; Parsons' convergence applies at best to the other three authors.⁸

⁸ Parsons' interpretation of Weber is ultimately no more satisfactory than that of Durkheim. Rather than being guided by values and norms as Parsons would have it, Weber (1958:280) held that "not ideas, but material and ideal interests, directly govern men's conduct." Further, the ability of individuals and groups to realize their interests is

Since *The Structure* interprets the Marshall-Pareto-Weber-Durkheim convergence as positive evidence for the voluntaristic theory of action, presumably Durkheim's failure to converge should be taken as negative evidence.

Durkheim never imputed to man in society a rational, calculating, scientific attitude; consequently he was not a positivist. He is not guilty of biological reductionism in *Division*, nor does *Suicide* introduce value content as an important independent variable. Here as elsewhere in Durkheim it is the strength of the social factor, not its content, which remains decisive. Durkheim employed the concept of internalization from the beginning. His work does not link internalization to recognition that sociological analysis must analyze phenomena as they appear to the actor. Nor is internalization of the moral component linked to any element of voluntarism or free will for the individual. Though not a positivist in Parsons' sense, Durkheim was a positivist in stressing the necessity of using only objective variables lending themselves to the kind of scientific analysis employed by the positive (natural) sciences. Durkheim never embraced a theory of action, voluntaristic or otherwise. Neither, in the final stages of his thought, was he an idealist. Never a positivist (in Parsons' sense), action theorist, voluntarist, or idealist, Durkheim never travelled the road so carefully laid out for him by Parsons; rather, he remained a social realist throughout.

REFERENCES

Alpert, Harry

- 1961 Emile Durkheim and His Sociology. New York: Russell & Russell.

largely a function of their position in the three social power hierarchies of class, status, and political power (Weber, 1968:926-39). Though noting, for instance, that Weber "had a deep, almost tragic, consciousness of the importance of coercion in human affairs," (Parsons, 1949:628) and that for both Marx and Weber "the core problems of the modern system lay in power relationships," (Parsons, 1970:855) by underplaying the importance of power and interests in Weber's work, Parsons generates a tone quite foreign to the original. (For an analysis of Weber, see Bendix, 1962. Comparisons of Weber and Durkheim emphasizing divergence as opposed to convergence may be found in Collins, 1968:42-67; and Bendix, 1971:282-98).

Bellah, Robert N.

- 1959 "Durkheim and history." American Sociological Review 24 (August): 447-61.

Bendix, Reinhard

- 1962 Max Weber: An Intellectual Portrait. Garden City, New York: Anchor Doubleday.
1971 "Two sociological traditions." Pp. 282-98 in Reinhard Bendix and Guenther Roth, Scholarship and Partisanship: Essays on Max Weber. Berkeley: University of California Press.

Benoit-Smullyan, Emile

- 1948 "The sociology of Emile Durkheim and his school." Pp. 499-537 in Harry E. Barnes (ed.), An Introduction to the History of Sociology. Chicago: The University of Chicago Press.

Collins, Randall

- 1968 "A comparative approach to political sociology." Pp. 42-67 in Reinhard Bendix (ed.), State and Society. Boston: Little, Brown and Company.

Demerath, N. J. and Richard A. Peterson (eds.)

- 1967 System, Change, and Conflict. New York: The Free Press.

Devereux, Edward C.

- 1961 "Parsons' sociological theory." Pp. 1-63 in Max Black (ed.), The Social Theories of Parsons. Englewood Cliffs, New Jersey: Prentice-Hall.

Durkheim, Emile

- 1888 "Suicide et natalité: Etude de statistique morale." Revue Philosophique 26 (November): 446-63.
1938 The Rules of Sociological Method. Tr. by Sarah A. Solovay and John H. Mueller. New York: The Free Press.
1951 Suicide. Tr. by John A. Spaulding and George Simpson. Glencoe, Illinois: The Free Press.
1953 Sociology and Philosophy. Tr. by D. F. Pocock. Glencoe, Illinois: The Free Press.
1958 Professional Ethics and Civic Morals. Tr. by Cornelia Brookfield. Glencoe, Illinois: The Free Press.
1960a The Division of Labor in Society. Tr. by George Simpson. Glencoe, Illinois: The Free Press.
1960b "The dualism of human nature and its social conditions." Pp. 325-40 in Emile Durkheim, et al., Essays on Sociology and Philosophy. New York: Harper Torchbooks.
1960c Les Formes Elementaires de la Vie Religieuse. Paris: Presses Universitaires de France.
1961a The Elementary Forms of the Religious Life. Tr. by Joseph Ward Swain. New York: Collier Books.
1961b Moral Education. Tr. by Everett K. Wilson and Herman Schnurer. New York: The Free Press of Glencoe.
1965 Montesquieu and Rousseau: Forerunners of Sociology. Tr. by Ralph Manheim. Ann Arbor, Michigan: The University of Michigan Press.

- 1969 "Individualism and the intellectuals." Pp. 19-30 in Steven Lukes, "Durkheim's 'individualism and the intellectuals.'" *Political Studies* 17(March):14-30.
- Foskett, John M.
1939 *Emile Durkheim and the Problem of Social Order*. Unpublished Ph.D. dissertation. Berkeley, California: University of California at Berkeley.
- Johnson, Barclay D.
1965 "Durkheim's one cause of suicide." *American Sociological Review* 30(December): 875-86.
- Lopreato, Joseph
1964 "A functionalist reappraisal of Pareto's sociology." *American Journal of Sociology* 69 (May):639-46.
- Merton, Robert K.
1965 "Durkheim's *Division Of Labor In Society*." Pp. 105-12 in Robert A. Nisbet (ed.), *Emile Durkheim*. Englewood Cliffs, New Jersey: Prentice-Hall.
- Parsons, Talcott
1935 "The place of ultimate values in sociological theory." *International Journal of Ethics* 45(April):282-316.
1949 *The Structure of Social Action*. Glencoe, Illinois: The Free Press.
1956 "Foreword." Pp. 7-10 in *Emile Durkheim, Education and Sociology*. Tr. by Sherwood D. Fox. New York: The Free Press.
1960 "Durkheim's contribution to the theory of integration of social systems." Pp. 118-53 in *Emile Durkheim et al., Essays on Sociology and Philosophy*. New York: Harper Torchbooks.
- 1964 "Evolutionary universals in society." *American Sociological Review* 29(June): 339-57.
- 1966 *Societies: Evolutionary and Comparative Perspectives*, Englewood Cliffs, New Jersey: Prentice-Hall.
- 1968 "Durkheim, Emile." *International Encyclopedia of the Social Sciences* 4:311-20.
- 1970 "On building social system theory: A personal history." *Daedalus* 99(Fall):826-81.
- Scott, John Finley
1963 "The changing foundations of the Parsonian action scheme." *American Sociological Review* 28(October):716-35.
- Shils, Edward
1961 "The calling of sociology." Pp. 1405-48 in Talcott Parsons, et al. (eds.), *Theories of Society*. Volume II. New York: The Free Press of Glencoe.
- Sorokin, Pitirim
1928 *Contemporary Sociological Theories*. New York: Harper and Brothers.
- Wallwork, Ernest
1972 *Durkheim: Morality and Milieu*. Cambridge, Massachusetts: Harvard University Press.
- Weber, Max
1958 *From Max Weber: Essays in Sociology*. Tr. by H. H. Gerth and C. Wright Mills. New York: Galaxy Books.
1968 *Economy and Society*. Volume II. New York: Bedminster Press.

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

AXIOMATIC THEORY, INFORMATIVE VALUE OF PROPOSITIONS, AND "DERIVATION RULES OF ORDINARY LANGUAGE"

SIAMAK MOVAHEDI

University of Massachusetts, Boston

RICHARD H. OGLES

University of Tulsa

American Sociological Review 1973, Vol. 38 (August):416-424

We present a logical analysis of Hans Zetterberg's strategy of "axiomatic theory" construction, especially his derivation of "ordinary propositions" from "theoretical propositions." Using formal criteria for assessing the relative logical or empirical content of propositions, we show that not only is Zetterberg's distinction vague but his derivation rules are logically faulty, and his assessment of the relative informative value of propositions is inconsistent. Thus we argue that Zetterberg failed to produce a coherent strategy for theory construction. Finally, we suggest that for the critical analysis of strategies of theory construction in sociology in general, the logical apparatus of modern symbolic logic and nondeductive inference be used.

A PROFUSION of works in sociology address problems concerned with theory construction. Although many authors maintain they are constructing axiomatic theories, the logical structure of their linguistic proposals often remains obscure. One author, however, explicitly advocates the rules of derivation contained in ordinary language in deductively constructing axiomatic theories in sociology. Hans Zetterberg in his last two editions of *On Theory and Verification in Sociology* (1963, 1965), attempts to derive "ordinary propositions" from "theoretical propositions" by articulating procedures which he takes to exemplify the "derivation rules of ordinary language."¹

Anyone acquainted with the major controversies in the philosophy of logic exhibited by the debates between logical empiricists and ordinary language philosophers can appreciate the importance of undertaking such a task.² It is debatable whether an elaborate, constructed symbolic system creates more

problems of analysis than it resolves.³ There is, on the other hand, little doubt that explicating the logic of ordinary language is extremely difficult and that a satisfactory account will be a long time in coming. Nevertheless, when one attempts to codify even limited aspects of the logic of theory construction in sociology, it is important to analyze the proposal made and explore its basic implications. Accordingly, we shall analyze Zetterberg's proposed derivations of ordinary from theoretical propositions and explore the implications of this proposal for his claims regarding the virtues of axiomatic theory construction in sociology. Our more general concern, however, is to show by our analysis the pitfalls in uncritically adopting any strategy of theory construction to extend sociological knowledge.

Most sociologists who either criticize or follow Zetterberg's strategy in constructing a theory emphasize that part of his work where he uses a simple rule similar to middle-term-syllogism as a rule of deduction.⁴

¹ Although Zetterberg implicitly countenances the use of formal logic in constructing axiomatic theories (Zetterberg, 1965:92, 100), he explicitly advocates the use of "the deduction rules of ordinary language" as long as "our concepts are defined in ordinary prose" (Zetterberg, 1965:163); moreover, in his characterization of various types of "causal" propositions he deliberately employs informal discourse.

² Compare, for example, the alternative analyses of the relations of formal logic to ordinary language given by Strawson (1952) and Reichenbach (1966).

³ Cf. Scriven (1958) and Strawson (1963) with Beth (1963) and Carnap (1963).

⁴ For instance, see Costner and Leik (1964). We should, however, add that although Costner and Leik's "Deductions from 'Axiomatic Theory'" may be a valuable comment on a system of correlation coefficients, it is an inadequate and a rather incomplete reconstruction of Zetterberg's proposal. Costner and Leik have demonstrated the inadequacy and limitations of the statistical language in which they reconstructed Zetterberg's work. Therefore, their analysis does not directly bear on the logical structure of Zetterberg's language.

They overlook the fact that Zetterberg relies on entirely different rules in deriving ordinary from theoretical propositions. These rules are intended to allow one to derive a large number of ordinary propositions from a small set of theoretical propositions. This strategy is supposed to be better than the one which is simply a permutation or combination of N variables taken two at a time in propositions of the form "the more . . . , the more . . . ," "the more . . . , the less . . . , etc.

Since we are primarily interested in the rules which allow one to derive a large number of ordinary propositions from a small set of theoretical propositions, we shall first examine Zetterberg's articulation of this distinction and then analyze his examples.⁵

⁵ The reader may be surprised to see that we do not consider Zetterberg's most famous example of what he takes to be an axiomatic theory, namely, his "axiomatization" of a set of propositions loosely extracted from Durkheim's *The Division of Labor in Society* (Zetterberg, 1965:159-66). We do not do so because in form and intent we consider this example a crude caricature of historically given axiomatized scientific theories. Zetterberg clearly gives the impression that to construct an axiomatic theory it is sufficient to divide a set of propositions into postulates and theorems, to derive the latter from the former, and to check that no one postulate is derivable from any set of other postulates. Evidently, he is not alone in this belief. (See Bailey, 1970, wherein he takes this view and also cites several other sociologists whose views are very close to the above. Also see Parsons, 1959:700-2 for a more sophisticated conception of logico-deductive theories. But note his application. He thinks that his account of the theory of action was a successful attempt at this type of theory construction. Cf. Black, 1961.) Although these are formal characteristics of axiomatized theories, these theories are distinguished from other deductive systems in that they possess additional formal features and are constructed for reasons other than explaining and predicting. We cannot in this paper substantiate this claim but a wealth of noncontroversial literature fully supports it. We cite a few readily available references here. See Blanché (1962) for an excellent history of axiomatics. A useful lesson to be learned from him is that axiomatic theories need not be fully formalized. For a clear exposition of the relation between uninterpreted formal axiomatic systems and their scientific interpretations read Hempel (1949). Hochberg (1959) has sketched a comprehensive philosophical position in the context of explicating axiomatic systems and suggesting their usefulness or lack thereof for sociologists. Probably the most helpful account for sociologists, however, is Bergmann's (1966); for he addresses himself to the fruitfulness and significance of axiomatization in social science.

THE RELATIVE INFORMATIVE VALUE OF PROPOSITIONS

Zetterberg makes a distinction between two types of propositions, theoretical and ordinary. According to him, this distinction follows from the informative value of propositions. Propositions of high informative value are called theoretical, while those of low informative value are called ordinary. Zetterberg expresses in two different ways the general principles which determine such informative value. He says,

In general, the number of different ways in which a proposition can conceivably be proved incorrect, the higher its informative value. Put differently, the higher the informative value of a proposition, the greater the variety of events for which it can account (1965:79).

The first way of specifying informative value seems to invoke the principle of the *falsifiability* of a proposition. But the second way is based on some notion of the *universality* or *scope* of a proposition. Although these may be viewed as complementary criteria for assessing a proposition's informative value, they do not in every case lead to equivalent results. As will be shown later, one proposition may be more *universal* than another; and yet the latter may be more *precise* than the former. Thus, they would, in a strict sense, be noncomparable in their degree of falsification. It is unclear how Zetterberg proposes to combine the two somewhat different criteria.

It would appear that Zetterberg is using the criteria Karl R. Popper (1954) uses to assess the empirical content of statements. For when Zetterberg addresses the subject of informative content in the context of confirmation of propositions, he quotes the following from Popper:

Science does not aim, primarily, at high probabilities. It aims at *high informative content*, well backed by experience. But a hypothesis may be very probable simply because it tells us nothing, or very little (Zetterberg, 1965: 102, italics supplied).

In addition, Popper's criteria for comparing the empirical content of statements is based on the principle of derivability or implication. Our imputation of Popper's view to Zetterberg is strengthened when we consider Zetterberg's criterion for comparing propositions. He states:

A critical task for the theorist in any science is to subsume a large number of propositions of low informative value under a few propositions of higher informative value. When the theorist asks about a proposition, "What does it mean?" he wants to know also (1) what are the less informative propositions that are *implied* in the one under consideration, and (2) what are the more informative propositions that *imply* the one under consideration (Zetterberg, 1965:79-80, italics supplied).

To elucidate Zetterberg's view it might then be well to discuss briefly Popper's conception of the empirical content or informative value of statements.⁶

Popper, along with other philosophers of science, defines the empirical content or informative value of a statement as being equal to the class of those events which it rules out or prohibits. The more possibilities a statement excludes, the higher its content. A statement which is compatible with all possibilities, a tautology, has zero content, i.e., it informs us of nothing. Since the content of a statement is equal to the class of those empirical events which it rules out or prohibits, it follows that falsifiability or testability determines the content of a statement and vice versa.

Falsifiability and logical probability are also closely related. The more falsifiable a statement, the lower its logical probability. This relation may be expressed, according to Popper, as $f(g) = 1 - p(g)$, where $f(g)$ is the falsifiability of a statement, and $p(g)$ is its logical probability, i.e., the a priori likelihood of a statement's being true. Substituting $\text{cont}(g)$ for $f(g)$, the relation between content and logical probability is thus defined: $\text{cont}(g) = 1 - p(g)$ (Popper, 1965).⁷

⁶ The content of a statement is defined, according to the scholastic dictum *omnis determinatio est negatio*, as the class of those logically possible worlds, or empirical events, which are ruled out by the statement in question (Bar-Hillel, 1964). The concept of content has also been defined implicitly within different logical and mathematical systems. One difficulty with the formal calculus of content is that it does not satisfy the requirements of additivity of the content of two inductively independent sentences. As a result, "information" has been introduced which is a nonlinear monoton-increasing function of content (Bar-Hillel, 1964). Since in this paper, the technical distinction between "content-measure" and "information-measure" is not crucial, the terms "information" and "content" will be used interchangeably.

⁷ In the mathematical theory of communication,

Popper does not hold that the empirical content or falsifiability of statements can logically be compared in terms of a metric measure. Nevertheless, he maintains that the empirical content or falsifiability of statements whose classes of potential falsifiers stand in class-subclass relations with reference to each other can be compared on an ordinal scale. For instance, the statement p is said to have a larger content than the statement q if, and only if, the class of the events prohibited by p includes the class of events prohibited by q as a proper subclass. The empirical content of statements whose classes of potential falsifiers do not satisfy the class-subclass relations is simply noncomparable (Popper, 1965).

The class-subclass or derivability relations are determined by both the level of universality and degree of precision of a set of statements—not by the level of universality alone. As a logical rule:

If of two statements both their universality *and* their precision are comparable, then the less universal or less precise is derivable from the more universal or more precise; unless, of course, the one is more universal and the other more precise . . . (Popper, 1965:123).

What is important to note then is the distinction between the universality of a statement and its precision. The universality of a statement refers to the extension of its subject, i.e., the class of entities about which an assertion is made. The precision of a statement, on the other hand, refers to the restriction of its predicate, i.e., that which is asserted of the subject. The larger the subject class, the higher the universality of a statement. Further, the smaller the predicate class, the more precise the statement.

Therefore, the content or informative value of a statement is determined by both its level of universality and degree of precision. The higher its level of universality, the greater

"information" is also a decreasing function of "probability." In this theory, "information" is defined in terms of the relative frequency of the transfer or receiving of a message or sequence of signals. The amount of information contributed by a message to a communicative situation is a logarithmic function of the inverse of the statistical probability of the message as a kind of event, or $\text{inf.} = \ln 1/p_i$ (Bar-Hillel, 1964; Hartley, 1928; Shannon and Weaver, 1949; Wiener, 1961).

its informative value. At the same time, the higher its degree of precision, the greater its informative value.

With regard to the relative informative value of two statements, statement p is said to have more informative value than statement q if, and only if, p logically implies q , or, if ' $p \supset q$ ' is a tautology (Popper, 1965).

Let us examine an illustration Popper offers in discussing the relationship between the demand for the highest possible empirical content, and levels of universality and degree of precision of statements. He asks that the following conceivable natural laws be considered:

- p : All heavenly bodies which move in closed orbits move in circles: or more briefly:
All orbits of heavenly bodies are circles.
- q : *All orbits of planets are circles.*
- r : *All orbits of heavenly bodies are ellipses.*
- s : *All orbits of planets are ellipses* (Popper, 1965:122).

The derivability relations holding among these four statements are a function of their universality *and* precision. The relative informative value or content of these four statements then is determined accordingly.

Moving from p to q the *degree of universality* decreases; and q says less than p because the orbits of planets form a proper subclass of the orbits of heavenly bodies. Consequently p is more easily falsified than q : if q is falsified, so is p , but not vice versa. Moving from p to r , the degree of precision (of the predicate) decreases: circles are a proper subclass of ellipses; and if r is falsified, so is p , but not *vice versa*. Corresponding remarks apply to the other moves: moving from p to s , the degree of both universality and precision decreases; from q to s precision decreases; and from r to s , universality. To a higher degree of universality or precision corresponds a greater (logical or) empirical content, and thus a higher degree of testability (Popper, 1965:122).

In light of the preceding discussion, and particularly of Popper's illustration of the derivability principle, we turn now to Zetterberg's example of what he takes to be ordinary and theoretical propositions.⁸

⁸ Although we are discussing Zetterberg's proposal within the context of Popper's work, we are not imposing Popper's unique methodological criteria on Zetterberg. We could have carried our analysis without any mention of Popper whatever.

THE CONSTRUCTION OF ORDINARY AND THEORETICAL PROPOSITIONS

Zetterberg maintains that "... it is essential for both researchers and practitioners to learn to extract the ordinary propositions from theoretical ones" (Zetterberg, 1965: 80). Therefore he asks for the ordinary propositions implied in the following theoretical proposition:

1. "Persons tend to engage in actions that maintain the evaluations they receive from their associates" (Zetterberg, 1965:80).

He then proceeds to find special cases of this proposition by searching in what he calls a taxonomy, first, for all terms that have "evaluations" as a "component," and second, for terms that are generated by "decomposing" the term "actions." Without going into the details of his procedure for "decomposing" terms, we can readily see its general nature. He takes general terms in *only* the predicate position and finds proper subsets of these terms to construct, by explicit definitions, ordinary propositions. For example, by defining "rank" as a proper subclass of "evaluations," he generates the following proposition which he takes to be more ordinary than the above theoretical proposition:

2. "Persons tend to engage in actions that maintain the rank they enjoy in their social structure" (Zetterberg, 1965:81).

By "decomposing" the word "action," by explicit definition, into proper subclasses, one of which is "prescriptions," he produces the following proposition, which he takes to be more ordinary than either (1) or (2) above:

3. "Persons tend to issue prescriptions that maintain the rank they enjoy in their social structure" (Zetterberg, 1965:81).

Similarly, "social structure" is "decomposed" into proper subsets, one of which is "electorate," the following ordinary proposition is then derived which he takes to be the most ordinary of the four propositions:

4. "Persons tend to issue prescriptions that maintain the rank they enjoy in their electorate" (Zetterberg, 1965:82).

To give a clearer picture of Zetterberg's logic of derivation and conception of the relative information value of propositions, let us reconstruct the above portion of his work in the following simple form, where P_i refers to the above numbered propositions:

The complex predicate of P_2 (actions that maintain the rank they enjoy in their social structure) is a proper subset of the complex predicate of P_1 (actions that maintain the evaluations they receive from associates); therefore, according to Zetterberg:

- a. P_2 is logically implied by P_1 ; i.e., ' $P_2 \supset P_1$ ' is a tautology.
- b. P_1 is more falsifiable and more severely testable than P_2 .
- c. P_1 is more universal than P_2 .
- d. P_1 has higher informative value than P_2 .
- e. P_1 is a more theoretical proposition than P_2 .

The complex predicate of P_3 is a proper subset of the complex predicate of P_2 and the complex predicate of P_4 is a proper subset of the complex predicate of P_3 . Therefore what holds for ' $P_1 \supset P_2$ ' also holds for ' $P_2 \supset P_3$ ' and ' $P_3 \supset P_4$ '.

So P_1 is the strongest statement and P_4 is the weakest statement of the four propositions.⁹

At this point it should be profitable to compare Zetterberg's ranking of propositions (1) through (4) with the way they would be ranked by the derivability or deducibility principle, as demonstrated in Popper's fore-mentioned example. P_2 would be higher in empirical content than P_1 . The comparison with P_1 indicates that they both have the same universality in what we might call their subject term, i.e., "persons"; but P_2 is more precise than P_1 in their respective "predicate term"; i.e., "actions that maintain the rank they enjoy in their social structure" is a proper subset of "actions that maintain the evaluation they receive from their associates." Thus P_2 says more than P_1 because if P_1 is falsified so is P_2 , but not vice versa. This is a consequence of P_1 being logically entailed by P_2 , which makes the statement ' $P_2 \supset P_1$ ' a tautology. So P_2 is more falsifiable and more severely testable than P_1 . Likewise, P_3 is more falsifiable and more severely testable than both P_2 and P_1 and has a higher degree of empirical content than

both. P_1 and P_2 are both logically entailed by P_3 , and that makes ' $P_3 \supset P_2$ ' and ' $P_3 \supset P_1$ ' tautologies. P_4 is logically the strongest statement among the four propositions. P_4 is the strongest and the most informative in the sense that it excludes more possible states of affairs than any of the other propositions. So there is at least one logically possible world in which P_4 is false and P_1 , P_2 , and P_3 are all true. Also, P_4 logically implies the three propositions P_1 , P_2 , and P_3 . We may summarize the foregoing analysis in the following form:

The complex predicate of P_2 is a proper subset of the complex predicate of P_1 , therefore:

- a. P_2 logically implies P_1 ; i.e., ' $P_2 \supset P_1$ ' is a tautology.
- b. ' $P_1 \supset P_2$ ' represents a synthetic (empirical) statement.
- c. P_2 is a stronger statement than P_1 ; P_2 excludes more possible worlds; P_2 is more severely testable, and more falsifiable than P_1 . There is at least one logically possible world in which P_2 is false and P_1 is true.
- d. P_2 has more empirical content and is more informative than P_1 .

The complex predicate of P_3 is a proper subset of the complex predicate of P_2 , and the complex predicate of P_4 is a proper subset of the complex predicate of P_3 . Therefore, what holds for ' $P_2 \supset P_1$ ' also holds for ' $P_3 \supset P_2$ ' and ' $P_4 \supset P_3$ '.

Thus, P_1 is the weakest, the least falsifiable, the least testable, and the least informative proposition among the above four propositions. *This proposition has the highest logical probability among the four propositions simply because it tells us less than the other three propositions.*

Now, if the distinction between ordinary and theoretical propositions is to be based on the principles of the universality, falsifiability, derivability, and the proposition's informative value, which appears to be the case with Zetterberg's proposal, it should follow logically that P_4 is a theoretical proposition having the highest informative value relative to the other three propositions; P_1 is an ordinary proposition having the lowest informative value among the four propositions; P_3 and P_2 fall somewhere in between the two extremes. Ironically, these implications are the exact opposite of those Zetterberg has drawn.

Zetterberg apparently assumes that in the case of two propositions having the same

⁹ The proposition p is said to be logically stronger than the proposition q if, and only if, p logically implies q , or if ' $p \supset q$ ' is a tautology.

universality in their subject term, the proposition containing the more restricted predicate is logically derivable from the proposition containing the less restricted predicate. This notion is, however, contrary to the mathematical and logical rules of deduction. From the proposition "All orbits of heavenly bodies are ellipses," the proposition "All orbits of heavenly bodies are circles" cannot be derived. However, Zetterberg seems to insist that we could derive it if we followed the deduction rules of ordinary language.

Having used Popper's analysis to examine Zetterberg's example, let us construct an example in ordinary language and inquire whether Zetterberg's "rules of deduction" fare any better on less formal grounds.

Suppose that we have the following statement:

I. British men tend to marry women who are under thirty-four years of age.¹⁰

This statement represents a legitimate claim for which we might easily find empirical support. Now, according to Zetterberg, to derive the ordinary propositions implied in the one under consideration, we should go to our taxonomy to decompose the class of under thirty-four years of age. Let us suppose that our taxonomy divides it into "under thirty," "under twenty-five" . . . and finally "under five." We can then write the following proposition as a logical implication of proposition I and the appropriate class-subclass definition:

II. British men tend to marry women who are under five years of age.

There is considerable debate among logicians over the extent of formal parallelism between the use of "implies" in ordinary language and the rules that govern the use of " \supset " (the sign of implication) in modern symbolic logic. But it is doubtful the most ardent defender of ordinary language analysis would claim that the above argument is

¹⁰ This example may strike one as not being the kind of proposition which Zetterberg would call theoretical. However, our intention is merely to show Zetterberg's logic of derivation and explicate what he seems to imply by the "deductive rules of ordinary language."

deductive at all.¹¹ The direction of derivability relation holding between these two statements is from statement (II) to statement (I), *but not vice versa*. Namely, statement (II) logically entails statement (I). An inference from statement (I) to statement (II) is inductive and subject to the inductive risk.

Since Zetterberg is, in fact, reversing the direction of derivability relations which hold among statements, then formally every statement in Zetterberg's logical system should be derivable from a tautology.¹² Accordingly, tautological statements should be regarded as the most informative propositions. Yet, tautologies are factually empty. Indeed, if we follow Zetterberg's logic, we will arrive at non-falsifiable propositions of little, if any, content. For instance, according to Zetterberg's proposal, the following proposition must be the highest theoretical proposition relative to Propositions (1) through (4) from his book which we cited earlier:

1'. persons tend to engage in actions.
"Actions" is the superset containing all other subsets to which Zetterberg refers. The class of actions may be divided into two proper subsets. One subset includes "actions that maintain the evaluation one receives from his associates," and the other subset includes "actions that do not maintain the evaluations one receives from his associates." Therefore, proposition (1') should be the highest theoretical proposition and the most informative one. However, it is not at all clear that proposition (1') is even empirically falsifiable. This proposition has a logical probability approaching 1.

To have a maximum tolerance for the linguistic proposals of others, we must allow them their own rules of deduction. We may then apply their criteria to see if their proposals are self-consistent. According to Zetterberg's criteria, a theoretical proposition is one of high informative value in the sense that there are a relatively large "... number

¹¹ Strawson (1952) thinks that most logicians have not sufficiently recognized the disparity between the sense of implication in ordinary language and the one in symbolic logic. Quine (1967), however, thinks that Strawson (1952) has certainly exaggerated the significance of any legitimate difference.

¹² In deductive logic, a tautology follows from every statement.

of different ways in which (it) can conceivably be proved incorrect. . . . Put differently (there are a great) . . . variety of events for which it can account" (Zetterberg, 1965:79). However, we have just seen that Zetterberg's highly theoretical propositions are almost nonfalsifiable in the sense that there is slight chance to prove them incorrect. Furthermore, judged by their level of universality, his theoretical propositions do not differ from his ordinary propositions. Hence, from the standpoint of Zetterberg's criteria, his highly informative propositions are almost noninformative.

Before closing our discussion of Zetterberg's strategy, we will analyze logically arguments he produces in discussing "axiomatization through definitional reduction" (1965:94-6) to illustrate that there is in some cases little disparity between ordinary language analysis and the application of formal logic, even when loose concepts are employed.¹³ On careful analysis, we can say that the following deductions have been advanced;

Deduction 1	Premises
Groups have less turnover than publics. (proposition 1)	
Groups are social aggregates interacting in terms of specified roles and with a common leader. (definition A)	
Publics are social aggregates interacting in terms of specified roles but without a common leader. (definition C)	
If a social aggregate has a common leader, then its turnover is low. (proposition I)	
	Conclusion

Deduction 2	Premises
Publics show less emotion than crowds. (proposition 2)	
Groups show less emotion than masses. (proposition 3)	
Publics are social aggregates interacting in terms of specified roles but without a common leader. (definition C)	
Groups are social aggregates interacting in terms of specified roles and with a common leader. (definition A)	
Masses are social aggregates interacting in	

terms of unspecified roles but with a common leader. (definition B)

Crowds are social aggregates interacting in terms of unspecified roles and without a common leader. (definition D)

If a social aggregate interacts in terms of specified roles, then its level of emotion is low. (proposition II)	Conclusion
---	------------

Proceeding with his deduction, Zetterberg states "Proposition I and Definition B imply that 'masses have less turnover than crowds.'" He then says "This is a novel hypothesis which, to the best of my knowledge, is presented here for the first time" (Zetterberg, 1965:96). Let us also put this latter deduction in systematic form.

Deduction 3	Premises
If a social aggregate has a common leader, then its turnover is low. (proposition I)	
Masses are social aggregates interacting in terms of unspecified roles but with a common leader. (definition B)	
Masses have less turnover than crowds. (the new proposition)	Conclusion

According to the rules of modern symbolic logic, all deductions in this example are invalid. For a categorical statement, i.e., "if a social aggregate . . . , then its . . . is low" can never be logically derived from a comparative statement, i.e., ". . . have less . . . than. . ." Groups may have less turnover than publics while at the same time both may have a "high" turnover. The same logic is applicable to Zetterberg's second deduction. The invalidity of Zetterberg's third deduction is also obvious. His argument includes a premise, proposition I, which has already been derived through an invalid inference. Nevertheless, even if we accept his premises, no deduction rule, including the deduction rules implied in some ordinary language, would allow Zetterberg to derive his conclusion from the given set of premises. For the term "crowd" does not occur in the premises of Zetterberg's third deduction.

CONCLUSION

To this point our primary concern has not been to criticize Zetterberg's proposal, but to understand it and follow some of its implications.

¹³ In fact, Black (1970:1-13) has convincingly argued that unless formal logic applies to arguments using loose concepts, there seems to be no empirical domain for logical analysis.

We find Zetterberg's proposal vague. From what we have reconstructed through his examples, we have shown that his proposal is not self-consistent and leads to contradictions. Moreover, he seems to violate the most fundamental rules of deductive logic, whether traditional or modern. Thus, we have tried to exhibit Zetterberg's failure to produce a coherent strategy for theory construction.

Space does not permit us to detail our own, admittedly incomplete, proposal for extending and organizing sociological knowledge. We can, however, point out that we doubt seriously the usefulness of axiomatic method in sociology and other theoretical disciplines.¹⁴ And we do not think that impoverished axiomatic theories like Zetterberg's would direct research in the way he and others claim. However, if these claims are to be vindicated, then we must draw upon the full apparatus of modern (truth-functional and quantificational) deductive logic and theories of nondeductive inference. Note that we do not advocate replacing sociological theories in ordinary language with those formalized in a logical or mathematical system. Nevertheless, the application of such formal systems is definitely useful for evaluating sociological arguments. We think they are indispensable in evaluating alternative strategies of theory construction.

BIBLIOGRAPHY

- Bailey, Kenneth D.
1970 "Evaluating axiomatic theories." Pp. 48-71 in Edgar F. Borgatta (ed.), *Sociological Methodology*, 1970. San Francisco: Jossey-Bass.
- Bar-Hillel, Yehoshua
1964 *Language and Information*. Reading, Massachusetts: Addison-Wesley.
- Bergmann, Gustav
1966 *Philosophy of Science*. Madison: The University of Wisconsin Press.
- Beth, E. W.
1963 "Carnap's views on the advantages of constructed systems over natural languages in the philosophy of science." Pp. 469-502 in Paul A. Schilpp (ed.), *The Philosophy of Rudolf Carnap*. La Salle, Ill.: Open Court.
- Black, Max
1961 "Some questions about Parsons' theories." Pp. 268-88 in Max Black (ed.), *The Theories of Talcott Parsons*. Englewood Cliffs, New Jersey: Prentice Hall.
- 1970 *Margins of Precision*. Ithaca, New York: Cornell University Press.
- Blanché, Robert
1962 *Axiomatics*. New York: The Free Press.
- Carnap, Rudolf
1963 "E. W. Beth on constructed language systems and P. F. Strawson on linguistic naturalism." Pp. 927-40 in Paul A. Schilpp (ed.), *The Philosophy of Rudolf Carnap*. La Salle, Illinois: Open Court.
- Costner, Herbert L. and Robert Leik
1964 "Deductions from axiomatic theory." *American Sociological Review* 29 (December): 819-35.
- Einstein, Albert
1959 "Remarks concerning the essays brought together in this co-operative volume." Pp. 665-88 in Paul A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*. New York: Harper Torchbooks.
- Hartley, R. V. L.
1928 "Transmission of information." *Bell System Technical Journal* 7 (April): 535-63.
- Hempel, Carl G.
1949 "Geometry and empirical science." Pp. 238-49 in Herbert Feigl and Wilfrid Sellars (eds.), *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts.
- Hochberg, Herbert
1959 "Axiomatic systems, formalization, and scientific theories." Pp. 407-36 in Llewellyn Gross (ed.), *Symposium on Sociological Theory*. New York: Row, Peterson and Company.
- Parsons, Talcott
1959 "An approach to psychological theory in terms of the theory of action." Pp. 612-711 in Sigmund Koch (ed.), *Psychology: A Study of a Science*. New York: McGraw-Hill.
- Quine, Willard Van Orman
1967 "Mr. Strawson on logical theory." Pp. 236-52 in Irwing M. Copi and James A. Gold (eds.), *Contemporary Readings in Logical Theory*. New York: Macmillan.
- Popper, Karl R.
1954 "Degree of confirmation." *The British Journal for the Philosophy of Science* 5 (August): 143-9.
1963 *Conjectures and Refutations: The Growth of Scientific Knowledge*. New York: Harper Torchbooks.
1965 *The Logic of Scientific Discovery*. New York: Harper and Row.
- Reichenbach, Hans
1953 "The philosophical significance of the theory of relativity." Pp. 195-211 in Herbert Feigl and May Brodbeck (eds.), *Readings in the Philosophy of Science*. New York: Appleton-Century-Crofts.
1966 *Elements of Symbolic Logic*. New York: The Free Press.
- Scriven, Michael
1958 "Definitions, explanations, and theories." Pp. 99-195 in Herbert Feigl, Michael Scriven, and Grover Maxwell (eds.), *Min-*

¹⁴ Cf. Einstein (1959) and Reichenbach (1953). Also see Popper (1963).

- nesota Studies in the Philosophy of Science, Vol. 2. Minnesota: University of Minnesota Press.
- Shannon, Claude E. and Warren Weaver
1949 The Mathematical Theory of Communication. Urbana, Illinois: The University of Illinois Press.
- Strawson, P. F.
1952 Introduction to Logical Theory. London: Methuen & Co., Ltd.
1963 "Carnap's views on constructed systems versus natural languages in analytic philosophy." Pp. 503-18 in Paul A. Schilpp (ed.), The Philosophy of Rudolf Carnap. La Salle, Illinois: Open Court.
- Wiener, Norbert
1961 Cybernetics. 2nd edition. New York: John Wiley.
- Zetterberg, Hans
1954 On Theory and Verification in Sociology. New York: The Tressler Press.
1963 On Theory and Verification in Sociology. Revised edition. Totowa, New Jersey: Bedminster Press.
1965 On Theory and Verification in Sociology. 3rd enlarged edition. Totowa, New Jersey: Bedminster Press.

FUNCTIONAL ALTERNATIVES AND ECONOMIC DEVELOPMENT: AN EMPIRICAL EXAMPLE OF PERMANENT EMPLOYMENT IN JAPAN *

ROBERT E. COLE

University of Michigan

American Sociological Review 1973, Vol. 38 (August):424-438

The presentation concerns the utility of conceptualizing the structural changes associated with modern economic development as functional alternatives. The concept is compared to other approaches represented as historicism, convergence, and structural modeling with environmental effects. The advantages of the functional alternative conceptualization are demonstrated through comparison of selected employment characteristics in Japan and the United States.

A SURVEY of modernization literature reveals the crudity of the analytic formulations offered. Most often, the goal of empirical research seems to be nothing less (and nothing more) than establishing similarities or differences in attitudes and social structure relative to the attitudes and social structure anticipated by modernization theory. Commonly, establishing differences leads the researcher to emphasize cultural or historicist explanations (or lag phenomena); establishing similarities, on the other hand, leads researchers to emphasize commonalities that all societies share or commonalities they share by virtue of having achieved a given level of economic development. A good example is the debate that has grown around

the study of occupational prestige rankings in different societies (e.g., Inkeles and Rossi, 1956; Hodge, Treiman and Rossi, 1966).

This paper deals with the concept of functional alternatives. Properly used, this concept provides important leverage for sociologists engaged in comparative research, particularly those working in the area of economic development and social structure. In general, this concept has been used to suggest that a range of structural or value arrangements exists that may fulfill a common function. Despite a growing disenchantment with structural-functional analysis by contemporary sociologists, even those more empirically-oriented, continue to find the concept of functional alternatives useful (e.g., Stinchcombe, 1968:80-125; Coleman, 1969: 291-2). There are good reasons for this. The final section of the paper will examine the system of employment security dominant in large-scale Japanese firms to demonstrate the utility of this concept.

We may summarize the above alternative conceptualizations with a typology that com-

* This paper benefited from the caustic comments of Arnold Feldman, Paul Siegel, and Thomas Smith. Lois Verbrugge's suggestions were less biting but of great value. I take full responsibility for the use and misuse of these comments and criticisms. An early draft of this paper was presented at the Interdisciplinary Conference on Processes of Change in Contemporary Asian Societies, University of Illinois, November, 1970.

Figure 1

Comparative Response of Two or More Societies to Modern Economic Growth

		STRUCTURE	
		Different	Same
FUNCTIONS	Different	Historicism	Structural Modelling with Environmental Effects
	Same	Functional Alternatives	Convergence Theory

compares two or more societies in their response to modern economic growth (see Figure 1). Historicism, as presented in Figure 1, is an argument for uniqueness in both structure and structural consequences. It denies attempts to formulate generalizations applicable to more than one society, culture or period. Its focus is on the crystallizing of temporal events in unique organizational and societal patterns. One understands the meanings of these historical experiences only by concentrating on relevant historical events. Robert Nisbet (1969) in his provocative book *Social Change and History* comes close to taking this position in his critique of macro-sociological theory.

Comparative sociologists reject the historicist view; they seek generalizations which apply to more than one society (Shils, 1963: 1-26; Bendix, 1963:532-9). They expect that the historical experience of one society will illuminate the meanings of historical experiences in other societies. Ideally, these general propositions are not rooted in any one society; they transcend specific societies. Yet, they prove their utility by helping to explain specific empirical processes.

To reject historicism is not to deny the conception of social structure as a system of historical dimensions. Indeed, as Reinhard Bendix (1963:437) suggests, comparative analysis forces us to see social structure not "as a natural system with defined limits and invariant laws governing an equilibrating process, but rather as a system of historical dimensions." These historical dimensions influence point of origin, route and temporary destinations of social structures under the impact of and interacting with economic development. To say that social structure is a system of historical dimensions means that

historical context influences the operation of seemingly invariant processes such as industrialization and thus makes possible a meaningful conception of process (cf. Nisbet, 1969; Gerschenkron, 1962). The extent to which we can conceptualize historical processes intelligently is based on our ability to separate random from nonrandom processes. As Boulding (1970:16) notes, the human mind has a profound tendency towards superstition that is "the imposition of a spurious order on its observation of random processes."

The issue at hand is to incorporate the role of history in our conception of social structure without compromising the generalizing goal of social science. Clearly, historicism does not allow such an outcome. The concept of functional alternatives does offer this flexibility.

We may now consider the cell in Figure 1 that represents convergence theory. Convergence theory argues for the increasing similarity of structural arrangements and their consequences in industrial societies. Briefly put, it envisions that, with advanced industrialization, unique national identities fade and common solutions to problems of social organization come to prevail. Scholars more or less identified with this position are Clark Kerr and associates (1966), Alex Inkeles (1966) and Marion Levy (1966). For many scholars, the convergence position is a technocratic one which asserts that social and political relationships must be restructured to mesh with the complex technological organization characteristic of higher levels of economic development (cf. Weinberg, 1968:10). The constraints of modern technology and economic organization are seen as the center of a series of concentric circles

which gradually impose convergence on the outer circles of social structure and value orientation. For others like Levy (1966:744) the source of convergence is the diffusion resulting from cultural contact between the relatively modernized and relatively non-modernized societies.

Convergence theory has not lacked critics. Arnold Feldman and Wilbert Moore (1959) have accepted the imagery of concentric circles but argued that convergence is limited to the "core" elements of the industrial system, with all industrial societies possessing the minimum characteristics of: a factory system of production, a stratification system based on a complex and extensive division of labor and hierarchy of skills, an extensive commercialization of goods and services and their transfer through the market, and an educational system capable of filling the various niches in the occupational and stratification system. Beyond these minimum core characteristics, Feldman and Moore emphasize the elements of divergence in industrial systems. Similarly, Goldthorpe (1966:648-59) duns convergence theorists for adopting an exaggerated degree of determinism which focuses on the impact of material exigencies on social structure. Critics, in short, have attacked the assumption of strict functional inter-dependence among component parts and stressed the partial nature of solutions to problems of social organization in the course of economic development.

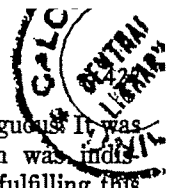
In stressing the partial nature of solutions to problems of economic organization, Bendix (1964) agrees that the industrial revolution imposes common imperatives on industrializing nations. Yet, he emphasizes the way these imperatives combine with the unique historical experiences of each country to produce an amalgam. This amalgam denies the simple applicability of one nation's experience to another. The significance of this position is that each successive level of industrialization may be seen as opening up common options and closing others, but the actual choices people make are based on subtle interactions between these common options and the specific social, political, economic and cultural history of the country in question. The problem with convergence theory lies in its proponents' willingness to

exaggerate the organizational requirements of modern industrial society without recognizing the needs of purposive historical actors (cf. Goldthorpe, 1966:648-59).

Thus far, our discussion has focused on the two cells, historicism and convergence theory. Both represent "all or nothing" propositions and, as such, appear simplistic. Edward Shils (1963) argued some time ago that we need sociological concepts which both allow for societal uniqueness and explain it in a wider analytical framework. Historicism allows for uniqueness—historical uniqueness is the very basis of this formulation—but it cannot explain it in a wider analytical framework. Convergence theory has an analytical framework, to be sure, but it does not allow for a society's historical uniqueness. With these limitations in mind, we turn to the remaining two cells.

The first is labeled structural modeling with environmental effects. The possibility of developing similar structural arrangements but with different outcomes has not been formulated by sociologists as a major societal response to economic development. Nevertheless, social scientists have often pointed out how "modern" appearing structural arrangements in nonwestern societies, often borrowed from the West, have consequences (unanticipated from a western viewpoint) quite different from their consequences in western societies (e.g., Riggs, 1966:368-71). These different consequences proceed from the fact that the new structural arrangements have to cope with and draw resources from quite different social and even physical environments. Such arrangements are often designed to meet quite different goals; this fact usually leads nations to select only those western characteristics which meet the needs of their leaders (native or colonial). The Japanese university system, for example, though modeled first after the French, American and finally Prussian systems in the pre-World War II period and the American system in the post-war period, has had a number of distinctive consequences not shared by its counterparts in Western Europe and America. These different consequences proceed from the quite different social context in which this institution appeared. Michio Nagai (1971) excellently documents

FUNCTIONAL ALTERNATIVES DEVELOPMENT



the impact of western models of education in conjunction with the needs of the Japanese in prewar Japan. In summary, conceptualizing the relationship between economic development and social change in terms of structural modeling with environmental effects appears to be a fruitful research strategy.

This brings us to the fourth cell, which is the focus of this paper. We shall use the concept of functional alternatives to refer to the empirical possibility that social units evolve different structural arrangements to solve common problems. In the past, sociologists have used such concepts as functional equivalents, functional alternatives, functional substitutes, and functional analogues loosely and interchangeably. Our discussion will apply to all these concepts. The terms themselves invoke the language of structural-functional analysis. One can make a strong argument for dispensing with the above terminologies and thereby dismissing the baggage of functional theory by substituting a term such as structural equivalents. But there are costs to developing new terminologies, one of which is the loss of continuity with past literature on the subject.

The functional terminology in this case is based on the premise that we can identify functional prerequisites or "universal needs" which must be met for societies to persist (Aberle et al., 1950; Levy, 1966:174-87). An even more demanding version (in that it requires more specification) is that we can identify, at varying levels of development, functional prerequisites which must be satisfied if a society is to proceed with modernization. When these functional requirements are not met, further economic development will not occur. This has been an implicit assumption in much modernization literature based on the search for universal preconditions or universal obstacles to development. Gerschenkron (1962:31-51) and Hirschman (1965:385-93) provide biting critiques of these assumptions. Gerschenkron shows that many alleged preconditions are concomitants of economic growth, while Hirschman demonstrates that many alleged obstacles have on occasion benefited growth.

For some early functionalists like Malinowski (1926:136), the assumption of func-

tional indispensability was ambiguous. It was not clear whether the function was indispensable or the structural item fulfilling this function. This vagueness has by no means disappeared as Gerschenkron and Hirschman show. Nevertheless, leading spokesmen for structural-functional theory have recognized the problem. The distinction now commonly made is that we may speak of functional needs but that these needs may be met by a range of structural alternatives (Merton, 1957:32-7).

Notwithstanding these modifications, critics continue to point to the deficiencies of functional analysis. There is no need to rehash these criticisms. (See Hempel, 1959: 271-307 for a more intensive treatment.)

Those criticisms relevant to our discussion are that key terms of functional analysis such as need and functional prerequisite have been used nonempirically and clear operational definitions have not been provided. If we do not specify how these terms can be applied to the empirical world, they lead to no specific predictions and cannot be put to empirical use. In particular, functional analysis does not explain why one alternative rather than another occurs in a given system. We will consider this question later under the section, historical explanation and functional alternatives.

These weaknesses have become increasingly clear to contemporary sociologists. We need a way capitalize on the strengths of the concept of functional alternatives and to remove its limitations. To this end, I propose that we focus on common problems facing societies at given levels of industrialization. This approach allows for universal problems such as establishing factory discipline and recruiting a labor force as well as problems confronting late industrializers in a world dominated by industrialized nations. Even universal problems such as recruiting a labor force are strongly influenced by the historical timing of the industrialization (e.g., the quality of the labor force changes).

This perspective does not deny the imperatives of industrialization emphasized by convergence theorists. However, unlike convergence theory which posits common responses to these imperatives, the functional alternative position is less demanding; it posits com-

mon problems which may be solved by a limited range of alternative social arrangements. It permits us to accept societal differences without resorting to explanations based on historical uniqueness. Instead, we may incorporate these differences in a common analytic framework.

A final caution: Figure 1 is intended as a conceptual tool; we may not assume that the empirical world nicely divides into the appropriate boxes. We are not always faced with situations of total convergence or completely comparable functional alternatives. Indeed, convergence is often partial (Dunning and Hopper, 1966) and functional equivalence seldom complete (Bellah, 1957; 1963).

HISTORICAL EXPLANATION AND FUNCTIONAL ALTERNATIVES

As noted earlier, a major weakness of functional analysis is its failure to explain why one rather than another functional alternative occurs in a given system. In considering the persistence of a given structural pattern, we cannot assume that once key decisions leading to its institutionalization have been made, a society will be bound to that pattern. The distinction that must be made concerns the way specific patterns arise, and how they may or may not come to be preserved. To establish causal links we need to ask three questions: what particular set of factors generate the pattern; by what set of factors do social arrangements reproduce themselves; and to what extent do the resources responsible for reproduction remain available to adjust to changing internal tensions and environmental conditions (cf. Stinchcombe, 1968:101-2).

Where a given social arrangement is preserved, the goal is to distinguish between what caused the particular pattern, how it came to be established through a process of positive feedback or deviation amplifying feedback, and the nature of the self replicating causal loop that preserves the pattern. This issue is one of historical selection: understanding why one functional alternative and not another gets selected.

Specifically, one must identify the emergence of specific social practices and evaluate their consequences for meeting the needs of

relevant actors. Implicit in this model is the learning process by which social actors adopt appropriate behaviors in response to patterned rewards and punishments. Moreover, taking a given action which precludes the result of alternative action foregone becomes a cost which the individual may weigh against the value to be derived from taking the first action (Hemans, 1961:51-82). In summary, once participants in a social situation get satisfactory results from specific behavior patterns, *ceteris paribus*, the selective principle encouraging a search for alternative practices is weakened (Stinchcombe, 1968:105). The question to be considered is what are the costs (opportunity costs) incurred by adopting one as compared to another social arrangement (Olson, 1970:123).

In any discussion of the historical evolution of functional alternatives, we must examine the extent to which the actors are aware of the problems to be solved, alternative solutions, and constraints on borrowing. Lack of awareness of the problem and/or alternative solutions may itself be a powerful factor in selecting alternatives. It is often noted that cumulative historical experience with modern economic growth has given the contemporary third world states a wealth of alternatives for solving industrialization problems. Yet, a country's historical experience also imposes constraints on choice. A major example is 19th century China, whose elite saw China as the center of the world, and outsiders as barbarians. Consequently, it was extremely difficult for the Chinese to borrow technology and especially forms of social organization from the West. The use of Soviet industrial experience by contemporary Chinese leaders as a negative model is similar in these respects. Generally speaking, the ambiguity of contemporary third world nations toward borrowing from their ex-colonial rulers suggest similar constraints. Moreover, Marxist scholars point to the ways that the colonial heritage and continuing economic ties between the advanced industrial and nonindustrialized nations condition the kinds of borrowing which takes place and maintain the dependency status of third world nations (Frank, 1969:48-61). In a similar vein, Tilly (forthcoming) maintains that much modernization theory rests on the false assumption that we can describe

and predict social change processes through exclusive focus on processes internal to a nation while ignoring international ties of dependence.

Finally, much of the accumulated material stock of transnational resources may not be relevant for backward economies. Simon Kuznets makes a distinction between the total and the relevant stock of transnational resources (Kuznets, 1968). This is especially relevant in the area of technology. In summary, researchers need to examine the extent of awareness of problems and alternative solutions, along with the objective possibilities that alternatives will be instituted.

Structural functional analysis arose in part as a challenge to evolutionary theory. Hence, a notable willingness to dispense with historical explanations has characterized many structural-functionalists and led to the charge that the theory was ahistorical. It is true that historical causes are contained in the state of current interactions by virtue of having shaped them. It is also true that the options open to any individual in a given social situation are determined by the state of current interactions and their rules.¹ This understanding has led many sociologists to explain the existence of a given structural item in terms of its present consequences for other items in the social structure.

Yet, the options open to contemporary actors are also historical outcomes; and we come to understand them by understanding their historical development. This interpretation is based on the view that every historical event, in principle, shapes the course of subsequent events. What is important here is that not only are an individual's behavioral options historical outcomes, but his "past history of success in his activities under given circumstances determines whether he will try them again, or others like them, in similar circumstances" (Homans, 1967:90).

In practice, however, many questions about current interactions do not require historical explanations. If we want to know the present impact of religion on party pref-

erence, we do not need an historical explanation. In this case, historical explanation serves as an "indirect cause" contained in the current social interactions. If, however, we ask why religion is more highly related to party preference than sex, then we must turn to the historical process that selected this functional alternative. In short, the research strategy is dictated by the questions we ask.

AN EMPIRICAL EXAMPLE: PERMANENT EMPLOYMENT IN JAPAN²

To illustrate the approaches suggested in this paper, I will now consider the practice of permanent employment in Japan. Few social practices in Japan have so caught the attention of American social scientists as has the practice of permanent employment. Primarily through James Abegglen's (1958) widely discussed book, *The Japanese Factory*, American social scientists have become aware of this practice (see also Odaka, 1963; Yoshino, 1967; Ballon, 1969; Cole, 1971; Marsh and Mannari, 1971, 1972).

Permanent employment, or the lifetime employment system as it is sometimes called, refers to the practice by which male employees, especially those in large firms are hired on graduation from middle school, high school or college, receive in-company training and remain employees in the same company until retirement at fifty-five. So conspicuous are the Japanese of the different status of job changers that they commonly use a special term, *chūto saiyosha*, to designate such individuals. Only as a response to the labor shortage has management in large firms been willing to hire them. Because their initial wages are often lower than those of a similar age and even skill level (Evans Jr., 1971:77), and they are often treated as outsiders and interlopers by other employees, their quit rates are high relative to regular employees. In addition, they are often put on temporary status for the first few years, a practice which lessens both the management's and union's obligations toward them. Given the likelihood of marginal and inferior

¹ Defining history narrowly one may argue that a non-experimental observational basis for a science is always historical insofar as causal explanation concerns the temporal priority of causal factors (Karl Popper, 1957:38-9).

² Selected portions of the following treatment appear in "Permanent Employment in Japan: Facts and Fantasies," in *Industrial and Labor Relations Review*, Vol. 26, No. 1, October 1972. Copyright © 1972 by Cornell University. All rights reserved.

status for an unspecified period, job changing, particularly by older workers who already have investments in a job, is often regarded as a venture filled with pitfalls (Cole, 1971:128-9).⁸

In the pre-World War II period, the practice of permanent employment applied primarily to white-collar employees; but with the end of the war, the newly formed unions acted swiftly to expand the coverage to blue-collar workers. Current data (1968) on job changing by educational level, show the highest rate of job changing by high school graduates (destined primarily for white-collar jobs) followed by elementary school graduates (destined for blue-collar jobs) and university graduates. The respective rates are: 4.2, 3.4 and 3.2. Elementary school graduates, however, show a higher rate of job changing under age thirty-five with high school graduates having the higher rate over thirty-five. (Office of the Prime Minister, 1970:135).

The permanent employment practice in Japan in no way restricts the formal rights of employees to change employers. It is a management policy to minimize the discharge of regular employees in large firms; it is reinforced by the formal distribution of rewards according to age and length of service (*nenkō*). The *nenkō* wage system is, in effect, a mechanism of deferred wage payment, with younger workers being underpaid relative to their productivity and older workers overpaid. The economic rationale of this reward structure is that employees are expected to spend their work careers in one firm. That an individual remains with a firm for an entire career should not be confused with his subjective hopes and aspirations. Cole (1971:131-5) presents case study data showing that this performance often conflicts with individual aspirations for job changing. Marsh (1971) reports case study data showing the acceptability of the norms and values of job changing among employees; he rejects the view that permanent employment arises

from an employee's sense of moral obligation. These data suggest that examining the structural factors affecting availability of employment opportunities rather than assuming a mystical "tradition" is a more useful approach to decomposing the meaning of permanent employment.

The simplest measure of the different systems of employment security and different opportunity structure for inter-firm mobility in the United States and Japan is the ratio of employed persons changing jobs.⁴ This ratio is reported for the one-year period from 1965-1966. Table 1 presents the findings. The job change ratio for American females of 6.9 percent is only slightly higher than the ratio of 6.5 percent for Japanese females. Among males, however, the ratio of 9.9 percent for American males is more than twice as high as the Japanese ratio of 4.7 percent. Looking at age-specific job change ratios, we see that the greatest gap among males opens up in the 20-24 age category where the ratio in the United States is almost four times higher than in Japan; the smallest gap for males occurs from age 35-54 where the American ratio is reduced to less than twice as high as the Japanese ratio. These differences for males in overall change ratio and age-specific change ratios should not lead us to ignore basic similarities in pattern between the two societies. In both societies, the change ratio is highest for males 18-19 with the ratio undergoing gradual decline thereafter. Note that these data do not distinguish between voluntary and involuntary job changes.

In the United States, high voluntary quit rates are combined with high involuntary quit rates based on managerial prerogatives to adjust the labor force to changing business conditions; an institutionalized layoff system typifies this approach. The American practices are buttressed by a wage system which rewards productive performance. In a situation where employees might change employ-

⁸ Marsh and Mannari (1972:617-21) report some of the same data but then conclude that the distinction between *chūto saisyōsha* and school recruits is not a major source of support for the permanent employment system. Indeed, in a strange bit of alchemy, they suggest that it now mitigates toward increasing inter-firm mobility.

⁴ We should be cautious about seeing the United States as the industrial nation to be emulated if modernity is to be achieved. If we compare the labor turnover rates (a more indirect measure of job mobility) in manufacturing of Japan with England and West Germany, the Japanese rate appears to be only slightly lower (OECD, 1965:50). Perhaps students of Japanese conditions ought to be comparing their findings to the unique labor market tradition of the United States.

TABLE 1: --Rates of Change of Employment (comparison between Japan and the U.S.). Ratios of Employed Persons Changing Employers in a Year (in per cent)

Age Groups	Japan		U.S.	
	Male	Female	Male	Female
18 - 19	10.2	8.1	31.7	29.0
20 - 24	7.5	8.2	28.5	14.9
25 - 34	4.4	6.0	13.8	8.5
35 - 39*	3.3	4.7	7.4	5.3
40 - 54*	2.8	4.2	5.2	4.7
55 - 64	0.5	3.2	3.8	2.4
More than 65	0.3	0.3	2.7	1.8
Total	4.7	6.5	9.9	6.9

1. Source: Japanese data from Economic Planning Agency, 1968: 152. U.S. data is reported as U.S. Department of Labor: Monthly Labor Review for 1966.
2. Rates of change of employment are calculated as follows; As for the Japanese, the number of persons who changed jobs (persons whose job as of July 1965 was different from that of a year ago) divided by the total numbers of those who have not changed jobs and those who have changed jobs. As for the Americans, the ratio of persons changing jobs out of those who were employed both in January 1965 and January 1966.
3. Age groups marked * are 25-44 and 45-54, respectively, for the Americans.

ment at any time, it is not surprising that a system of deferred wage payment is unacceptable.

The existence of permanent employment in Japan confounds the expectations of economists and sociologists that high rates of job mobility are associated with advanced levels of industrialization (Kerr, et al., 1964:17-18). Some sociologists even describe the emergent "post-industrial society" precisely in terms of the temporary nature of individual participation in work organizations (Ben-nis and Slater, 1968). These expectations of high job mobility seemed based on the view that individuals as resources to be efficiently and rationally used by work organizations must be separable from these organizations under appropriate economic conditions. Such conditions are said to be present in advanced societies characterized as they are by rapid technological innovation and the need con-

tinually to adjust and transfer factors of production.

The stickiness of Japanese inter-firm mobility rates has led most observers to concentrate on the character of intra-generational mobility patterns in Japan. The practice seems to symbolize all the uniqueness that we have come to expect from Japan: enduring loyalty to the corporate group, a system of shared obligations, and strong employee dependence on powerful superiors. Scholars most associated with this perspective are James Abegglen (1958) and Nakane Chie (1970). They represent the historicist analysis of this phenomenon. Such analyses are incompatible with the generalizing goals of social science and ignore fundamental similarities with practices in other advanced industrial nations (to be discussed below).

In addition, Abegglen's discussion of the

permanent employment practice provides no basis for analyzing change over time for either historical or contemporary practices. Permanent employment is presented as an outgrowth of Japanese tradition and an adaptation to modern needs. Much doubt has been cast on Abegglen's historical treatment (Taira, 1962; Odaka, 1963; Sumiya, 1966; Yoshino, 1967; Cole, 1971a). Considerably more variation in mobility rates existed historically than can be accounted for by simple reference to the enduring strength of Japanese tradition. It is possible, in fact, to trace the sets of constraints and available resources which led Japanese industrial leaders in the inter-war period to select permanent employment as a solution to their needs and Japanese workers to accept these innovations (Cole, 1971a). In short, it is possible to depict the process of institutionalization whereby permanent employment gradually became established in selected firms and for selected employees to the exclusion of alternative arrangements. This was an adaptive response by employers and employees to economic, social, political and historical conditions.

Some scholars (e.g., Bennett, 1967) take the alternative position of convergence. They see rapid transformations in Japanese patterns of labor mobility. In particular, the shift from a labor surplus to a labor shortage economy is viewed as critical in driving up inter-firm mobility rates to the level of other advanced industrial societies. The nature of industrial composition also influences rates of inter-firm mobility. Turnover rates, size distribution of firms and internal promotion ladders vary widely by industry. As Japan shifts its industrial structure to patterns characteristic of other advanced industrial societies, it may be argued that the patterns of inter-firm mobility will move in the same direction. The enthusiasm of convergence advocates, however, is matched only by the lack of convincing data. I have discussed this problem in depth elsewhere (Cole, 1972). Here, it will suffice to note that despite massive shifts in industrial composition and marked changes in labor market relationships over the last twenty years, we can detect only modest changes in inter-firm mobility rates. This suggests that permanent

employment as crystallized in Japan has some significant and persistent strengths. Nevertheless, a useful research task for the future, when the appropriate data sets can be assembled, would be to systematically compare Japanese rates of inter-firm mobility to other nations, adjusting for industrial composition.

The remaining discussion will not deal with the historical evolution of this pattern nor with contemporary pressures for change. Rather, it will focus on present Japanese structural arrangements with their characteristically low rates of inter-firm mobility relative to the United States. The question is how may we best conceptualize the Japanese vis-à-vis the American arrangements.

As advanced industrial societies, both Japan and the United States support a complex division of labor with individuals ranged in a hierarchy of occupational skills. Consequently, both societies must continually motivate individuals to train for, occupy and perform these occupational roles. The rapidity of technological innovation in both societies with the rapid obsolescence of occupational skills means that these problems are not solved on initial entry to the labor force. Insofar as both societies are able to operate complex technologies free from labor constraints, we may see the structural arrangements regulating employment security and allowing differing opportunity for inter-firm mobility as functional alternatives. In using such alternatives, both societies motivate individuals so that their advanced technologies are kept operative.

The core of the permanent employment system is present in any ongoing industrial society. To be successful, all productive units try to reduce the costs arising from employee turnover. The greater the firm's investment in embodying specific training in individuals, the more important it becomes to reduce movement out of the firm; otherwise, recruitment, screening, training and termination costs would prove unmanageable (Becker, 1964). On the other side, workers develop psychological and economic stakes in their employment; labor market arrangements must guarantee some degree of employment tenure if they are to motivate workers. Employees tend to prefer some sys-

tem of internal replacement and upgrading since the opportunity costs of enterprise mobility generally exceed zero.

The elaborations on this core structure vary according to the political, social and cultural heritage of a given country and its industrial structure and labor market situation. The recent work of Piore and Doeringer (1971) helps conceptualize these differences.⁵ They examine internal labor markets of administrative units such as manufacturing plants. Within these units, administrative rules and procedures govern the pricing and allocation of labor. The internal labor market connects to the external labor market through job classifications which constitute ports of entry to and exit from the internal labor market. The remainder of the jobs, which are shielded from the competitive forces of the external market, are filled by the promotion or transfer of workers already employed.

In Japan, the ports of entry and exit and their traffic volume seem to be more limited than in the United States. Japanese rules defining the boundaries of internal markets and governing allocation of the work force are more rigid. Hence we can speak of more closed internal markets in the large industrial firms of Japan and more open internal markets in the United States. Piore and Doeringer (1971:6) associate rigidity in the internal market with investment in enterprise specific human capital, on-the-job training, and the role of labor as a fixed or quasi-fixed factor of production. These characteristics loom especially large in Japan (see Somers and Tsuda, 1966:195-236, esp. 207). The greater role of seniority in pricing and allocating labor in Japan also becomes understandable in this context.⁶ Rather than emphasize the uniqueness of *nenkō* and its basis in Japanese tradition, we may emphasize the importance of seniority to workers having less recourse to external markets.⁷

⁵ See also Dunlop (1966).

⁶ The more important role played by seniority in Japanese industry as compared to the U.S. has been pointed out by numerous observers. Marsh (1972:614-15) documents its greater importance in Japan than in the U.S.

⁷ George Taylor (1966:131-3) makes the same argument in explaining why seniority principles develop in some American industries and not others. He notes that seniority is not a part of the labor

Adapting Albert Hirschman's observations (1970), we posit that when "exit" is not a viable option for improving employee treatment, the "voice" option (working from within) is likely to be developed. Hirschman, using a competitive model of the business firm and focusing on the impact of consumer choice for improving product quality, argued for the dominance of the exit option and the relative insignificance of the voice option as adaptive mechanisms in business firms (Hirschman, 1970:76). My thesis, however, concerns the quality of employee treatment (e.g., role of seniority) and deals exclusively with the behavior of business organization members; it also recognizes that although a firm's product markets may be highly competitive, its labor markets may be less so. Marsh and Mannari (1972) note that a feedback mechanism operates whereby once the benefit of seniority is established, employee incentive to change employers is reduced. In Hirschman's terms, loyalty (induced by seniority benefits) makes exit less likely and develops voice. In summary, industries and nations vary markedly in the priority given to exit and voice. In large Japanese manufacturing firms, voice as manifested in the protection offered to employees by seniority takes priority over exit among regular male employees. In America, voice as manifested in seniority protection is also important in large manufacturing firms relative to the craft industries. But, compared to Japan, exit plays a more important role in American manufacturing.

The strength of the Japanese approach in crystallizing a practice of permanent employment for selected employees is that it places a high premium on the resource of employee commitment and the benefits that

management relationship in most craft unions, but it may be almost a fetish in some manufacturing firms. The craftsmen's skills are readily transferable to other employers; and, in many crafts, employment is casual with there being little chance for promotion. Craft employees, Taylor points out, like factory workers try to maximize job security; but they look to the market for job security rather than to a particular employer. In contrast, he points out that most employees in large scale manufacturing operations are not market oriented. Their skills are not readily transferable to different employers; and consequently, they try to maximize job security by maximizing the protection offered by seniority arrangements.

flow to the organization from mobilizing this commitment.⁸ By combining a system of deferred rewards (*nenkō* wage) with long-term service, loyalty to the firm and motivation to achieve formal organizational goals are maximized.

This approach works as a trade-off for the strengths of American practices. The American's high rates of inter-firm mobility provide for quick readjustment of labor pools, skills and costs to changing business conditions. The layoff system, modified to meet some worker interests, institutionalizes management prerogatives. High inter-firm mobility enables American organizations to mobilize external sources of trained labor at short notice.

The emphasis in Japan on recruiting at school graduation and retaining employees, reduces the flexibility of Japanese employers.⁹ But Japanese employers avoid the instabilities endemic in America, of high turnover rates, poor returns on training costs, high cost of recruiting and terminating and seemingly high levels of alienation.¹⁰

The Japanese real economic growth rate was about three times as high as the American rate from 1955 to 1972. This involved the enormous capacity of Japanese firms to absorb technological innovation. In a recent four year period (1966-1969), private equipment investment expanded at the remarkable average rate of 26 percent a year (Economic Planning Agency, 1970:15-16). Permanent employment practices have certainly been important in realizing this capacity through minimizing the dislocations and costs to individuals and business firms alike which result from high turnover rates. This success depends in turn on the highly developed in-plant training and retraining programs of Japanese firms. Peter Drucker (1971:110-22) suggests that American managers have much to learn from the Japanese about "continuous training" for employees.

⁸ Commitment, while undoubtedly never complete, involves both performing appropriate actions in a given social context and accepting the normative system that sets out the rules and their rationale (cf. Moore, 1965:40).

⁹ It also means that employers are likely to adjust prices downward rather than production when confronted with short-run problems.

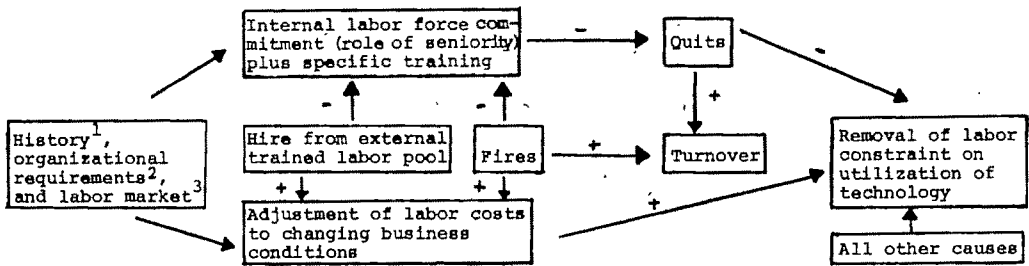
¹⁰ For an analysis on the American scene, see Stanley Lebergott (1968:122-7).

The permanent employment system avoids much "waste" associated with employee and union resistance to technological innovation deriving from threats to employment security, as has characterized the United States. Employees, relatively confident that they will be retained and retrained, can be expected to reduce their opposition to technological innovation. This is especially true with a wage system like Japan's, not directly occupationally based.¹¹ Similarly, employers can extensively train their employees with less fear of losing their investment through inter-firm mobility.

That permanent employment contributes to the acceptance of technological innovation might seem surprising. On economic grounds alone we would predict an inverse relationship. We would anticipate that high economic growth rates would lead to high rates of voluntary job changing, particularly in Japan with its high variance in sectoral growth rates. The latter leads to the expectation that high growth employers and industries could bid higher for desirable labor. That high rates of voluntary job changing do not occur in Japan suggests that more than a simple economic explanation of permanent employment is called for.¹²

¹¹ This thesis is easily exaggerated, however. Public opinion polls for example, show a less than favorable attitude by the Japanese public toward the impact of technology (Ishida, 1971:98). Cole (1972:92-100) reports case study data documenting employee restriction of production. Alternately, it has been noted in American industry that employment guarantees are associated with considerable managerial freedom to modify jobs and redeploy the internal labor force (Piore and Doeringer, 1971:57).

¹² The case being made in this paper might seem to be strengthened by comparing both nations internally, examining the relationship between industrial sector growth rates (or some measure of variability in demand) and job changing. Suppose we conducted a simple correlation analysis between rates of growth of industrial sectors and job change rates. The interpretation of a high or low coefficient is by no means obvious in terms of the formulations presented above. The coefficient would tell us something about the relationship between growth rates and job changing rates in the specific countries and about the internal rankings of job changing rates among industrial sectors. We would be able to see if the internal rankings were the same in the two nations, a not uninteresting question. But it would not explain the different *levels* of job change rates *between* the two countries. That is, clarifying the

FIGURE 2^a

a -- Conversations with Paul Siegel and Gary Saxonhouse were especially helpful in formulating this model. I bear sole responsibility for its final form.

1 -- Historical heritage of values and behavior including strength of primary group ties in organizational life.

2 -- Common need to make a profit and reduce replacement and termination costs arising from employee turnover.

3 -- Industrial and occupational composition, balance of labor supply and demand.

Figure 2 represents the thesis presented in this section.

The generalized model suggests that a firm's ability to remove labor constraints on the use of technology depends on two major inputs: first, the mobilization of high labor force commitment (e.g., minimizing quits, and alienation and maximizing loyalty to the firm), with a "capture" of the firm's investment in specific training; and second, the ability to adjust labor costs to changing business conditions and mobilize external sources of trained labor.¹⁸

In practice, it is difficult to mobilize high labor force commitment and adjust labor costs to changing business conditions simultaneously. Internal labor force commitment depends on assurance of employment security and adherence to the often strongly held value of employees that vacancies be filled from within the organization. But it is difficult, if not impossible, to maximize these conditions when management policy is to fire,

lay off and hire trained external labor. It is difficult to reconcile these two inputs. Maximizing one generally results in minimizing the other. The Japanese have opted for maximizing internal labor force commitment, and the Americans for maximizing the adjustment of labor costs to changing business conditions. Insofar as the choices made in both nations successfully remove labor constraints on the utilization of technology they serve as functional alternatives.

One may ask, of course, which alternative more efficiently removes labor constraints in the use of technology, or whether there are optimal trade-offs which perhaps neither country has achieved. Both questions deserve further investigation. It may be that as Japanese economic growth slows down, employers will need more flexibility in adjusting labor costs (i.e., the need will grow to increase involuntary discharges). Japanese employees particularly of large firms have greater employment security than Americans. With a strong seniority system, Japanese employees were more committed to their employers and less inclined to change them. The growing labor shortage in Japan, however, particularly of young employees, reduces internal labor force commitment. Partly in response to these changes, job-based wage systems are becoming larger components of industrial wage systems (Cole, 1971; Marsh, 1972; Yoshino, 1968). These

role of industrial sectors in explaining internal differences does not permit us to make inferences as to the causes of differences between systems. The issue is why job change rates in Japan are lower across all industrial sectors. The answer proposed in this paper is that we are dealing with an historical selection process that has operated on a nation's entire industrial structure.

¹⁸ The level of the labor force commitment important to the firm can be expected to vary with the nature of the technology in question.

developments heighten employee incentives to shop around. The shortage of new school graduates, particularly at the lower levels of the educational system, is intensifying as declining birth rates begin to be felt. Recruiting new school graduates for a lifetime career has been central to the permanent employment practice. But with the growing labor shortage, it is now predicted that school graduates will become a minority of new hires by 1975 (Japanese Productivity Center, 1972:111).

These developments suggest that the Japanese seem to be moving closer to the American model. For that matter, Robert Evans, Jr. (1971:45-93) suggests that over time Americans may be moving closer to the Japanese. Notwithstanding, we can not extrapolate these trends—to the extent that they exist—to predict complete convergence of the two systems. The institutionalized basis for the permanent employment practice in Japan lies in its high degree of legitimacy and the commitment of powerful social actors to its maintenance. In contemporary Japan, employers are at best ambivalent about the rewards associated with discarding the practice; and union leaders and employees seem committed to limiting management prerogatives with respect to discharge (and to expanding employee rights to change employers).

In summary, there is some evidence for partial convergence as well as persisting evidence that the different structural arrangements are functional equivalents. This should not be surprising. It is unlikely that one society's complex structural pattern will have consequences completely equivalent to those of a pattern in a different society. Permanent employment in Japan seems to have quite different consequences for worker motivation than do employment relationships in the United States.

Much work remains in clarifying the positions outlined above. To what extent do market factors influence the rates of job changing in the two nations? To test a market interpretation would require a comprehensive model of job changing including employer and employee decisions and voluntary and involuntary separations in the context of economic and sociological variables.

REFERENCES

- Abegglen, James
1958 *The Japanese Factory*. Glencoe, Illinois: Free Press.
- Aberle, D. G., A. K. Cohen, M. J. Levy, Jr. and F. X. Sutton
1950 "The functional prerequisites of a society." *Ethics* 60(January):100-11.
- Ballon, Robert
1969 *The Japanese Employee*. Rutland, Vermont: Charles Tuttle.
- Becker, Gary
1964 *Human Capital*. New York: National Bureau of Economic Research.
- Bellah, Robert
1957 *Tokugawa Religion*. Glencoe, Illinois: Free Press.
1963 "Reflections on the Protestant ethic analogy in Asia." *Journal of Social Issues* 19(January):52-60.
- Bendix, Reinhard
1963 "Concepts and generalizations in comparative sociological studies." *American Sociological Review* 28(August):532-9.
1964 *Nation-building and Citizenship*. New York: Wiley.
- Bennett, John
1967 "Japanese economic growth: Background for social change." Pp. 411-53 in Ronald Dore (ed.), *Aspects of Social Change in Modern Japan*. Princeton: Princeton University Press.
- Bennis, Warren and Philip Slater
1968 *The Temporary Society*. New York: Harper and Row.
- Boulding, Kenneth
1970 *A Primer on Social Dynamics*. New York: Free Press.
- Cole, Robert
1971 *Japanese Blue-Collar: The Changing Tradition*. Berkeley: University of California Press.
1971a "The theory of institutionalization: permanent employment and tradition in Japan." *Economic Development and Cultural Change* 20(October):47-70.
1972 "Permanent employment in Japan: facts and fantasies." *Industrial and Labor Relations Review* 26(October):615-30.
- Coleman, James
1969 "Race relations and social change." Pp. 274-341 in Irwin Katz and Patricia Gurin (eds.), *Race and the Social Sciences*. New York: Basic Books.
- Doeringer, Peter and Michael Piore
1971 *Internal Labor Markets and Manpower Analysis*. Lexington, Mass.: D. C. Heath.
- Drucker, Peter
1971 "What we can learn from Japanese management." *Harvard Business Review* (March-April):100-22.
- Dunlop, John
1966 "Job vacancy measures and economic

- analysis." Pp. 27-47 in National Bureau of Economic Research (ed.), *The Measurement and Interpretation of Job Vacancies: A Conference Report*. New York: Columbia University Press.
- Dunning E. G. and E. I. Hopper
1966 "Industrialization and the problem of convergence: a critical note." *Sociological Review* 14(July):163-86.
- Economic Planning Agency
1968 *Economic Survey of Japan (1967-1968)*. Tokyo: The Japan Times.
1970 *Economic Survey of Japan (1969-1970)*. Tokyo: The Japan Times.
- Evans, Robert, Jr.
1971 *The Labor Economies of Japan and the United States*. New York: Praeger.
- Feldman, Arnold and Wilbert Moore
1969 "Industrialization and industrialism: convergence and differentiation." Pp. 55-71 in William Faunce and William Form (eds.), *Comparative Perspectives on Industrial Society*. Boston: Little Brown.
- Frank, Andre Gunder
1969 "Sociology of development and underdevelopment of sociology." Pp. 21-94 in Andre Gunder Frank (eds.), *Latin America: Underdevelopment or Revolution*. New York: Monthly Review Press.
- Gerschenkron, Alexander
1964 *Economic Backwardness in Historical Perspective*. New York: Frederick A. Praeger.
- Goldthorpe, John
1966 "Social stratification in industrial society." Pp. 648-59 in Reinhard Bendix and Seymour Martin Lipset (eds.), *Class Status and Power*, second edition. New York: Free Press.
- Hempel, Carl
1959 "The logic of functional analysis." Pp. 271-307 in Llewellyn Gross (ed.), *Symposium of Sociological Theory*. New York: Harper and Row.
- Hirschman, Albert
1965 "Obstacles to development: a classification and a quasi-vanishing act." *Economic Development and Cultural Change* 13 (July):395-8.
- Hirschman, Albert
1970 *Exit, Voice and Loyalty*. Cambridge: Harvard University Press.
- Hodge, Robert, Donald Treiman and Peter Rossi
1966 "A comparative study of occupational prestige." Pp. 309-21 in Reinhard Bendix and Seymour Martin Lipset (eds.), *Class, Status and Power*. Second edition. New York: Free Press.
- Homans, George
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace and World.
1967 *The Nature of Social Science*. New York: Harcourt, Brace and World.
- Inkeles, Alex and Peter Rossi
1956 "National comparisons of occupational prestige." *American Journal of Sociology* 61(January):329-39.
- Inkeles, Alex
1965 "The modernization of man." Pp. 138-50 in Myron Weiner (ed.), *Modernization*. New York: Basic Books.
- Ishida, Takeshi
1971 *Japanese Society*. New York: Random House.
- Japan Productivity Center
1972 *Katsuyō Rōdō Tokei (Practical Labor Statistics)*. Tokyo: The Center.
- Kerr, Clark, John Dunlop, Frederic Harbison and Charles Meyers
1964 *Industrialism and Industrial Man*. New York: Oxford University Press.
- Kuznets, Simon
1968 "Notes on Japan's economic growth." Pp. 385-422 in Lawrence Klein and Kazushi Ohkawa (eds.), *Economic Growth: The Japanese Experience Since the Meiji Era*. Homewood, Ill.: Richard D. Irwin.
- Lebergott, Stanley
1968 "Labor force and employment trends." Pp. 97-143 in Eleanor Sheldon and Wilbert Moore (eds.), *Indicators of Social Change*. New York: Russell Sage Foundation.
- Levy, Marion, Jr.
1966 *Modernization and the Structure of Societies*. Princeton: Princeton University Press.
- Mallinowski, B.
1926 "Anthropology." *Encyclopedia Britannica*, First Supplementary Volume. London and New York: The Encyclopedia Britannica, Inc.
- Marsh, Robert and Hiroshi Mannari
1971 "Lifetime commitment in Japan: roles, norms and values." *American Journal of Sociology* 76(March):795-812.
1972 "A new look at 'lifetime commitment' in Japanese industry." *Economic Development and Cultural Change* 20(July):611-30.
- Merton, Robert
1957 *Social Theory and Social Structure*, revised edition. New York: Free Press.
- Moore, Wilbert
1965 *The Impact of Industry*. Englewood Cliffs, New Jersey: Prentice-Hall.
- Nagai, Michio
1971 "Westernization and Japanization: the early Meiji transformation of education." Pp. 35-76 in Donald Shively (ed.), *Tradition and Modernization in Japanese Culture*. Princeton: Princeton University Press.
- Nakane, Chie
1970 *Japanese Society*. Berkeley: University of California Press.
- Nisbet, Robert
1969 *Social Change and History*. New York: Oxford University Press.
- Odaka, Kunio
1963 "Traditionalism, democracy in Japanese industry." *Industrial Relations* 3(October):95-103.

- Office of the Prime Minister
 1970 *Nihon no Shūgyō Kōzō*: Showa 43 (Employment Structure of Japan: 1968) Tokyo: Bureau of Statistics.
- Olson, Mancur
 1970 "An analytic framework for social reporting and policy analysis." *The Annals of the Academy of Political and Social Science* 388(March, 1970):112-26.
- Organization for Economic Cooperation and Development
 1965 *Wage and Labor Mobility*. Paris: OECD.
- Popper, Karl
 1957 *The Poverty of Historicism*. New York: Harper and Row.
- Riggs, Fred
 1966 *Thailand: The Modernization of a Bureaucratic Polity*. Honolulu: East-West Center Press.
- Shils, Edward
 1963 "On the comparative study of new states." Pp. 1-26 in Clifford Geertz (ed.), *Old Societies and New States*. New York: Free Press.
- Somers, Gerald and Masumi Tsuda
 1966 "Job vacancies and structural change in Japanese labor markets." Pp. 195-236 in National Bureau of Economic Research (ed.), *The Measurement and Interpretation of Job Vacancies: A Conference Report*. New York: Columbia University Press.
- Stinchcombe, Arthur
 1968 *Constructing Social Theories*. New York: Harcourt, Brace and World.
- Sumiya, Mikio
 1966 "The development of Japanese labor relations." *The Developing Economies* 4 (December):499-515.
- Taira, Koji
 1962 "Characteristics of Japanese labor markets." *Economic Development and Cultural Change* 10(January):150-68.
- Taylor, George
 1955 "Seniority concepts." *Arbitration Today*. Proceedings of the National Academy of Arbitrators. Washington: Bureau of National Affairs.
- Tilly, Charles
 Forth- "Postscript: western statemaking and coming theories of political transformation." In Charles Tilly (ed.), *The Formation of National States in Western Europe*. Princeton, New Jersey: Princeton University Press.
- Weinberg, Ian
 1968 "The problem of the convergence of industrial societies: a critical look at the state of a theory." *Comparative Studies in Society and History* 10(October):1-15.
- Whitehill, Arthur, Jr. and Shin'ichi Takezawa
 1968 *The Other Worker*. Honolulu: East-West Center Press.
- Yoshino, M. Y.
 1967 *Japan's Managerial System*. Cambridge: The MIT Press.

EDUCATION AND MOBILITY: FROM ACHIEVEMENT TO ASCRIPTION *

BARBARA JACOBSON

Herbert H. Lehman College of City University
of New York

JOHN M. KENDRICK

Yale University

American Sociological Review 1973, Vol. 38 (August):439-460

Modernization literature uses the criteria of ascription and achievement to distinguish pre- and post-industrial stratification systems. Analysts usually consider the development of systems of mass education and changes in the division of labor as the chief structural sources of such changes. However, sequences of such change do not necessarily recapitulate these contrasts. If the study of sequences is placed in the forefront of social change research, the life cycle point appropriate for the distinction between ascription and achievement becomes problematic. Moreover, the emphasis on temporal order derived from the stress on sequence suggests that situationally anchored cohort studies of intra-generation mobility are better for studying certain aspects of these processes than inter-generational mobility studies. A series of propositions are advanced contrasting socio-historical and biological explanations for changing mobility streams associated with education. The propositions are then applied to the experiences of three cohorts in Puerto Rican development. We conclude by noting the following. (1) Certain education related changes increase ascriptive characteristics by transferring some work promotion criteria out of the workplace into the classroom. (2) Like all dichotomies, the contrast between ascription and achievement is much too simple. It does not incorporate the multiple sequences of change, nor consider their direction problematic.

ONE of the most familiar propositions in sociology concerns the relationship between industrialization and social mobility; viz., as societies industrialize, changes in technology and work organization combine with a transfer of a status inheritance mechanism from family to school systems to increase social mobility, particularly occupational mobility. This change of the institutional locus of stratification placement is usually conceived as normative, a change from "ascriptive" to "achievement" criteria for placement (Levy, 1966:624-34; Moore, 1965:98-100; Parsons, 1951:180-200; Smelser and Lipset, 1966).

Though the support for this proposition derives from comparisons between non-in-

dustrial and industrial societies, it is, typically and erroneously, generalized to account for the process of change as well. This generalization is widely accepted but unsupported by the only observations appropriate: careful historical studies of change processes and the role of education in them (Stone, 1969; Talbott, 1971; Thompson, 1963: 711-45). This essay examines this process by analyzing longitudinal data for Puerto Rico on these aspects of stratification change.

The distinction between ascription and achievement criteria usually refers to the extent to which children inherit their parents' social status. The data for such comparisons come from comparative studies of inter-generational social mobility. By contrast, studying change processes alters the data requirements. Studies of intra-generational occupational mobility are more appropriate for analyzing these changes. The methodological assumption in which comparison equals change, space substitutes for time, comes from the research strategy of comparative statics, in which factors of change are problematic and sequences non-problematic. Additionally, we shall conceive of change as a series of points temporally ordered and probabilistically related, rather than as a simple binary contrast. When we

* The order of names is deliberately alphabetical. Our respective contributions are neither senior nor junior, large nor small. Support for this research came from the National Institute of Mental Health, the University of Puerto Rico, and the Center for Intersocietal Studies at Northwestern University. An earlier version of this paper was read at the American Sociological Association Meetings in Denver, Colorado in August, 1971 under the title "Cohorts, Modernization, and Social Mobility." We wish to thank Wendell Bell, Allan Schnaiberg, and an unidentified referee for detailed comments on earlier versions and Arnold Feldman for advice, assistance and stimulation.

apply this concept to the contrast between ascription and achievement, studies of work-life mobility, or career as Everett Hughes suggests (1971), enrich the social meaning of the contrast. Finally, such a conception places a high priority on ordering events correctly and, thus, on dating events with some precision. For reasons to be discussed, studies of work-life mobility meet this requirement better than those of inter-generational mobility.

Our conception permits the following portrait of work-life mobility in Puerto Rico during its recent period of intensive economic development. In at least one respect, the sequences are the reverse of those posited by the static comparisons; instead of enhancing a transition from ascription to achievement, some educational processes reverse the sequence. *We suggest that the wrenching of the status transmission processes out of the family and into the school is paralleled by a similar displacement of status career assignment processes out of the work setting and into the school.* Thus, the classic liberal assumption—built into such dichotomies—that education gives individuals increased control of their fate is not borne out.

WORK HISTORIES AND SOCIAL CHANGE

Factors and Sequences

The basic model for the study of modernization or industrialization is a "transition" model. Research proceeds by attempting to specify "factors" which account for the transition (Moore, 1963:40; Moore and Feldman, 1960). This procedure appears to be a model of scientific economy. Researchers specify beginning and end points of change such as tradition and modernity or being non-industrialized and industrialized. The beginning points of each are then searched for factors which account for the successful transitions of one as opposed to another society. Such factors as the presence of certain motivational and ideological systems or the simple mobility of resources, including labor, are advanced as explaining why these societies "took off." The same procedure can be used to account for a transition from ascriptive to achievement stratification systems. Here, the most frequent explanations

offered are factors which account for the introduction of systems of mass education.

Although the logic is familiar, the metaphor may not be. Nisbet argues that this logic depends on the acceptability of the growth metaphor (1969) which treats change as the unfolding of features present in the origin state: the seed determines the flower. The metaphor then yields models of change in which the sequence of a move from one position to another—i.e., from tradition to modernity—simply recapitulates the contrast in a linear fashion through time and in a homogeneous fashion across population segments.

An inverted version of this metaphor also persists. This version assumes that factors present in the institutional structures of industrial societies constrain social change in the same direction. As Inkeles (1960) observes, industrial societies tend to converge. Features of the end point dictate the transition. The assumption of equifinality, as Geussous (1967) terms it, reverses the metaphor: different types of seeds may produce the same flowers. However, it shares the assumption that factors specified at either the beginning or end control the transition.

The chief shortcoming of these models for the study of historical change is the assumption that time is an irrelevant formal dimension. Researchers need not refer to the temporal order of events to account for change. However, factor language is causal language and causal language requires that the temporal order of variables be correct. Thus, temporal order is necessary to explanations of change. To move in this direction, however, is to step into largely unexplained terrain. We use the concept of sequences as a framework for what follows (Moore, 1970; Kubler, 1962), principally because it conforms to the requirement of temporal order. Moreover, historical change is no longer conceived as sets of binary contrasts but as a series of points, temporally ordered and probabilistically related. Accounting for historical change becomes a process of creating and relating sets of these sequences.

Sequence and Work Life Mobility

Moving from a transition model to a sequence model encourages research on the

role of education in stratification change. Temporal order requires dating the elements in analysis as closely as possible (Ryder, 1965). This dating makes the conventional inter-generational comparisons less than adequate. The issues are familiar and need only be listed. Comparisons of children's and parents' positions are temporally irregular. First, many children are as old as many parents (Duncan, 1966:56-63). This means that variation existing in the mode of stratification assignment over the age range of parents or children does not appear in the analysis. Thus in comparing changes in inheritance patterns based on a sample of children collected in the United States in, say, 1840 with one collected in 1940, one would be unable to date the critical age points for status transmission in each sample. Even standardizing parents or children in cohorts would prove difficult, assuming the relevant information existed. One needs to be able to standardize both, since the span between the latest and earliest ages at which parents give birth to children is so great.

This paper meets the temporal requirement with an analysis of intra-generational occupational mobility which stratifies cohorts by educational performances. Samples can be organized to insure an age range variation wide enough to permit comparisons of some temporal reach. And the cohort's standardizing mechanism assures the analyst of homogeneous age and historical location within cohorts and heterogeneity across cohorts. (This point makes some assumptions about the quality of certain kinds of retrospective data which we will treat later). Intra-generational mobility studies organized in a cohort fashion also make separating education and occupational change possible. Comparisons across cohorts of similar educational levels answer the question of whether occupational change changes the mobility stream for given levels of education. Finally, intra-generational mobility studies provide an opportunity to separate those mutually bedeviling explanatory problems—aging and historical change—if the characteristics of age that indicate different biological and historical location are exploited to their fullest (Ryder, 1965). Previous uses of inter-generational mobility studies have tended to mute, if not ignore, such problems (Blau and

Duncan, 1967). In the following essay we address ourselves to these methodological issues—separating education and occupational changes and separating aging and historical effects.

Work Life Mobility: Ascriptive or Achieved?

Ascription and achievement may seem unpromising concepts for organizing and interpreting work life mobility. We shall, however, use the terms in a slightly unfamiliar way. The argument, in abbreviated form, is as follows. Much literature on industrialization and modernization suggests that the locus of status transmission moves from the family to the school as societies industrialize. A seldom mentioned concomitant of this change is the growing use of educational certification requirements for employment and promotion (Miller and Roby, 1970; Berg, 1970). Hence, the locus of criteria for work life mobility moves from the work place to the school room. The activities of the work place become less important for jobs than attributes of schooling. This change replaces achievement criteria with ascriptive ones.

Talcott Parsons put the ascription/achievement contrast to many but consistent uses (Mayhew, 1968). Two are important for interpretations of work life mobility. The first and most familiar was to describe criteria of stratification placement; the second and less familiar refers to the reproduction of structural patterns across functional contexts. As formulated by Parsons and used by Smelser and Hoselitz, among others, an ascription/achievement dichotomy is first concerned with the modalities of social objects; in the case of stratification processes, it concerns characteristics of social actors relevant for stratification placement. The distinction for Smelser (1966:8) is between stratification placement based on birth and placement based on some sort of behavioral performance. Hoselitz (1963) distinguishes between who a person is and what he can do. The difficulty with such a formulation is that it attempts to account for two simultaneous but inherently different processes. First, it points to personnel practices critical to the sorting process—employers and their agents—and specifies characteristics of pros-

pective employees to which they respond. Second, it points to changes in the degree to which members of the labor force control their mobility. It is to this second aspect we direct attention. The structural changes of industrialization and modernization are alleged to place the individual more in control of his social fate. Instead of assignment on the basis of characteristics over which he has little or no control, he can now achieve positions previously unattainable. This conception of equality is embodied in the phrase "equality of opportunity." Thus, education is assigned a critical role in the theoretical development of the schema. As Clark Kerr epigrammatically puts it, "it conduces toward a new equality which has nothing to do with ideology" (Kerr, 1960).

Such statements do not make clear the life cycle specifics of ascription and achievement. Certain elements are clear. On the one hand, all the accidents of birth from biological constitution to social station are subject to ascription, in the sense that they are beyond the individual's control. On the other hand, most adult decision making in everyday life, while constrained within social and psychological parameters, is controlled by the individual. As Jean Paul Sartre notes, one always has the freedom to say no.

But where in the life cycle does achievement begin, and how does it relate to ascription? That birth is not an invariant point for such distinctions is apparent in another commonplace, the role of the family in educational placement and achievement and, thus, in assignments to occupational position. But this problem is not as telling as the cognitive implications of terms like "control." It implies that as students make decisions about how long to remain in schoolrooms, how hard to work, and what curricula to select, they know the occupations they want as well as how much and what sorts of education these require. This might possibly be the case in a settled industrial society in which the salience of education for later life was well understood by parents who, in turn, communicated this fact at a reasonably early age to children. However, in a society undergoing the rapid demographic, economic and normative changes usually associated with industrialization, in which certain jobs appear for the first time, the very criteria of

achievement are in flux. In such a society early and accurate knowledge is highly unlikely.

Relaxing the assumption that birth is the point at which the ascription/achievement dichotomy applies permits the elements of ascription to be moved forward in the life cycle. Certainly any attempt to establish some new invariant point would be foolish—too little is known and too much is variable (J. M. Tanner, 1971). But introducing certification requirements into an existing labor force, educated when or where information about certificates was non-existent or ignored, makes of education, for persons already in the labor force, a characteristic over which they have no control. Employers and their agents do begin to respond to different characteristics, but not to characteristics which permit certain workers to gain more control over their mobility prospects. To fit our point into Hoselitz' phrases, it is who they are (how much education) and not what they do (how well they do their work) that becomes the central sorting principle. This principle is a consequence of one of the rarely emphasized elements of industrialization: the displacement of the achievement criteria for work mobility out of the setting of work itself into the classroom.

The second aspect of the ascription/achievement dichotomy refers to structural reproduction. This aspect represents a generalization of the first use. In that use certain biological or group attributes of the person determined stratification placement. It also means that structural patterns in one institution are reproduced in another (Mayhew, 1968:113). Stratification across families may be reproduced in the polity, the economy, etc. More concretely, it may be thought of in terms of the definition of labor pools. An employer with a job opening does not fill it by canvassing every available candidate. This process is not universalistic. Rather, in the interests of convenience he takes advantage of certain particularistic criteria such as friendships of present employees, firms from which he has earlier recruited, and the like. When educational certification becomes such an attribute of convenience, patterns of school attendance are reproduced in work. Resources become ascriptively linked.

EDUCATION, WORK LIFE MOBILITY, AND
SOCIAL CHANGE

The view that modernization creates areas of social life in which ascription increases appears in fragments in the sociological literature. In this section we paste these fragments into a set of propositions. These propositions refer to historical and life cycle changes and provide means to test for the differences.

Historical Change and Mobility

J. H. Goldthorpe (1966) argues that the tightening links between educational and occupational achievement in the course of industrialization may not only foster more inter-generational mobility but also decrease the prospects for upward mobility once the work life is begun. He notes that those who start with little education may discover their upward mobility chances progressively diminished as promotion criteria become more tightly linked to educational performance than to work performance. Goldthorpe indicates that this may even be the case at upper as well as lower strata. He draws on studies done during the 1950's on industrial managers. As educational standards for managers moved upward, the chances of shop workers moving into management appear to decrease. Thus, depending on the sequences of changes in education and occupations, workers get whipsawed. The nature and levels of the credentialling process may change such during their work life that the element which seemed to be a mobility mechanism early in life—educational achievement—becomes an ascribed barrier later in life.

If we distill Goldthorpe's observation into a proposition in which we separate historical and life cycle effects, we get the following statement: *as workers age, the effect of their educational attainment on occupational careers changes contingent on the increase of credentialism in the occupational structure and the amount of education they have.* Thus, the following variables, all of which may be interactively related, determine occupational careers: educational change, aging, and occupational change. This proposition suggests that mobility experiences do not vary consistently with age or education levels but with how these two interact with

historical changes. It predicts that if individuals of the same age and education are compared, they will have different mobility patterns if they reach these age points at different historical times.

Aging, Education, and Mobility

Other statements related to these same processes require the opposite correction: the addition of historical change. All propose to relate changes in occupational paths to aging or to education and aging. They consist of three essentially linear propositions: (1) the effect of education becomes increasingly stronger as age related processes accumulate; (2) the effect of education weakens; and (3) the effect of education does not change through the work life.

Joseph Kahl (1968:55), in a comparative treatment of Brazilian and Mexican data, suggests the first possibility: the controlling effect of education increases as occupational careers lengthen and actors age. His reasoning is as follows. Selecting a first job has many elements of chance in it that are overcome as a person's occupational life progresses. The first job may be the product of such factors as the short term state of the local economy, the information channels of the moment, and friendship patterns. The longer term career pattern, however, washes out such short term fluctuations. Therefore, to put this proposition in the language of analysis of variance, *the range of occupations within any level of education should contract as workers age, and the constriction should be less within levels of first job than across levels of education.*

Kahl's explanation is an alternate to Goldthorpe's. He notes the same process: education's increasing circumscription of mobility. However, in Kahl's formulation, this process occurs as individuals age; in Goldthorpe's as the social fabric changes. Both formulations use time differently.

Peter Blau and Otis Duncan (1967) state the reverse of this position. They argue that the effect of education weakens as work lives lengthen. They reason that the host of factors which determine promotions, lateral moves, and demotions in the work setting should accumulate. This accumulation should diminish the role of education in occupa-

tional movement. Thus, *within each education level, the spread or range of occupations should expand as these careers lengthen, and that expansion should be greater across education levels than within first job levels.*

This argument implies an opening up process with respect to education. The ascriptive character of education decreases as factors specific to the work setting become more central to mobility.

Our last proposition comes from the summary of some cohort literature on aging by Gosta Carlsson and Katarina Karlsson (1970).

They advance a fixation model for aging, one which also appears in Norman Ryder's (1965) paper on cohorts and social change. The model is best summarized in Ryder's language: "from amorphous placidity through technical competence to terminal rigidity." The proposition underlying it is simple: with increasing age people become less likely to change. And it is important. If correct it would reduce the realm of problematics in the study of social change dramatically. It asserts that each cohort, in its late life, reflects only the conditions prevailing during its earlier years. Conditions present later in life may be ignored. In effect, the proposition argues that social changes are displayed only in the lives of the young.

This final proposition reduces the argument to its simplest form: *As careers lengthen mobility decreases.*

While these four propositions suggest various forms of a relationship, they possess several shortcomings. First, they do not take account of the directionality of mobility. The expansion or reduction of variance involved has no necessary relationship to the upward and downward movement of subjects. Most of the concern, however, with the effects of education on work lives centers on the reduction in upward and/or the increase in downward mobility. Second, education is held constant in a model in which it is the independent variable. That is, the level of education is not considered problematic. These propositions all assume that, regardless of the amount of education received, these relationships hold. Third, these last propositions assume that changes in the social setting, while possibly changing the mechanisms of explanation, do not alter the form of the

relationship. Kahl and Blau and Duncan basically presuppose an industrial society. The Blau and Duncan data are American; Kahl's samples come from the industrialized sectors of Brazil and Mexico. In each case, while their language reaches for the more general case, their explanations remain specific to an advanced industrial social order. Carlsson and Karlsson's proposition is not specific to any social order; however, they consider it applicable in changing and non-changing, industrial and non-industrial societies.

SETTING AND SAMPLE

The setting for this research, is the island of Puerto Rico and the period under examination is that of its rapid industrialization. Three aspects of this setting are important for this analysis: (1) the rate and kind of change in the educational system; (2) the kind of change in the occupational structure of the island; and (3) the way in which the sample relates to these two previous characteristics.

That the island underwent sharp economic changes following World War II is reasonably well known, but what is not so well known are the changes through which the education system passed. The basic characteristics of the expansions of the public school system during this period were two-fold: an increase in the democratization of access with little change in the quality maintained in the class room (Sussmann, 1968). A few selected statistics indicate the first, the extent of democratization of access between 1940 and 1960. The enrollment ratio for those age 18-19 went from 8.7% in 1940 to 29.5% in 1960; the enrollment ratio for those age 7-13 went from 66.8% to 83.7%; the proportion of illiterates amongst persons ten years of age or older went from 31.5% to 17%. Second, the quality of education was something of a constant during and prior to this period, perhaps as far back as 1928. This absence of change permits one simplifying assumption: we can ignore the possibility that the same amount of education for a later cohort represents a significantly better education.

Changes in the occupational structure after World War II are important. The effect of

an expanding educational system on the work lives of those workers already in the labor force depends on the rate and kinds of expansion or contraction of the occupational structure. It is impossible to bring to this question the sort of precision one would want. Not only are the statistics not collected nor designed for such purposes; but they require some means of integrating the peculiar mix of educational output, occupational input, and migration characteristics of the island during this period. Two matters are clear, however. These latter two factors—occupational change and migration—had opposite effects. Migration, of course, eased the supply of workers considerably—despite an unemployment rate that never dropped below 10%; but changes in the occupational structure indicate a growth in those work settings, likely to employ certification requirements—manufacturing and government, coupled with a growth in those skill activities likely to require certification.

The question of the characteristics of the sample is peculiarly important for the kind of questions we have outlined in the preceding pages. Since these focus on explanations of temporal changes, the sample problem is difficult. Moreover, the analysis is a secondary one. The sample was not constructed with these questions in mind. Nonetheless, it has peculiar characteristics which make its use in the present context particularly appropriate.

The data come from a two wave panel survey of the Puerto Rican labor force. The original sample was a stratified—by education and residence—random sample of the Puerto Rican labor force in 1953, re-interviewed in 1966–1968. The first sample consisted of just over 1,000 cases, the analysis of which appears in *Social Class and Social Change* by Melvin Tumin with Arnold Feldman (1961); the re-interviewed sample consists of 669 respondents, the total of the previous 1,000 who were alive and had not migrated. This analysis is based on the re-interviewed sample of 669.

This kind of sample provides certain benefits for such an exploratory analysis as this. First, the sample includes all elements of the Puerto Rican labor force in 1954, and its experiences can be said to represent the defining education and residence units as they

moved through the island's history. Second, the re-interview in 1966 provides a long enough occupational history for the history to have been affected by changes introduced after 1945. It does not, however, constitute random samples of the three cohorts we will introduce at any point in time, not when they entered the labor force—an impossible task; nor in 1954—for the sample was not constituted in terms of age.

DATA ANALYSIS

This analysis stresses issues of change rather than statics, separates life cycle phenomena from historical ones, biography from social structure. Such an analysis requires a multivariate strategy. We shall, however, avoid the contemporary use of "partials." Our reasons are numerous. Chiefly, we are unconvinced the mathematicians among us provide instruments properly subservient to theory construction, at least of the variety which interests us. They tend, instead, to provide instruments with a logic of their own, ones which assume an a priori coherence of mathematical logic and social patterns. One may conclude that the instruments for comparative statics are reasonably numerous, rich, and, perhaps, even useful; but those which include temporality in the model are either underdeveloped, too restrictive in their assumptions, or have yet to issue from the workshop. In the absence of such, we shall stress an analysis strategy which remains simultaneously sheathed in the theory and close to the intricacies of the data.

Second, the aim of much statistically sophisticated multivariate work is to reduce a large number of variables to the smallest possible. Our own aim is different. It is to search for differentiating elements in such a way as to highlight them.

Third, we prefer a data analysis strategy which stays as close as possible to the vagaries of the data, to the experiences of respondents in our case. We are not denying the inevitable status of models of some sort in research; we are simply placing ourselves under the constraints of a rather familiar norm in sociological research—to respect the integrity of the data. This decision places the weight of the analysis on

the design; not on the mathematics. Only in such a fashion, we are convinced, do statistics remain the servant of the analyst.

The aim of the following design is to incorporate as much of the temporal into the analysis as possible. The ingredients for the design are dual and will be treated in the following order: first, the usual statement of the variables and the way in which we constitute them for the analysis; and, second, the logic of the analysis or, more concretely, the steps through which we move and why we do so. We turn first to the variables.

The Variables

Constructing the basic independent variable for the analysis involves a familiar procedure. We constructed a twelve category "interaction variable" which combines two variables: social stratum of the respondents and historical location. This procedure permits assessing the relative impact of history and/or aging on various social strata in a non-linear fashion. We use education, broken into four categories, as the stratum variable. And a cohort variable as the index of historical location. On this latter variable respondents are scored in terms of the calendrical point they entered the labor force—before 1918, between 1918 and 1931, and after 1931. This scoring, of course, positions the respondents in terms of the occupational structure at the time of their entry into the labor force and ignores changes in that structure which are specific to the cohort as it moves through its occupational life. These we shall measure in another fashion.

The educational variable also contains a temporal component. For reasons we shall presently explain, we slice into the histories of our respondents at three times—1944, 1954, and 1966. All respondents completed school before 1944. However, some, like veterans, either returned to school between 1944 and 1966 or gave different reports about their education in the moves of the panel. In some cases these differences moved respondents from one education category to another. We considered report differences between the two waves as representing validity, not reliability problems (Jacobson and Kendrick, 1970). As a result, we did not try to reconcile differing reports.

The dependent variable for this analysis is the work history of respondents coded in terms of the skill level of each job held by respondents. Chart 1 shows the twelve groups into which we coded these histories.

We constructed these twelve skill history categories according to the following three guidelines. Each category must (1) provide proper attention to the general direction of the entire career and permit a decision concerning the precariousness of the last position; (2) it must be sensitive enough to register mobility changes between the three historical points in the analysis, yet to be described; and (3) it must acknowledge the problems of retrospective data.

In accord with the first guideline, the code includes the modal and end points in the work histories, the sequences in which the more durable points were occupied, and the durations of these points. It does this in the following manner. The first decision point involves the extent to which the end point is characteristic of the career. If the career involves time in other skill positions, the coding decision is made on the basis of what other positions and the percentage of the history involved. Thus, four of the scores reflect the extreme durability of the score at the end point: the always unskilled (group 1), the semi-skilled (group 5), the skilled (group 10); and the white collar (group 12). The second grouping of scores reflects the fact that most of the career was spent above the end point position and is divided into just how large a portion of the career it involves. It represents, of course, the various downwardly mobile groups. The final general grouping involves the same decisions regarding the upwardly mobile respondents.

This procedure provides several advantages. By ranking our scores by the skill level at the end point first and by the direction of the move into that position second, we accomplish two ends. First, we create a variable which accords best with our theoretical questions. It retains the individual history as well as the stratum levels at which that history moves; thereby avoiding the Scylla of simple classifications of movement between two points and the Charybdis of assuming a co-incidence between the end and the path of a work history. Second, it preserves a modicum of comparability with other

Chart. 1. Categories of Skill Histories

1. Always in unskilled labor.
2. Downwardly mobile into unskilled labor just prior to interview; most of work life spent in unskilled labor.
3. Downwardly mobile into unskilled labor just prior to interview; most of work life spent in semiskilled or skilled labor.
4. Recently upwardly mobile into semiskilled labor; most of work life spent in unskilled labor.
5. Always in semiskilled labor.
6. Downwardly mobile into semiskilled labor just prior to interview; most of work life spent in semiskilled labor.
7. Downwardly mobile into semiskilled labor just prior to interview; most of work life spent in skilled labor.
8. Recently upwardly mobile into skilled labor; most of work life spent in unskilled or semiskilled labor.
9. In skilled labor at time of interview; work history highly variant. Approximately equal proportions of work life spent in unskilled, semiskilled, and skilled labor.
10. Most of work life spent in skilled labor.
11. Recently upwardly mobile into white-collar work; most of work life spent in skilled labor or lower.
12. Most of work life spent in white-collar work.

Note: The decision rules for coding were both more precise and more detailed than these descriptions. Coding was done from the raw skill histories by the computer on the basis of these decision rules. Copies of both the program and the rules are available from the authors.

work by ranking first on the basis of the end point, at the same time enriching this essay by including the immediate path into that position, an item usually ignored.

In accord with the second guideline, the code is sensitive to mobility changes between the three historical points. Should the respondent move to a different skill position from the one previously occupied and occupy that new position at the next point, the code registers that change. It is also sensitive to those changes in which the respondent either spends a good portion of time at a different level but returns to the previous level or moves to a third level by the time of the next incision.

In accord with the third guideline, it emphasizes those points in the work history at which recall is most likely to be strong and backgrounds those points most subject to distortion or memory loss. Since the data come from a panel survey administered in 1954 and 1966, we concluded that informa-

tion about the work history covering the period from 1940 to the last job in the last interview was likely to be strong. Furthermore, we assume two elements in the histories prior to 1940 are too weak to prove dependable for certain matters. These are: (1) the dates assigned specific jobs before 1940 and (2) the sequences of jobs before 1940. The coding score does rely, however, on a gross time estimate of the duration of the longer of these jobs and of enough information about them to code the skill level—a fairly conventional requirement for mobility analyses which use the first job of respondents.

The last problem of the variable construction involves the time span covered by the work histories and provides the basis for separating historical effects from aging effects. We have done this by cutting into the occupational histories at three different points: 1944, before our period of change begins; 1954, the point of the first interview;

and 1966, the point of the last interview. Thus, to illustrate this point, a comparison of the distribution of skill histories of the youngest cohort in 1954 (ages now ranging from thirty-seven downward) with the distribution of the middle cohort in 1944 (ages ranging from twenty-eight to forty-one) controls for the factor of aging and permits the historical circumstances, in particular the occupational structure, to vary. Comparisons which control for historical circumstances and permit aging to vary are easily visualized. Chart 2 provides the age ranges for each cohort at each time point.

The Logic of the Analysis

The analysis proceeds in the following manner. Three cross-tabular distributions comprise the entire data input. These consist of the distributions of the skill histories within each of the twelve categories of the interaction variable—education and cohort—for the three time periods. The analysis proceeds by refracting these distributions through a variety of question-prisms without introducing new data. Each question or each set of propositions focuses on different comparisons. The first section examines the extent to which a comparative statics analysis might assist the work and the possible dangers of describing the distributions in terms of their central tendencies alone. The second section examines the extent of change within each of the twelve education-cohort categories from 1944 to 1966 to determine the patterns of change characteristic of the sample and asks whether or not the data support propositions centered on education or temporal change (history/aging). The third section of the analysis turns to the problem of separating the effects of aging from those of historical change. The fourth and fifth sections of the analysis examine

the relative vulnerability of two of the education-cohort groups to change.

Analysis #1

The first section of the analysis consists of an examination of the data distributions in their most detailed form along with a re-aggregation of the twelve skill histories into a simplified changers/non-changers analysis. Tables 1 and 2 present the data for this analysis.

Table 1 consists of three separate subtables which differ in two ways. The first is the temporal length of the skill history. Table 1a covers the period from the respondents' time of entry into the labor force to 1944; Table 1b covers the period from time of entry, again, to 1954; and Table 1c extends that to 1966. Thus, as noted earlier, the period covered always extends backward to the time of labor force entry and forward to a later date. The second difference between the three tables appears in the differing subtotals for each of the twelve education-cohort groups. These subtotals change during this span from 1944 to 1966 for some of the more obvious reasons: respondents grow too old to work; leave the labor force because of children, unemployment, or to return to school, only to return to the labor force at a later date. The most dramatic case of this change is the post 1931 cohort with nine or more years of school. The number of cases moves from fifty-two in 1944 to ninety-three in 1954, a result of veterans who returned to school.

One of the most familiar forms of analysis for occupational changes is the simple changers/non-changers model. While the twelve skill histories presented in Chart 1 and for which we have distributions in Table 1 are much more complex, it is a simple matter to re-aggregate them into the dichotomies

Chart 2. Ages of Three Cohorts in 1944, 1954, and 1966

Labor Force Entrance	Dates		
	1944	1954	1966
1. Pre 1918	42 years or more	52 years or more	64 years or more
2. 1918-1931	28-41 years	38-51 years	50-63 years
3. Post 1931	27 years or less	37 years or less	49 years or less

EDUCATION AND MOBILITY

Table 1a. Skill Histories from Time of Entry into Labor Force until Each of 12 Education-Cohort Groups



1944 Education with Each of 3 Time Periods of Labor Force Entrance	Skill History Scores in 1944*												Group Totals
	1	2	3	4	5	6	7	8	9	10	11	12	
No School													
1. Pre 1918	24	2	1	6	3	5	-	-	10	1	-	-	52
2. 1918-1931	16	3	2	5	3	-	2	-	-	2	-	-	33
3. Post 1931	9	1	-	2	-	-	-	-	-	-	-	-	12
1-4 Years of School													
4. Pre 1918	10	2	1	5	1	4	6	2	3	9	2	1	46
5. 1918-1931	24	4	3	5	7	5	-	2	8	4	-	-	62
6. Post 1931	34	6	2	7	5	2	-	2	5	4	-	-	67
5-8 Years of School													
7. Pre 1918	2	1	1	-	6	1	1	1	2	-	2	4	21
8. 1918-1931	13	3	1	7	6	7	6	3	10	11	3	2	72
9. Post 1931	23	2	3	7	17	6	2	3	2	5	1	-	71
9 or More Years of School													
10. Pre 1918	-	-	-	-	-	-	1	1	-	1	-	25	28
11. 1918-1931	-	-	-	3	5	1	2	-	2	6	8	24	51
12. Post 1931	3	1	-	1	9	4	2	1	2	11	3	15	52
Skill History Totals	158	25	14	48	62	35	22	15	44	54	19	71	567

* Descriptions of skill histories in Chart 1.

of change and no change. Table 2 presents types: those who remained in the same skill the results of that in terms of the twelve position (Col. 1), those who changed before education-cohort groups. Respondents in 1944 but not after (Col. 2), those who each group are divided into four different moved up after 1944 (Col. 3), and those

Table 1b. Skill Histories from Time of Entry into Labor Force until 1954, for Each of 12 Education-Cohort Groups

1954 Education with Each of 3 Time Periods of Labor Force Entrance	Skill History Scores in 1954*												Group Totals
	1	2	3	4	5	6	7	8	9	10	11	12	
No School													
1. Pre 1918	21	2	-	7	4	6	2	-	7	3	-	-	52
2. 1918-1931	15	5	2	-	1	1	-	3	2	2	-	-	31
3. Post 1931	6	2	-	3	-	3	-	-	-	-	-	-	14
1-4 Years of School													
4. Pre 1918	7	2	1	5	-	7	4	1	7	8	2	-	44
5. 1918-1931	13	6	1	5	7	7	3	6	6	8	1	-	63
6. Post 1931	27	5	4	7	6	4	2	3	9	5	-	-	72
5-8 Years of School													
7. Pre 1918	2	-	-	1	3	1	1	2	2	1	1	4	18
8. 1918-1931	11	6	-	2	5	7	6	2	11	13	-	4	67
9. Post 1931	14	5	2	4	10	6	5	4	8	6	2	-	66
9 or More Years of School													
10. Pre 1918	-	-	-	-	-	-	1	-	-	-	2	26	29
11. 1918-1931	-	1	-	1	2	1	3	1	4	7	9	30	59
12. Post 1931	1	-	2	2	10	5	8	-	2	11	19	33	93
Skill History Totals	117	34	12	37	48	48	35	22	58	64	36	97	608

* Descriptions of skill histories in Chart 1.

Table 1c. Skill Histories from Time of Entry into Labor Force until 1966, for Each of 12 Education-Cohort Groups

1954 Education with Each of 3 Time Periods of Labor Force Entrance	Skill History Scores in 1966*												Group Totals
	1	2	3	4	5	6	7	8	9	10	11	12	
No School													
1. Pre 1918	18	4	3	3	3	3	1	2	5	3	-	-	45
2. 1918-1931	13	6	4	2	1	1	1	-	3	1	-	-	32
3. Post 1931	3	4	-	2	-	2	-	1	1	-	1	-	14
1-4 Years of School													
4. Pre 1918	5	2	1	4	-	6	2	4	6	9	1	1	41
5. 1918-1931	9	6	4	4	5	7	4	3	11	8	-	-	61
6. Post 1931	17	7	7	7	4	11	2	4	10	3	-	-	72
5-8 Years of School													
7. Pre 1918	1	-	1	1	3	1	1	1	1	3	-	4	17
8. 1918-1931	7	5	4	3	5	7	7	2	5	13	4	4	66
9. Post 1931	10	1	-	3	6	9	6	6	11	7	7	-	66
9 or More Years of School													
10. Pre 1918	-	-	-	-	-	1	-	-	-	1	2	23	27
11. 1918-1931	-	-	-	-	2	2	2	-	3	7	8	35	59
12. Post 1931	-	1	1	1	6	6	3	-	2	14	10	49	93
Skill History Totals	83	36	25	30	35	56	29	23	58	69	33	116	593

* Descriptions of skill histories in Chart 1.

who moved down (Col. 4). The fifth column simply sums together the third and fourth. A few comments pertain to this table each of which initiates lines of analysis to follow.

It is obvious that comparative statics just

will not do. In the present case, it would necessarily need to pretend, among other matters, that the skill requirement of a respondent's present job fits the entire career. Such an assumption would fit few of these

Table 2. Proportion Changing Skill Position Before and After 1944 by Education-Cohort Groups

Education-Cohort Groups	(1) % Never Change	(2) % Change Before 1944--No Change After	(3) % Change After 1944--Net Effect Up	(4) % Change After 1944--Net Effect Down	(5) % Change After 1944-- Col. (3) +Col. (4)	Totals Cols. (1) +(2) +(5)
No School						
1. Pre 1918	46	23	12	19	31	100%
2. 1918-1931	42	16	13	29	42	100%
3. Post 1931	21	0	43	36	79	100%
1-4 Years of School						
4. Pre 1918	14	52	22	12	34	100%
5. 1918-1931	22	22	27	29	56	100%
6. Post 1931	29	11	27	33	60	100%
5-8 Years of School						
7. Pre 1918	21	47	21	11	32	100%
8. 1918-1931	19	40	17	24	41	100%
9. Post 1931	23	11	43	23	66	100%
9 Years or More of School						
10. Pre 1918	71	18	7	4	11	100%
11. 1918-1931	3	62	28	7	35	100%
12. Post 1931	6	25	55	14	69	100%

cases. Of the twelve education-cohort groups, only in group ten—respondents with nine or more years of school who entered the labor force early—were more than 50% of the subjects stable; and here the stability is something of a coding artifact. Since our interests focused on and our sample largely consisted of manual workers, we retained only one category above that—non-manual workers. Respondents who started above the manual line would, thus, have to fall below that line at some point in their history to be recorded as changing. The surprising discovery is that the two younger groups at this educational level—groups eleven and twelve—have the opposite characteristic, the lowest levels of stability. This despite the constraints of that coding artifact. Further underlining the unnecessary fiction of statics is the fact that, including the previously mentioned group ten, only three groups have over 30% stability. This absence of stable positioning is worth underlining. One repeatedly sees the observation that the rate of change in industrial societies is high; but here we discover that even in a social setting so recently initiated into the industrial mysteries, workers move far more than expected.

Second, the rate of change has little to do with levels of education. Whether one looks at columns one, two, or five, little similarity within or, for that matter, across levels of education exists. Moreover, even the direction of the change after 1944 appears to have little to do with education. Columns three and four reveal no patterning in terms of education.

Instead, the patterning is definitely one that favors the aging/historical change explanations. Column five reveals a consistent monotonic pattern within each level of education, the younger the cohort the higher the rate of change. Moreover, the differences are not only consistent, they are dramatic. This patterning suggests that changes in the social structure of the order of those in Puerto Rico between 1944 and 1966 tend to alter the skill characteristics of younger workers more than those of older ones. It would appear, from this initial arrangement of the data, that the Karlsson and Carlsson argument more accurately fits our data. However, the conclusion is premature. An analysis such as the

preceding has some obvious and some not so obvious deficiencies for our purposes. The most obvious is the absence of tests of significance, a simple matter to correct. However, other deficiencies make the correction unnecessary. The first of these is that the propositions in earlier portions of this paper concern the direction and extent of skill movements and shifts in the distribution of careers between the three time periods. While simply changing positions may not be contingent on education, changes in direction may be. But the most important deficiency of the previous analysis is the failure to separate the effects of aging from those of historical change. We suggested above that the young tend to be particularly effected by the changes between 1944 and 1966; but we have yet to determine why this is the case, i.e., whether age or history accounted for it. What we have determined, a matter of some importance later, is that changes among the youngest cohorts reflect changes of individuals rather than aggregate changes only.

Analysis #2

Thus, the analysis shifts at this point. We shall return to a dependent variable which consists of the twelve skill histories described in Chart 1 and which permits simultaneous attention to range and direction of movement as well as starting and end points. We use the Mann-Whitney U statistic, an ordinal analogue of a T test, for comparisons between distributions (Siegal, 1956:116-17).

Table 3 summarizes the first of these analyses. It indicates which groups have significant skill history distributional changes between time periods of the three contrasts: 1944 to 1954, 1944 to 1966, and 1954 to 1966.

The pattern presented is uneven at best, but initially suggests again that the explanations of aging and historical change comprise the best fit. The distributions of five education-cohort groups changed significantly from 1944 to 1966, four of which were those who entered the labor force after 1931—groups 3, 6, 9, and 12. Thus, at each level of education, the youngest group changed significantly during this twenty-two year period of industrial change. This result, of course, re-

Table 3. Skill History Distributions Compared for Each of 12 Education-Cohort Groups, Recording Whether or Not Each Changed between 1944 and 1954, 1954 and 1966, 1944 and 1966

Education-Cohort Groups	Skill History Distribution Comparisons Between					
	1944 & 1954		1954 & 1966		1944 & 1966	
	Change	No Change	Change	No Change	Change	No Change
<u>No School</u>						
1. Pre 1918		*		*		*
2. 1918-1931		*		*		*
3. Post 1931		*		*	**	
<u>1-4 Years of School</u>						
4. Pre 1918		*		*		*
5. 1918-1931	**	*		*	**	
6. Post 1931		*		*	**	
<u>5-8 Years of School</u>						
7. Pre 1918		*		*		*
8. 1918-1931		*		*		*
9. Post 1931	**		**		***	
<u>9 Years or More of School</u>						
10. Pre 1918		*		*		*
11. 1918-1931		*		*		*
12. Post 1931	*		**		***	

* .10 level of significance with Mann Whitney U test.

** .05 level of significance with Mann Whitney U test.

*** .01 level of significance with Mann Whitney U test.

produces the finding of individual change in Table 2. Once again, it suggests either that major social changes of the variety of those in Puerto Rico during this period are more likely to alter the lives of younger workers, or that change, regardless of social setting, occurs more often for younger than older workers. In the latter case, the fixation hypothesis seems appropriate: individuals tend to change until they reach middle age.

Although this pattern of life cycle changes is clear enough, another pattern overlays it. The fifth education-cohort group to change significantly during this period is group five, composed of individuals with one to four years of school who entered the labor force between 1918 and 1931. Since this change also appeared in the 1944 and 1954 comparisons—a point at which the age range for the group was thirty-nine to fifty-two, one is tempted to argue that the fixation hypotheses are only further borne out. However, were this set of changes to constitute a pattern, one would expect all the youngest cohorts to change significantly between 1954 and 1966, a comparable aging point, as well as the other education groups in the same cohort (1918-

1931), which they do not. Further complicating the picture, only two education groups in the young cohort (post 1931)—the five to eight group and the nine+ group—changed significantly between 1954 and 1966 as well as between 1944 and 1954. All of these complicating overlays suggest that, instead of a simple fixation pattern, differential rates of change appear at varying educational levels, varying life cycle points, and, possibly, varying historical points.

This table, also, rather sharply dismisses Goldthorpe's observation that an acceleration of industrialization necessarily shoves individuals with little education or advanced age downward. None of the five groups with changes during this period were downward changes. Older and less educated groups at least held their own through the period. Only if industrial change pushed a great many workers out of the labor force by 1954 could the downward mobility thesis hold. In that case, they would have missed the sample in 1954. That is an unlikely prospect, however. Though unemployment was high at that point, it had been high for some time and remains so. Moreover, one version of the life

cycles propositions gains no support for this table. This is the view that as workers reach older ages they do not so much slow down rates of job changing as progressively disengage themselves from demanding physical and mental work to take up lower status, less trying work. Insofar as such changes might reflect themselves in downward mobility, they do not appear in these tables.

This conclusion is an important one, for the absence of a relationship with downward mobility raises a question about our use of the ascription/achievement dichotomy to organize our propositional net. Insofar as one simply interprets that to mean that industrialization precipitates an increasing loss of control over mobility prospects and means by that, that downward mobility increases, this table provides no support. Thus, insofar as the proposition is thought to apply to the histories of these twelve education-cohort groups as they move through this period, it does not fit. However, the proposition as stated addresses comparisons *between* groups.

Analysis #3

A brief summary is in order. Our analysis to this point suggests that as the economy changed in Puerto Rico during these years, its impact on the skill histories of individuals largely corresponded to the biology of aging: younger workers were mobile, older ones settled. In addition, the experiences of older workers in other periods of industrialization appears to have been bypassed: their positions were not systematically jeopardized by such changes. All of the foregoing then, suggests that industrialization with its differentiated division of labor, very likely permitted biology to manifest itself in work, that a biologically derived propensity to change in the early years appears in work when a differentiated economy and a normative structure encouraging mobility render it possible. However appealing that conclusion may temporarily appear on the basis of the analysis, it should remain tentative. The data permit two further interrogations which may well modify it. In the first place, we have yet to seriously address ourselves to the question of the relations between the processes of

aging and those of historical change. Did the mobility patterns associated with aging change during these periods? As the economy moved from commercialized agriculture and a labor force largely consisting of agricultural proletarians to one more nearly industrial, did the mobility of younger workers change? Analysis #3 examines these questions. At the same time the economy changed the "massification" of education proceeded particularly by expanding educational facilities. While such changes certainly did not directly affect our sample—they were all out of school and in the labor force by the early 40's—the increasing provision of more and better educated workers during this period may well have supplied an indirect effect particularly by increasing certification requirements. Analysis #4 explicitly addresses itself to these questions of educational refraction. It is, of course, this last comparison which permits a confrontation with the earlier suggestions that status achievement processes move from the work place to the school.

Table 4 suggests that, whatever changes industrialization introduced into the life paths (skill histories) of Puerto Ricans, these changes were unevenly dispersed across strata. Mobility constricted within strata associated with credentialling processes but remained the same within other strata.

But the first point of note is that increased mobility is completely absent in all comparisons. Thus, where differences between groups are statistically significant, the later group in time always has less mobility. This conclusion is not a small one. At a minimum it means what it says. Changes in the structure of occupational trajectories for workers already in the labor force during this period did not increase opportunities for upward mobility for them when compared with previous cohorts from the same stratum at similar age points prior to such changes. Two other possible statements concerning more or less mobility might be made at this juncture. One might argue that the later cohort was better educated and, thus, that, while mobility did not improve within strata, it did improve for the cohort. A table, not included in this paper, which compares cohorts without controlling for strata indicates that intragenera-

Table 4. Skill History Distribution Comparisons which Separate Societal Effects from Aging Effects for Each of 4 Education Levels

Comparison	Education Level	Life Cycle Point	Labor Force Entrance for Groups	Years Compared	Skill History Distributions Compared		
					Same	Different	Somers Dyx
1.	No education	1 (app. mid. yrs.)	1918-31 post 31	44 54	*		
2.	No education	2 (middle yrs.)	pre 17 1918-31	44 54	*		
3.	No education	2 (middle yrs.)	pre 17 post 31	44 66	*		
4.	No education	2 (middle yrs.)	1918-31 post 31	54 66	*		
5.	No education	3 (app. retirement)	pre 17 1918-31	54 66	*		
6.	1-4 years	1 (app. mid. yrs.)	1918-31 post 31	44 54	*		
7.	1-4 years	2 (middle yrs.)	pre 17 1918-31	44 54	*		
8.	1-4 years	2 (middle yrs.)	pre 17 post 31	44 66		**	-.23
9.	1-4 years	2 (middle yrs.)	1918-31 post 31	54 66	*		
10.	1-4 years	3 (app. retirement)	pre 17 1918-31	54 66	*		
11.	5-8 years	1 (app. mid. yrs.)	1918-31 post 31	44 54	*		
12.	5-8 years	2 (middle yrs.)	pre 17 1918-31	44 54	*		
13.	5-8 years	2 (middle yrs.)	pre 17 post 31	44 66	*		
14.	5-8 years	2 (middle yrs.)	1918-31 post 31	54 66	*		
15.	5-8 years	3 (app. retirement)	pre 17 1918-31	54 66	*		
16.	9+ years	1 (app. mid. yrs.)	1918-31 post 31	44 54	*		
17.	9+ years	2 (middle yrs.)	pre 17 1918-31	44 54		***	-.36
18.	9+ years	2 (middle yrs.)	pre 17 post 31	44 66		***	-.39
19.	9+ years	2 (middle yrs.)	1918-31 post 31	54 66	*		
20.	9+ years	3 (app. retirement)	pre 17 1918-31	54 66		**	-.30

* .10 level of sig. with Mann Whitney.

** .05 level of sig. with Mann Whitney.

*** .02 level of sig. with Mann Whitney.

tional mobility, in this sense, did not improve during this period for cohorts already in the labor force. The second possible comparison involves parents with their children, the traditional focus of mobility studies. That comparison in other works on Puerto Rico with the first wave of this data suggests a good bit of such mobility, but the comparisons are too different to be made here. (Tumin with Feldman, 1961).

The second point of note is that where differences do emerge the reason is that younger cohorts have not been as mobile as older cohorts at similar age points. Such results appear in two sections of the table—the comparisons for the stratum with one to four years of schooling and the stratum with nine or more years of school. In the lower of the two strata the comparison of the youngest cohort in 1966—when its age range was from fifty down—with the older cohort in 1944—when its age range was forty-three and older—proves to be statistically significant. Both the negative score of the Somers Dyx and a glance at the comparative distributions indicate that the young cohort, after this period of change, has not attained the same mobility as the older one at the same age point prior to the period. The same differences are true in the highest stratum, only more of them and sharper. In this case, a look at the distributions in Table 1 indicates even more clearly what is occurring. The oldest cohort whose education and initial entry into the labor force came before 1917 is, by 1944, almost without exception, massed in white collar occupations. The insurance of white collar employment provided for this oldest cohort was not available for the later two cohorts. The problem, of course, for both strata is to determine just why this is the case. Such a determination involves a series of steps, the first of which is to isolate the time period of most important determination. Temporally speaking, the differences in the distributions could come from two temporal conjunctions. The first is that the latest cohort in the stratum with one to four years of schooling and the latest two cohorts in the higher stratum began their work life already disadvantaged relative to other cohorts. Thus, the conjunction of education and the economy at the time each of these cohorts

entered the labor force would provide the object of search. The second is that the subsequent events on the island, particularly those in the period from 1944 to 1966, account for the differences. The question, however, permits of no easy answer. In fact, the data become somewhat opaque when examined from this vantage point. In the first place, the problematics of respondents' memories makes any serious answer unusually suspicious. A comparison of entering skill positions across cohorts is undermined by the unusual memory reach required of the oldest respondents. In the second place, the emphasis on movements in the blue collar regions vitiates comparisons of movement in white collar positions.

Analysis #4

The analysis of the previous sections isolates the experiences of two strata as particularly critical for our discussion—that of the stratum with one to four years of school and that with nine or more years. The following section consists of two parts, each devoted to an intensive analysis of the comparative mobility of each stratum.

Table 5 presents the material for the analysis of the stratum with one to four years of school. This table compares the skill histories of the stratum of focus with adjacent education levels within cohorts. In short, it is a between-education-levels-within-cohort analysis. The table is to be read as follows. It groups each of the three cohorts separately, then presents the educational scale for the

Table 5. Educational Scale Comparisons for 3 Cohorts at Each of 3 Temporal Points

Years of Labor Force Entry	Scale #	Year	Scale			
			1	2	3	4
Pre 1918	(1)	1944	0	1-8	9+	
	(2)	1954	0	1-8	9+	
	(3)	1966	0	1-8	9+	
1918-1931	(4)	1944	0-4	5-8	9+	
	(5)	1954	0	1-4	5-8	9+
	(6)	1966	0	1-8	9+	
Post 1931	(7)	1944	0-4	5-8	9+	
	(8)	1954	0	1-4	5-8	9+
	(9)	1966	0-4	5-8	9+	

cohort at each of the three temporal points of 1944, 1954 and 1966. The educational scale derives from a procedure of comparing each level of education with the remaining three educational levels. Thus, the distribution for those with no education was compared with that for one to four years, five to eight years, and nine or more years. Next, the one to four stratum was compared with each of the two strata above, etc. The distributions deemed alike, using the Mann-Whitney U statistic, were grouped together. The resulting scale then expresses the educational points at which the skill histories are significantly different. As an illustration, the scale for the oldest cohort—the pre-1918 cohort—in 1944 was a trichotomous one—the skill history distribution of those heads with five to eight years. Thus, in effect, so far as subsequent skill mobility was concerned, these two educational levels constitute only one stratum, not two. Having a slight bit of school proved just as determinative for subsequent skill movements as having as much as eight years.

The youngest cohort in the stratum with one to four years of school was less mobile by its middle years (1966) than the oldest cohort in its middle years (1944). This youngest cohort is also the only cohort in which, by their middle years (1966), the skill history distribution of this stratum was no different than those who had failed to enter school. Thus, scale #9 compared with #5 and #1 indicates that, for each of the older cohorts, the benefits of slight schooling appeared; whereas they were absent for this youngest cohort. Moreover, scale #1 indicates that, not only did the stratum with one to four years of school of this oldest cohort in its middle years before our period of change do better than those with no education; they also did as well as those with five to eight years of school. Thus, the mobility of the youngest cohort is doubly damaged in its middle years. Its slight bit of education failed to assist mobility, whereas the acquisition of a slight bit more would have done so. Moreover, the difference now coincides with the line of functional literacy.

These differences may be examined a bit more thoroughly by comparing the move-

ments of this youngest cohort through this period with those of the older two cohorts. The earliest—those who entered the labor force before 1918—enter the period with a trichotomous classification and emerge from it with the same classification. The ability of those with no education and as much as those with five to eight years is never damaged. If certification of functional literacy progressed during this period, it did not damage the positions of workers already well into their work lives. The middle—1918–1931—cohort presents another sequence, however. It enters the period with the same differentiation of the younger cohort (scale #4 compared to #7), differentiates the stratum of interest—those with one to four years—from those with no education, and then emerges with the same differentiation as the older cohort maintained throughout the period. A comparison of this sequence with the absence of change in the older cohort suggests that the trajectory described by the older cohort is a pre-industrial one undamaged by subsequent industrialism, while the trajectory of the middle cohort belatedly but eventually becomes similar. The critical group for such an argument is that with one to four years of school in the middle cohort. On the basis of this table—in which it first differentiates itself from the lower education level and then reaches the distribution of the higher level . . . and Table 3—in which it was the only education group in this cohort to change significantly between 1944 and 1966, this group does appear to be a relatively highly mobile group. It appears that literacy certification requirements simply did not exist for this group. Interestingly enough, the youngest cohort appears to pass through two points in this sequence—scales 7 and 8—before such certification cuts short the sequence. Thus, combining for the moment the observation in the previous analysis, that the youngest cohort in the stratum with one to four years of schooling fails to reach the pre-industrial same-age levels of older cohorts, with the observations of this section, strongly suggests the following. At least at this level, education increasingly shapes the temporal flow of work life. In the sense previously discussed, the criteria for occupational advancement move

from the work place to the schoolroom. With respect to these sequences, ascription replaces achievement.

Analysis #5

The same conclusions emerge from an analysis of the highest education level; but from a different organization of the data. Unlike the previous section in which we compared the movements of the stratum of interest with an adjacent stratum, in this section we divide the highest education stratum—those with nine or more years of school—into two groups—those with nine to eleven years of school and those with twelve or more—and compare the movements of these groups. Thus, instead of focusing our interest on comparisons between the two relatively well educated groups—those with five to eight years and those with nine or more—to see if the difference widened, our treatment in this section will focus on the role of high school certification as a changing differentiating point. Table 6 presents these results.

The table is divided, first, into the three cohorts based on the date of entry into the labor force and, then, into the three time points—1944, 1954, and 1966—at which we stopped the flow of the skill histories. This provides nine comparisons for the two education strata.

An analysis of Table 6 indicates that the same pattern as appeared in the previous section also displays itself here, except the differences emerge earlier. The use of high school degrees as certification points which both control access as well as subsequent occupational moves begins to appear with the middle cohort rather than the youngest cohort. A summary look at the table shows that: (1) Just as in the previous analysis, the oldest cohort describes what we are labeling a pre-industrial trajectory—the two groups do not differentiate; they enter the period and leave it with similar distributions of the skill histories. (2) The middle cohort enters this period already sharply differentiated and emerges from it still differentiated unlike the same comparison in the previous analysis.

Table 6. Comparisons of Respondents with 9-11 Years of School and 12 or More Years--Three Cohorts, Three Time Points

Cohort	Year	Educ.	Skill History Score												Totals	Test of Significance	Somers Dyx
			1	2	3	4	5	6	7	8	9	10	11	12			
Pre 1918	1944	9-11	0	0	0	0	0	0	0	1	0	0	0	5	6 22	Same	
		12+	0	0	0	0	0	0	1	0	0	1	0	20			
	1954	9-11	0	0	0	0	0	0	0	0	0	0	1	6	7 22	Same	
		12+	0	0	0	0	0	0	1	0	0	0	1	20			
	1966	9-11	0	0	0	0	0	0	0	0	0	1	1	4	6 21	Same	
		12+	0	0	0	0	0	1	0	0	0	0	1	19			
1918-1931	1944	9-11	0	0	0	2	4	1	2	0	2	6	5	9	31 20	Diff. .05	.487
		12+	0	0	0	1	1	0	0	0	0	0	3	15			
	1954	9-11	0	1	0	1	2	1	1	1	4	6	4	13	34 25	Diff. .05	
		12+	0	0	0	0	0	0	2	0	0	1	5	17			
	1966	9-11	0	0	0	0	2	2	1	0	3	6	5	15	34 25	Diff. .05	
		12+	0	0	0	0	0	0	1	0	0	1	3	20			
Post 1931	1944	9-11	1	0	0	0	6	3	2	0	1	9	3	3	28 24	Same	
		12+	2	1	0	1	3	1	0	1	1	2	0	12			
	1954	9-11	1	0	2	1	5	4	6	0	2	8	7	4	40 53	Diff. .01	
		12+	0	0	0	1	5	1	2	0	0	3	12	29			
	1966	9-11	0	1	0	1	3	4	3	0	1	12	5	10	40 53	Diff. .01	
		12+	0	0	1	0	3	2	0	0	1	2	5	39			

(3) The youngest cohort enters undifferentiated and emerges differentiated. (4) The youngest two cohorts describe a parallel age trajectory in terms of the Somers' Dyx comparisons. These conclusions suggest, in turn, that, at this level of education, education becomes ascriptive.

Finally, one comment concerning the declining Somers' Dyx scores as the two younger cohorts move through this period. Does this suggest that age related mobility processes run counter to the historical changes? Perhaps. But it is difficult to determine. First, since our focus was on movements across manual occupations, we have only one category for the nonmanual. A finer distinction here might indicate something quite different. Second, the declining Somers' Dyx are largely a consequence of movement across manual occupations rather than into nonmanual positions.

CONCLUSION

Our analysis highlights the limitations of binary conceptualizations of social change where factors of change are problematic and sequences non-problematic. Not only are binary contrasts such as ascription and achievement too simplistic; more importantly, they obscure the diversity of change. The impact of the interlocking changes in education and work on certain of the life experiences of Puerto Ricans was far from unitary. Not only does its impact vary across the historical time associated with changes in the social structure during industrialization and across the life cycles of individuals, but also across levels of educational certification. Moreover, although other studies indicate intergenerational mobility was comparatively high on the island at the mid point of the time span we are examining (Tumin with Feldman, 1960), the preceding clearly indicates that the growth of certification requirements damaged the career mobility opportunities of some workers.

However, the importance of our study reaches beyond the issue of education and modernization on the island of Puerto Rico. It suggests an adequate analysis of the importance of education for job careers proceeds best if change strategies such as ours

are adopted. Our emphasis on the temporal order derived from the stress on sequencing suggests that situationally anchored cohort studies of intra-generation mobility provide better designs for the study of certain aspects of these processes than do the isolated studies of inter-generational or intra-cohort mobility.

Inter-generational mobility studies, whether in industrialized or industrializing societies are the most likely to conceive of education and social change through the polarities of ascription/achievement. Whether the studies are those of the limiting case of inter-generational mobility or the broader ones of changes between successive generations—changes that have occurred over periods of many years—it is in these types of social change inquiries that we are most likely to hear that the directionality of the change is from ascription to achievement. Inferences based on this type of sequencing—that of inter-cohort—are in no way affirmed by our data. If we are pressed to work with the polarities, we would have to say that the obverse is more descriptive: the movement is rather from achievement to ascription.

Such a conclusion, meshes interestingly enough, with that of Wacław Machajski, a Polish intellectual writing around the turn of the last century. He argued that processes associated with education and educational change, under certain circumstances, contributed to rather than alleviated alienation in the workplace (Haimson, 1955). Insofar as a revolutionary movement permitted itself to be controlled by its intelligentsia, its own revolution would result simply in a displacement of one ruling elite with another; and the control of workers over the circumstances of their lives would be little enhanced by such changes. Something of that same process is occurring here. Though the changes involved are neither so abrupt nor so much the product of the class struggle as those envisioned by Marxist thought in the early part of this century, the result is little different, perhaps even more pronounced in this latter case. Insofar as the sanctions of promotion and demotion move away from the workplace, move away from those settings in which the worker exercises some control however little, just so far has this dimension of alienation proceeded.

However, moving from macro to micro sequencing, from inter-cohort to intra-cohort analysis places our question of the importance of education on job careers in yet another focus. Our data concur with recent studies (Berg, 1970; Jencks, 1972) which claim that as individuals age the amount of schooling which they managed to attain becomes less salient for their subsequent job mobility. If we adopt the polarity of ascription and achievement here, the directionality for the micro would be the reverse of the macro. Indeed, a coupling of our study in an industrializing society with the present findings in an industrialized one might lead some to the conclusion that previously postulated change processes are not only in need of revision but patently wrong.

Such a generalization is precipitous. Our data highlight the limitations of looking at the importance of education by considering the sequences of inter-cohort and intra-cohorts separately. They stress that the issues of social change can not be circumscribed by simply looking at the life cycles of individuals or by conceiving of change as a series of points which can be contrasted in a binary fashion. We are convinced a proper sociological accounting of social change comes best from a conception of change viewed as a series of points temporally ordered and probabilistically related both within and across the lives of cohorts.

REFERENCES

- Becker, Howard S.
1960 "Notes on the concept of commitment." *American Journal of Sociology* 66(July): 32-40.
- Berg, Ivar
1970 *Education and Jobs: The Great Training Robbery*. New York: Praeger Publishers.
- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: John Wiley & Sons, Inc.
- Carlsson, Gosta and Katarina Karlsson
1970 "Age, cohorts and the generation of generation." *American Sociological Review* 35 (August): 710-18.
- Duncan, Otis Dudley
1966 "Methodological issues in the analysis of social mobility." Pp. 51-97 in Nell J. Smelser and Seymour Martin Lipset (eds.), *Social Structure and Mobility in Economic Development*. Chicago: Aldine Publishing Company.
- Eisenstadt, S. N.
1966 *Modernization: Protest and Change*. Englewood Cliffs, N.J.: Prentice-Hall, Inc.
- Goldthorpe, John H.
1966 "Social stratification in industrial society." Pp. 648-59 in Reinhard Bendix and Seymour Martin Lipset (eds.), *Class, Status, and Power*. Second Edition. New York: The Free Press.
- Guessous, Mohammed
1967 "A general critique of equilibrium theory." Pp. 23-35 in Wilbert E. Moore and Robert M. Cook (eds.), *Readings on Social Change*. Englewood Cliffs, N. J.: Prentice-Hall, Inc.
- Haimson, Leopold
1955 *The Russian Marxists and the Origins of Bolshevism*. Cambridge.
- Hoselitz, Bert F.
1963 "Main concepts in the analysis of the social implications of technical change." Pp. 11-31 in Bert F. Hoselitz and Wilbert E. Moore (eds.), *Industrialization and Society*. UNESCO: Mouton.
- Hughes, Everett C.
1971 *The Sociological Eye*. Chicago: Aldine Publishing Co.
- Inkeles, Alex
1960 "Industrial Man: the relation of status to experience, perception, and value." *American Journal of Sociology* 66(July): 1-31.
- Jacobson, Barbara and John Kendrick
1970 "Education: social fact or social process." *American Behavioral Scientists* 14(November/December): 255-71.
- Kahl, Joseph A.
1968 *The Measurement of Modernism*. Austin: The University of Texas Press.
- Kerr, Clark, J. T. Dunlap, T. H. Harbison and C. A. Myers
1960 *Industrialism and Industrial Man*. Cambridge: Harvard University Press.
- Kubler, George
1962 *The Shapes of Time: Remarks on the History of Things*. New Haven: Yale University Press.
- Landes, David S.
1969 *The Unbound Prometheus*. Cambridge: Cambridge University Press.
- Levy, Marion J., Jr.
1966 *Modernization and the Structure of Societies: A Setting for International Affairs*. Princeton, N.J.: Princeton University Press.
- Mayhew, Leon
1968 "Ascription in modern societies." *Sociological Inquiry* 38(Spring): 105-20.
- Miller, S. M. and Pamela Roby
1970 *The Future of Inequality*. New York: Basic Books, Inc.
- Moore, Wilbert E.
1963 *Social Change*. Englewood Cliffs, N.J.: Prentice-Hall.
1965 *The Impact of Industry*. Englewood Cliffs, N.J.: Prentice-Hall.

- 1970 "Toward a system of sequences." Pp. 155-66. John C. McKinney and Edward Tiryakian (eds.) *Theoretical Sociology*. New York: Appleton-Century-Crofts.
- Moore, Wilbert E. and Arnold S. Feldman**
1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Nisbet, Robert**
1969 *Social Change and History*. New York: Oxford University Press.
- 1970 "Developmentalism: a critical analysis." Pp. 167-204 in John C. McKinney and Edward A. Tiryakian (eds.), *Theoretical Sociology*. New York: Appleton-Century-Crofts.
- Parsons, Talcott**
1951 *The Social System*. Glencoe, Illinois: The Free Press.
- Ryder, Norman B.**
1965 "The cohort as a concept in the study of social change." *American Sociological Review* 30(December):843-61.
- Siegel, Sidney**
1956 *Nonparametric Statistics for the Behavioral Science*. New York: McGraw-Hill Book Company, Inc.
- Smelser, Neil J.**
1963 "Mechanisms of change and adjustment to change." Pp. 32-54 in Bert F. Hoselitz and Wilbert E. Moore (eds.), *Industrialization and Society*. UNESCO: Mouton.
- Smelser, Neil J. and Seymour Martin Lipset**
1966 "Social structure, mobility and development." Pp. 1-50 in Neil J. Smelser and Seymour Martin Lipset (eds.), *Social Structure and Mobility in Economic Development*. Chicago: Aldine Publishing Company.
- Sonquist, John A. and James N. Morgan**
1967 *The Detection of Interaction Effects*. Ann Arbor, Michigan: Survey Research Center, University of Michigan.
- Stone, Lawrence**
1969 "Literary education in England, 1640-1900." *Past and Present* 42(February): 69-139.
- Sussmann, Lelia**
1968 "Democratization and class segregation in Puerto Rican schooling: the U.S. model transplanted." *Sociology of Education* 41 (Fall):321-41.
- Talbott, John E.**
1971 "The history of education." *Daedalus* 100 (Winter):133-50.
- Tanner, J. M.**
1971 "Sequence, tempo and individual variation in the growth and development of boys and girls aged twelve to sixteen." *Daedalus* 100(Fall):907-30.
- Thompson, E. P.**
1963 *The Making of the English Working Class*. New York: Vintage Press.
- Tumin, Melvin with Arnold S. Feldman**
1961 *Social Class and Social Change in Puerto Rico*. Princeton: Princeton University Press.

GROUP DISORDERS IN THE PUBLIC SCHOOLS *

PAUL RITTERBAND

*The City College of the City University
of New York*

RICHARD SILBERSTEIN

Columbia University

American Sociological Review 1973, Vol. 38 (August):461-467

A large body of evidence points to high rates of group disorder in America's high schools. This paper examines several alternative models to account for the distribution of these events in the nation's largest school system. It is the thesis of the paper that while disorders occur in the schools, their roots lie in the larger society.

BOTH social science and journalistic accounts have been reporting high rates of disorders in America's high schools. Bailey (1970:9) found 85% of the nation's high school principals reporting some form of student disruption during the period 1967-1970. A study commissioned by the National Association of Secondary School Principals (1969) reported that 59% of all high schools and 67% of the urban high schools had experienced some form of student protest during the 1968-69 academic year. Using newspaper accounts, Westin (1970) counted 2,000 high school disruptions from November 1968 through May 1969. A large scale study commissioned by the House of Representatives Sub-Committee on General Education (Congressional Record, Feb. 23, 1970) reported that 18% of the schools had experienced "serious protests." While the occurrence of school disorders is well documented, the causes remain somewhat obscure. In this paper we shall attempt to account for rates of two forms of group disorder in the academic high schools of New York City during the period November 1968 through June 1969.¹ We shall be attempting to determine to what extent if any, variation

in rates of student disorder and disruption reflect variations in system characteristics or ethnic aggregates in the schools. Our thesis is that while disorders take place in schools, their roots lie in the larger society.

I. DATA: SOURCES AND RELIABILITY

The data for the study have been made available to us by the Educational Planning Division of the New York City Planning Commission. The basic school characteristics are based on the records of the Board of Education of the City of New York. The disorders we analyze are those for which the police were called. The classification of these events is based on the descriptions of the disorders recorded in the police department's log book of calls to the schools. The police reports routinely describe the events by number of persons involved, arrests (if any) and the nature of the activity. They do not report personal attributes of those participating, e.g. race, sex, age; nor were time data made available. Analysis is restricted to events in which more than five students participated. We make this restriction for two reasons. First, we are explicitly interested in group disorders. Second, we have reason to believe that events involving five or fewer persons are likely to be less reliably reported than is the case for larger events. Based on the police descriptions, events were further classified as political or non-political group disorders. Political disorders are events in which any of the following were noted: mass picketing, circulation of petitions, presentation of list of demands. Non-political disorders tend to be free-for-all fights and gang rumbles. The time period for which we have data (November 1968

* The research reported here was conducted under grant 1 RO1 MH17613-01 from the National Institute of Mental Health. We wish to thank Harry Davidow and Janice Weinman for helping us gain access to the data. We are grateful to Seymour Spillerman and Charles Tapiero for their very helpful comments on earlier drafts of this paper.

¹ We have restricted our analysis to the comprehensive academic high schools of which there were fifty-six in New York City in 1968. We have excluded the four test schools (access to which is controlled by passing an entrance examination) and the vocational high schools. The data for these schools are not fully comparable.

through June 1969) followed the massive school strike which flowed from the community control dispute.² There is some reason to believe that this might have been an extraordinary period for student disorder. However, the volume of disorder reported in New York City corresponds with that found in the nation at large. Sixty per cent of New York's academic high schools experienced political disorders, 30% non-political disorders and 66% any form of group disorder.

Both the school characteristics and disorders are reported in the form of aggregates. Police reports do not describe the individuals involved by race, SES, etc. Since we shall be reporting ecological correlations, our data could lead us to commit ecological fallacies in so far as we make inferences to the individual level. The inference from aggregate to individual level findings is premised on the assumption that there are no significant ecologically determined interaction effects.

II. ALTERNATIVE MODELS

Three alternative models might be appropriate for understanding group disorders. These are (1) random events, (2) exogenous causation and (3) system effects. Each model and its empirical implications will be examined in turn.

1. Random Events

Each event presumably had some "cause" if by cause we mean some precipitating act or plan in the minds of those involved. In this context we take random to mean that the distribution of events does not reflect patterned variation in factors presumed to be causal and that the events are independent of one another. In a larger, more comprehensive population of organizations or even of schools we might find a pattern to the events which could be shown to be associ-

ated with a causal factor. The true independent variables (whatever they might be) in the more comprehensive population would be constants in the study population; the population under study would be homogeneous with respect to those causes. For example, if the "cause" of rates of disorders is presumed to be the oppressive environment of the school and the data fit a random distribution, then they might do so because all schools in New York are equally oppressive, or the variation among schools, is too small to account for variations in rates of disorders. Alternately one might think of disorders as characteristic of adolescence. Since the study population is homogeneous with respect to age, a distribution reflecting randomness would be expected.

If the distribution of events in the study population is random over schools, then it should conform to the Poisson distribution. The key assumptions of the Poisson distribution are (1) that disorder proneness is equal for all schools i.e. the population of schools is homogeneous with respect to whatever the causal factor might be; (2) the outbreak of one event does not change the probability that another event will occur in the same school or across schools. That is, there is neither contagion (across schools) nor reinforcement (within schools) (Spilerman, 1970, 631; Coleman, 1964, 288-307).

These assumptions are built into the Poisson model which is expressed as

$$P_k = \frac{\lambda^K e^{-\lambda}}{K!}$$

where K = a given number of events, P_k is the probability that k events will occur in a given school; λ is the disorder proneness for the schools in the system; e is a constant (Napierian log). For each school in the system probabilities are generated for $0 \dots n$ events. The sum of probabilities within any school is equal to one. The sum of these probabilities over the fifty-six schools gives the expected distribution of events throughout the system under the Poisson assumptions. Given the Poisson assumptions above, λ is equal to \bar{X}_k events, i.e. the disorder-proneness for each school is assumed to be the mean number of disorders observed in the system. In general, if a given phenomenon is random, the mean affords the best estimate

² In the fall of 1968, a sequence of three strikes occurred in the New York City school system, effectively crippling the educational process and at the same time creating overt hostility between the teachers' union and the advocates of "community control." Since the union was predominantly Jewish and the community control forces predominantly black, the situation developed strong racial animosities within the system which exist to some extent today.

of an expected frequency. Similarly, the mean is our best estimate of λ (at this point).

The Poisson procedure yields a hypothetical (expected) distribution representing what the frequency would be if disorders were in fact random events. We then can compare the derived expected distribution with the observed distribution of events. The extent to which the two distributions differ will determine the rejection or acceptance of the non-random hypothesis; if the distributions are dissimilar we would accept the hypothesis that the events were not random.³

The observed distributions and the distributions expected (given the Poisson assumptions) are presented in Columns A and

B of Table 1. Clearly the actual distribution of political disorders in no way resembles the expected distribution. The very high chi square reflects the marked discrepancy between the observed and expected distributions. The distribution of the non-political events comes much closer to that expected under Poisson assumptions. Its shape is roughly that of the expected distribution. The data suggest that schools do not differ significantly in their proneness towards non-political disorders, but do differ in their proneness to political disorders.

Insofar as non-randomness is attributable to heterogeneity (i.e. to the relationship of disorders with exogenous variables) and the exogenous factors are the same for both political and non-political events, we should expect significantly lower correlations with non-political events. We shall examine this inference in the next section.

2. Exogenous Causes of Disorder

The distinction we make between exogenous and system causes of disorder cor-

³ The distributional tests in Table 1 are calculated on absolute numbers. The disorder data reported in Tables 2 and 3 are based on rates. We do this so that we may introduce school size as a conceptually and empirically distinct issue. A recent paper of Scott and El-Assal (1969) underscores the problems of interpretation which emerge when the problem of rates and incidence is not handled explicitly. The correlations and regressions were also computed for absolute numbers with comparable results.

Table 1. Distribution of Events Observed and Expected--Poisson Technique

A			B			C		
Political Disorders			Non-Political Disorders ²			Political Disorders Adjusted for Black Enrollment (Relaxing Homogeneity) ¹		
Number of Schools With k Disorders			Number of Schools With k Disorders			Number of Schools With k Disorders		
k	Observed	Expected	k	Observed	Expected	k	Observed	Expected
0	22	8.13	0	39	33.96	0	22	16.18
1	11	15.67	1	11	16.99	1	11	14.84
2	8	15.12	2	3	4.25	2	8	10.47
3	3	9.48	3	1	0.71	3	3	6.61
4	3	4.69	4	2	0.09	4	3	3.86
5	3	1.81				5	3	2.07
6	1	0.58				6	1	1.06
7	3	0.16				7	3	0.50
8	1	0.04				8	1	0.22
9	0	0.01				9	0	0.11
10	1	0.00				10	1	0.06
Chi square=143.788			Chi square=9.278			Chi square=24.866		
df=6			df=3			df=6		

¹ Categories 5 and 6 were collapsed as were categories 7-10 in order to calculate the chi square.

² Categories 3 and 4 were collapsed to calculate the chi square.

Table 2. Rates of Disorders as a Function of Ethnic Characteristics of Students and Teachers

	Political Disorders		Non-political Disorders	
	Zero-order r	Beta	Zero-order r	Beta
Percentage Black students	.542	.340	.380	.466
Percentage Black teachers	.535	.318	.160	-.169
Percentage Puerto Rican students	.326	-.005	.145	.055
		$r^2 = .355$		$r^2 = .158$

responds in part to that made between inputs into organizations and structural characteristics of organizations. The exogenous causes of behavior in this instance refer to the ethnic attributes of the pupils and staff. Given the political climate in the nation and the city there was every reason to expect that rates of political disorders would be a function of the rates of black pupil enrollment and black teacher employment in the schools, as is demonstrated in Table 2. Much of the civil rights struggle had focused on the schools, and we believe that the political disorders in the high schools reflected the salience of the issues in society at large. By 1968 the state of the schools had become a major political issue in black communities and particularly among many black teachers. There had been a marked shift in sentiment from racial integration to self-determination through community control. The National Association of Afro-American Educators had gone on record in favor of community control.⁴

In New York City the local African

American Teachers Association became a major factor in the community control dispute. The Harris and Swanson Poll (1970) of New Yorkers' opinions on the politics of the school system showed the black population to be the most aroused and concerned of any ethnic group in the city. Interestingly enough, these same poll data show the Puerto Ricans to be the least aroused group in the city. Puerto Ricans and blacks tend to be enrolled in the same high schools, thus the zero order correlations show a relationship between rates of Puerto Rican enrollment and rates of disorders. However, the regression coefficients show no Puerto Rican effect once the black variables are entered.⁵

The effect of black teachers is interesting and complex. They seem to have played a completely different role in the two sorts of disorders. Their presence tended to increase the probability that political disruptions would occur, while they slightly inhibited the occurrence of non-political disorders. We believe that black teachers may have "politicized" black pupils (particularly through the Afro-American clubs) in the schools.⁶ The data presented are consistent

⁴ The ethnic political climate in and around New York's schools during this period is discussed in several papers of the senior author. We have not tried to generalize the New York City data to the nation; however, using a national sample, Bailey (1970:10-11) reports that schools in the North were more likely to have experienced racially based political disorders than were schools in the South. Since we are suggesting that school disorders reflect the racial political climate in the larger society, it is significant that Bailey's findings are consistent with Spillerman's (1971) report of North-South differences in racially based civil disorder. Bailey's findings suggest that the etiology of group political disorders presented in this paper for New York City academic high schools may well be the same for high schools in other Northern cities.

⁵ Approximately 1.3% of the teachers of New York's high schools are "hispanic," while 4% of the teachers are black. The "hispanic" teachers are distributed much more evenly throughout the school system than are the black teachers. The proportion "hispanic" on the teaching staff is unrelated to political disorders ($R = .009$) and negatively related to non-political disorders ($R = -.157$).

So, too, though there is a zero order correlation of .36 between social class as measured by welfare population and rate of political disorders, this correlation disappears when controlled for rate of black student enrollment.

⁶ Results of participant observation were reported to us by a former member of the New York City

with these observations. The black teachers may have inhibited the occurrence of non-political events by channeling some of the energy of the black pupils into political activity and protest.

Given black pupil and black teacher populations as the key independent variables, Table 2 shows that the distribution of political disorders is far more determinant than is that of non-political disorders as measured by per cent variance explained. In the language of the previous section, the distribution of non-political disorders would appear to have a significantly higher random component as was predicted from the deviations from the Poisson distributions.

3. System Effects

System effects are of two sorts, namely the dynamics of the disorders themselves and the custodial and academic characteristics of the schools. The effects of both these system characteristics will be examined in this Section.

3.a. The Dynamics of Disorders

In our presentation of the random model we indicated that there were three sources of deviation from the Poisson distribution; namely heterogeneity, non-independence of events (reinforcement and contagion) and variation over time. In the Second Section we were able to show the effects of heterogeneity particularly with regard to political disorders.

Planning Commission. This observer reported that black students were in fact being politicized through Afro-American clubs led by black teachers. In at least one instance during this period, the District Attorney brought charges against a faculty advisor to the Afro-American Cultural Club "for masterminding riotous school disruptions by black students . . ." (New York Times, May 20, 1969).

A referee asked, "Since race is not one of the variables given in the data, how do we know that most of the disorders were not instigated by white students?" In a review of newspaper stories (*New York Times*) for the period, we found twenty-one events reported which clearly meet the criteria for group political disorders noted above. Of these sixteen were identifiable as black student protests, and in five instances race was not specified. Thus, at least three-fourths of the events reported in the newspaper were black student political disorders. In no case was it reported that white students instigated the political disorders.

The heterogeneity solution does not preclude the possibility that reinforcement as well had taken place.

Contagion or reinforcement is premised on the notion that whatever the source of the initial disturbance, the probability that each subsequent event will occur is a function of the occurrence of the previous event as well as the exogenous variable(s). The assumption of heterogeneity without reinforcement (implicit in the previous section) is statistically simple and neat but runs counter to plausible alternative explanations of the phenomenon. Consider the following. If the black students constitute the major pool of dissatisfaction and disrupters, then by analogy we may consider them the "capital" input needed to produce disorders. The non-reinforcement model assumes a constant marginal value for the capital. The presumed disorder proneness of the black students, neither increases nor decreases as a consequence of previous disorders. If political disorders are rooted in students' grievances about school conditions (e.g. the felt absence of a black studies program), then one would expect that the response of the administration would affect the course of subsequent events and thus the rate of events. For example, if earlier demonstrations led to the administration's meeting the students' demands, it would be reasonable to expect some change in the likelihood of occurrence for the next event. The administration's acquiescence might "consume" some of the initial "capital" of black frustration, or alternately (and perhaps more probably), administrative acquiescence might result in increasing demands. Similarly, rejecting students' demands too might be expected to alter the probability of further demonstrations.

How can we determine whether the deviation from the distribution expected based on Poisson assumptions is attributable to reinforcement? Coleman (1964:229-307) initially suggests modifying the Poisson distribution by introducing another parameter to take into account contagion or reinforcement. However, it has been shown that the distribution "... derived from assumptions of contagion, and the ... distribution, derived from assumptions of heterogeneity and no contagion are identical." Coleman con-

cludes, therefore, that demonstration of the presence of heterogeneity or contagion requires "... over time data, which can show the development of contagion if it exists" (301). In the case at hand, such data are not available and thus a direct test is not possible. However, an indirect test can be made by modifying the initial Poisson assumptions by introducing another piece of information.

The initial Poisson distributions were generated under assumptions of homogeneity and the absence of contagion and reinforcement. The distribution was generated by two items of information, the number of events and the number of units in the system. We now propose to introduce a third bit of information which will take into account the heterogeneity shown above (see Section II). The initial Poisson distributions assumed equal disorder proneness for all schools where $\lambda = \bar{X}_K$, i.e. disorder proneness was estimated by mean events in the system. We shall now generate a distribution with a variable disorder proneness value as a function of black pupil population for each school. That is, the assumption of homogeneity will be relaxed while the other assumptions are maintained.

Relaxing the assumptions of homogeneity was accomplished by using the prediction equation obtained by regressing political disorders on black students.⁷ Comparing Columns A and C in Table 1 show the results of the adjusted distribution. The improvement is visually apparent and is expressed as well in the chi-square values. The initial chi-square value for the Poisson distribution was 143.79; the adjusted $\chi^2 = 24.87$. These results are consistent with the thesis that black enrollment played the major role in generating political disorders. They

suggest that the other Poisson assumptions can be maintained. One would also expect that additional variables would not significantly add to the model. The section on system effects (below) will examine this inference.

3.b. Educational and Custodial Characteristics of the Schools

The absence of a reinforcement effect is consistent with the thesis that political disorders are unrelated to the response of the school administration to the students' grievances. Does the school make no independent contribution? The educationist literature would lead one to expect the schools to contribute greatly to disorder. One paper suggests that "... student unrest has been at least in part a rejection of the gigantic" (Leggett et al., 1970). An early study (Wattenberg 1936:99, cited in Cole 1969:29) suggested that discipline in the high schools deteriorated during the depression as class size increased and the rate of attendance of lower class pupils increased.

Perhaps disorder (of both sorts) is a response to academic failure and the irrelevance of the school system to the students' needs and interests. Equally plausible is the notion that disorder occurs when inexperienced teachers (who tend to teach the non-white pupils) cannot capture their pupils' interest and/or cannot control them. However, no matter what the school characteristic, whether size (presumably implying bureaucratization and impersonality) or academic failure (with the inference of frustration) or teacher inexperience, even when a zero order correlation appears, it is demonstrably spurious. No school characteristic seems to make any significant difference in generating political disorders. (See Table 3.)

⁷ The unstandardized regression coefficient is the effect parameter for the rate of black student enrollment. Each school's disorder proneness (λ) is estimated by the product of rate of black student enrollment and the regression coefficient, a practice which results in a unique λ value for each school. For every school in the population a unique probability of 0 ... n ($n=10$) events was computed. For each possible number of events per school, i.e. 0 ... 10, we then sum across schools. This yields the number of schools expected to experience the indicated number of events under the assumptions stated.

SUMMARY AND CONCLUSIONS

Our initial problem was to determine the extent (if any) to which school characteristics could account for variations in rates of school disorders. The only measured characteristics which we found to be significant were the ethnic composition of the student body and the teaching staff. The data support the thesis that disorder proneness was a function of

Table 3. Correlations of School Characteristics with Disorders

	Political Disorders		Non-political Disorders	
	Zero-Order	Partial by Percentage Black Pupils	Zero-Order	Partial by Percentage Black Pupils
Enrollment	.002	.017	-.140	-.143
Utilization	.012	.162	.021	.118
Age of school building	-.093	-.045	.106	.157
Achievement level	-.360	-.059	-.205	.026
Drop-out rate	.319	.012	.135	-.110
Pupil-teacher ratio	.192	.020	.132	.016
Average class size	-.288	-.066	-.115	.062
Teacher seniority	-.360	.006	-.069	-.035
Pct. tenured teachers	-.161	-.011	-.132	-.029

individuals and their interactions, not of the school system. The greater the proportion of such individuals in a school the higher the incidence and rate of disruption. Variations in measured school educational and organizational characteristics had no effect on rates of disorders. Political disorders occurred in locations in which "politicizers" (i.e. black teachers) and "polticizees" (i.e. black pupils) were brought together in numbers large enough to make a difference. The schools did not seem to create their own political climate as much as they reflected the political climate of the larger society.

REFERENCES

- Bailey, Stephen K.
1970 *Disruption in Urban Public Secondary Schools*. Washington: National Association of Secondary School Principals.
- Cole, Stephen
1969 *The Unionization of Teachers*. New York: Praeger.
- Coleman, James S.
1964 *Introduction to Mathematical Sociology*. New York.
- Coleman, James S. et al.
1966 *Equality of Educational Opportunity*. Washington: United States Government Printing Office.
- Harris, Louis and Bert E. Swanson
1970 *Black-Jewish Relations in New York City*. New York: Praeger.
- Hunt, Jane
1969 "Principals report on student protest." *American Education* (October):4-5.
- Leggett, Stanton et al.
1970 "The case for a small high school." *Education Digest* 36(November):15-18.
- Pucinski, Roman C.
1970 "Results of survey on student unrest in the nation's high schools." *Congressional Record* (February):E1178-E1180.
- School Stability Resource Team
1971 *Stability and Disruption in the Public Schools of New York City*. New York: Board of Education.
- Scott, Joseph W. and Mohamed El-Assal
1969 "Multiversity, university size, university quality and student protest." *American Sociological Review* 34(October):702-9.
- Spillerman, Seymour
1970 "The causes of racial disturbance: a comparison of alternative explanations." *American Sociological Review* 35(August):627-49.
- 1971 "The causes of racial disturbance: tests of an explanation." *American Sociological Review* 36(June):427-42.
- Westin, Alan F. et al.
1970 *Civic Education in a Crisis Age: An Alternative to Repression and Revolution* (mimeographed). New York: Center for Research and Education in American Liberties, Columbia University and Teachers College.

SYSTEM SIZE AND RULING ELITES *

BRUCE H. MAYHEW

Temple University

American Sociological Review 1973, Vol. 38 (August):468-475

A baseline model is developed to show that, when other variables are held constant, Mosca's proposition on the relationship between the relative size of ruling elites and the sizes of the systems they govern follows from the definitions of the variables. The model provides a baseline against which empirical trends may be assessed.

The implications of this model for the much studied relationship between administrative ratios and organizational size are discussed. While the model cannot be applied to this relation on a purely definitional basis, it can be applied in conjunction with Michels' prediction of minority rule. If Michels' prediction holds, the observed results from most studies can be deduced from the baseline model. Michels' expectation is sufficient to account for the general nature of the relationship between the relative size of the administrative component and organizational size. Some specific models are illustrated to indicate the effects of observing various maximum relative sizes of administrative components.

INTRODUCTION

MOSCA'S (1939) hypothesis on the relationship between the relative size of ruling elites and the size of the social systems they govern has received wide currency (Svalastoga, 1964:534-5; and see references below). According to Mosca (1939:53):

... the larger the political community the smaller will be the proportion of the governing minority to the governed majority. . . .

Perhaps a better rendition would be:

Proposition: The relative size of a ruling elite is a decreasing function of the size of the system it governs.

The object of the present paper is to show that this proposition follows from (1) the definitions of the variables (a) *system size* and (b) *relative size of the ruling elite*, and (2) the *ceteris paribus* condition required of all propositions in an observational science. From the development of the model I will construct to show this, it will become apparent that Mosca's definition of the ruling elite as a *minority* of the system's population is critical. For this reason, it is appropriate to indicate the prevalence of this view before proceeding.

Mosca's definition of the ruling elite as a minority of the system's population is con-

sistent with Michels (1962), Mills (1956), Zeigler and Dye (1969), and Prewitt (1969). An issue of the *American Behavioral Scientist* was recently devoted to the topic; especially relevant are the observations by Zeigler and Dye, the editors of the issue, and Prewitt. According to Zeigler and Dye (1969:167): "In all societies, under all forms of government, the few govern the many. This is true in democracies as well as dictatorships. Elites are the few who govern; the masses are the many who are governed." According to Prewitt (1969:169):

It is common to all forms of government that the few govern the many. Whether governors hold authority by virtue of the consent of the many, through arbitrary usurpation, or from long-established tradition, and whether the governed look upon the governors with pride, indifference, or hostility, the governors are few in comparison with the populations they govern.

Prewitt (1969:170) continues with a citation from Bryce (1924:542):

In all assemblies and groups and organized bodies of men, from a nation down to a committee or a club, direction and decisions rest in the hands of a small percentage, less and less in proportion to the larger size of the body, till in a great population it becomes an infinitesimally small proportion of the whole number. This is and always has been true of all forms of government, though in different degrees.

Although the object of the above quote is to indicate the assumption of minority rule,

* The Author is indebted to Leo C. Rigsby of Vanderbilt University for helpful comments on the original draft of this paper.

Bryce clearly gives his own statement of Mosca's proposition, and claims that it has universal validity. In citing Bryce's statement, Prewitt (1969:170) continues:

Bryce articulates an assumption with a long history in political analysis. Whatever might be the state's wealth, its size, its permanence, or its purpose, a few of its citizens are always called upon to govern the remainder. This axiom figures prominently in theoretical writings as divergent as those of Marx and Mosca, Pareto and Plato, Lasswell and Lenin, Madison and Machievelli. It is probably the most written about axiom in all political theory.

Whatever the case for contemporary political systems, the ruling elite certainly seems to have been a minority in pre-industrial states (see Sjoberg, 1960 and Eisenstadt, 1963).

For purposes of the present paper, Mosca's proposition will be considered as applicable to those political systems that have ruling elites. That is, I do not claim that all political systems have ruling elites.

A MODEL FOR MOSCA'S PROPOSITION

The size (S) of a social system is the number of persons who comprise it, and the size (E) of the ruling elite is the number of persons who constitute the ruling *minority*. Accordingly, the relative size (r) of the ruling elite is $r = E/S$. Both social systems and their ruling elites are considered as organized entities, not individuals, so that it is necessarily the case that $S > 1$ and $E > 1$. Because Mosca defines a ruling elite to be a minority of the system's population (all definitions used here are from Mosca, 1939), we must have $2 < E < S/2$, while size has the general range $5 < S < \infty$. A social system smaller than five cannot have a minority that meets the condition that $E > 1$.

With these definitions it is possible to construct a baseline model for the relationship between the relative size of ruling elites and system size. This is accomplished by assigning a uniform probability density function to the integers in the range $2 \leq E < S/2$ for each value of S . This function gives each value of E the same likelihood of occurrence in a population of size S . By definition it establishes the *ceteris paribus* condition for the relationship between the relative size of the ruling elite and system size and permits

the derivation of an expected relationship between r and S .

Accordingly, let $s = 1, 2, 3, \dots, N$ be an index for integers in the range $2 \leq E < S/2$ let v_s be the magnitude of E assigned to s and let p_s be the probability of s . By definition

$$p_s = 1/N, \quad (1)$$

we can further define

$$A = \sum_{s=1}^N p_s v_s \quad (2)$$

to be the expected value of E for any given S . From the definitions given it is easy to see that when S is odd

$$A = S/4 + 3/4, \quad (3)$$

and when S is even

$$A = S/4 + 1/2, \quad (4)$$

so that we shall have two functions, one for odd values of S and one for even values of S . And we have

$$\delta A / \delta S = 1/4 \quad (5)$$

when S is odd or even. And, from the definition $r = E/S$ we can now arrive at an expected value of r (symbolized R) for any, given S :

$$R = \frac{S/4 + 3/4}{S}, \quad (6)$$

when S is odd, and

$$R = \frac{S/4 + 1/2}{S}, \quad (7)$$

when S is even. Furthermore, when S is odd

$$\delta R / \delta S = -3/4 S^2, \quad (8)$$

and

$$\delta^2 R / \delta S^2 = -3/2 S^3. \quad (9)$$

Similarly, when S is even

$$\delta R / \delta S = -1/2 S^2 \quad (10)$$

and

$$\delta^2 R / \delta S^2 = 1/S^3. \quad (11)$$

Further, for S odd or even

$$\lim_{S \rightarrow \infty} R = 1/4. \quad (12)$$

THEOREMS

The model constructed permits the derivation of a set of theorems for the relationship between the absolute size of the ruling elite and system size as well as for the relationship between the relative size of the ruling elite and system size. It will be understood that each theorem is stated separately for S odd and S even.

Theorem 1: Ceteris paribus, the absolute size of the ruling elite is a monotone increasing function of system size.

This is clear from equation 5.

Theorem 2: Ceteris paribus, the rate of increase in the absolute size of the ruling elite with respect to size is a constant.

In fact, equation 5 indicates that it is $\frac{1}{4}$.

Theorem 3: Ceteris paribus, as system size increases without bound, the relative size of the ruling elite increases without bound.

This is also apparent from equation 5, that is $\frac{\partial^2 A}{\partial S^2} = 0$.

Theorem 4: Ceteris paribus, the relative size of the ruling elite is a monotone decreasing function of system size.

This is clear from equations 8 and 10.

Theorem 5: Ceteris paribus, the rate of

decline in the relative size of the ruling elite with respect to size is a monotone increasing function of system size,

This is indicated in equations 9 and 11.

Theorem 6: Ceteris paribus, as system size increase without bound, the relative size of the ruling elite approaches a constant.

Equation 12 shows this value to be $\frac{1}{4}$.

Theorem 4 is, of course, Mosca's proposition. Bryce's (1924) confidence in its validity appears to be justified; it will hold by chance alone. However, since the model operates differentially on odd and even values of size, the regression of R on S will exhibit an ever decreasing 'bounce' over the total range of S . The least squares regression line for the relation between R and S will conform to the proposition, so that Mosca's expectation holds for all values of size.

Table 1 illustrates values of A and R for selected values of S , and Figure 1 provides

Table 1. Expected Size of the Ruling Elite (A) and Expected Relative Size of the Ruling Elite (R) for Selected Values of Size (S)

S		A	R(odd)	R(even)	S		A	R(odd)	R(even)
Odd	Even				Odd	Even			
5,	6	2.0	.4000	.3333	39,	40	10.5	.2692	.2625
7,	8	2.5	.3571	.3125	41,	42	11.0	.2683	.2619
9,	10	3.0	.3333	.3000	43,	44	11.5	.2674	.2614
11,	12	3.5	.3182	.2917	45,	46	12.0	.2667	.2609
13,	14	4.0	.3077	.2857	47,	48	12.5	.2659	.2604
15,	16	4.5	.3000	.2813	49,	50	13.0	.2653	.2600
17,	18	5.0	.2941	.2778
19,	20	5.5	.2895	.2750	59,	60	15.5	.2627	.2583
21,	22	6.0	.2857	.2727
23,	24	6.5	.2826	.2708	69,	70	18.0	.2609	.2571
25,	26	7.0	.2800	.2692
27,	28	7.5	.2778	.2679	79,	80	20.5	.2595	.2563
29,	30	8.0	.2759	.2667
31,	32	8.5	.2742	.2656	89,	90	23.0	.2584	.2556
33,	34	9.0	.2727	.2647
35,	36	9.5	.2714	.2639	99,	100	25.5	.2576	.2550
37,	38	10.0	.2703	.2632					

graphic illustration of the decay functions indicated in equations 6 (Figure 1-a) and 7 (Figure 1-b) for selected values of S . Table 1 indicates that R will be near its minimum value at $S=100$. For large values of S the relation between R and S will exhibit a very small negative slope. The tendency of the relation to level off at larger values of S is clearly indicated in Figure 1.

GENERAL IMPLICATIONS

To briefly summarize, the baseline model developed here shows that when other variables that may be expected to affect the relative size of ruling elites are held constant—the *ceteris paribus* condition—Mosca's proposition on the relation between the relative size of ruling elites and the size of the systems they govern follows from the definitions of the variables. Accordingly, we make this prediction even if it had never been empirically observed.

The baseline model provides a gauge against which it will be possible to compare empirical trends in order to determine whether variables other than size affect the

relative size of ruling elites, or whether size itself has an effect in excess of that provided by the model. These comparisons may be made by constructing a new variable expressing the difference between observed values of r and R for any given S . That is, if the variable

$$\Delta = |R - r| \quad (13)$$

exhibits a functional relationship to S , this would indicate that size has an effect above and beyond creating a sample space within which r varies randomly. Taking the signed value of the difference would permit determination of whether observed r 's are greater or less than chance expectation as indicated by the model.¹

Following Rapoport (1949) and Childers, et al. (1971), it is appropriate to indicate that empirical results which conform to the predictions of baseline models are amenable to two alternative interpretations. One is that the phenomena indeed occur randomly. The other is that a variety of countervailing forces impinge upon the dependent variable in such a way as to produce this balanced outcome. Most sociologists would tend to accept the second view, regarding it as a starting point for research directed to disentangling the effects of diverse variables.

Another important point has to do with the level of system organization to which the baseline model is applied. Table 1 might give

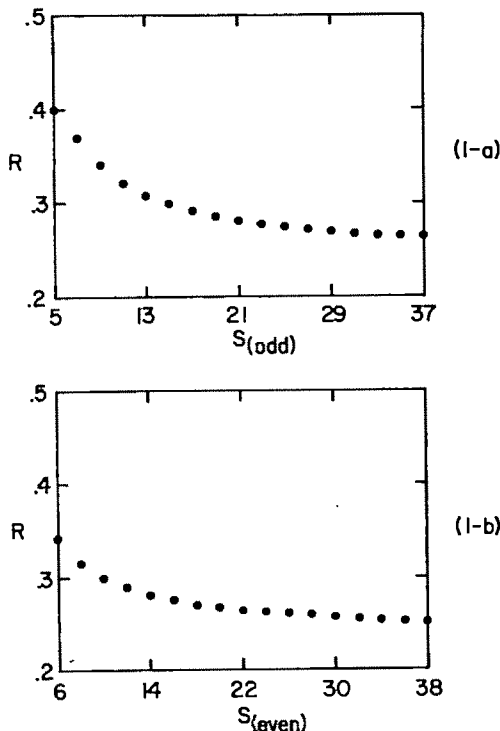


FIGURE 1. RELATIONSHIP BETWEEN THE RELATIVE SIZE OF THE RULING ELITE (R) AND SYSTEM SIZE, FOR ODD ($S_{(odd)}$) AND EVEN ($S_{(even)}$) VALUES OF SIZE.

¹ The probability density function defined over the relevant set of minority producing integers for a given S need not be uniform in order to produce the results of the present model. It may be normal or (more accurately) the binomial approximation to the normal distribution. This alternative assumption for the density function may be preferred in assessing empirical departures from the baseline's expectations because (1) most sociologists probably expect a normal distribution to be more reflective of empirical trends (cf. Childers, et al., 1971) and (2) it permits assessment of departures from the baseline's expected values in terms of standard units of the normal curve, so that it would be possible to see whether observed values fall within one standard deviation of expectation 66% of the time, within two standard deviations 95% of the time, etc. The analysis is simplified by expressing the Δ values of equation 13 in terms of standard deviation units. As indicated, the use of the normal probability density function yields identical expectations to those provided by the uniform density function; equations 3-12, and therefore the theorems indicated in the text, are identical for both types of probability density functions.

the impression that the present model has applicability only to very small systems, since relatively little decline in R occurs beyond $S = 100$. However, an understanding of the way in which administrative control systems in political communities are organized mitigates this impression. Large political systems, such as states and empires, are made up of smaller units. If the system is sufficiently large, it will be comprised of relatively small sub-units nested within larger sub-units which will in turn be nested within still larger sub-units, *etc.*, telescoping the number of sub-systems within sub-systems as size increases. Since, as in Bryce's (1924: 542) observation cited earlier, Mosca's proposition is expected to hold at all levels of political organization, it need only hold for small units—say in the range five to one hundred—to be magnified to hold over a range of one hundred to one thousand at the next higher level of organization, or over a range of one thousand to ten thousand at the next higher level. Because large political systems are comprised of smaller ones, the rates of decline in r observed over a range of say ten thousand to one million need be nothing more than the result of the systems' internal composition of smaller units for which the model holds. Thus, it would be a fallacy to think that the model does not tell us what is happening in systems of size greater than one hundred. This same line of reasoning applies with equal force to the structure of formal instrumental organizations discussed in the following section.

SPECIFIC IMPLICATIONS

The question arises as to whether the present model tells us anything about the much studied relationship between (1) the relative size of the administrative component in instrumental formal organizations and (2) the size of the organizations. Most studies have found a negative relationship and recent studies in particular indicate that the form of the relation is a curvilinear decay function (for an overview of the literature see Akers and Campbell, 1970; Klatzky, 1970; and Blau and Schoenherr, 1971, and references in each).² Both of these trends are consis-

tent with the present model's predictions.

Initially, it may be observed that students of the subject *do not* define the administrative component to be a minority.³ And, unless it is so defined, the present model does not apply on the basis of definitions alone.

However, by invoking Michels' (1962) "Iron Law of Oligarchy," we can arrive at the same general results. That is, for a given set of data on organizations, if it is observed that the relative size of the administrative component is in fact less than $\frac{1}{2}$ for all sizes, verifying Michels' prediction, then we can use the observed fact, rather than the "minority definition" to apply the present model. Accordingly, in such cases no explanation of why the relative size of the administrative component declines at a decelerating rate is required—except to the extent that the rate of decline departs from the baseline prediction. Rather, what requires explanation in such cases is the observed fact of minority size of the administrative component. For resolution of this question, I refer the reader to Michels (1962) and his many detractors (cf. Cassinelli, 1953; Lipset, 1962).⁴

Unfortunately, their practice of throwing out more than four hundred small systems before performing one of their analyses and a lesser number of small systems before performing the other casts considerable doubt on the findings. Two more recent studies which offer hints of a lack of a negative relationship under certain circumstances are similarly suspect because of the size measures used. Holdaway and Blowers (1971) use a subset of all employees, rather than total system size; while Hendershot and James (1972) use volume of activity (number of students enrolled), rather than total number of employees.

³ Even when theoreticians are discussing phenomena that might be called "ruling groups" in organizations—without respect to whether the constituent members of the group are administrative personnel—they usually do not define these groups to be minorities. For example, Thompson's (1967) discussion of a "dominant coalition" in formal organizations specifically indicates that the coalition may not be a minority.

⁴ I am interpreting Michels' (1962) "Iron Law of Oligarchy" to be a probabilistic prediction of the form:

Proposition: In the evolution of formal instrumental organization through time, the outcome of the process of structuring authority relations in the systems is a structure in which a minority of the system's personnel occupy positions from which the organization's activity is directed.

As long as this statement is interpreted in the con-

² The most cited exception to a negative relationship is from the study by Terrien and Mills (1955).

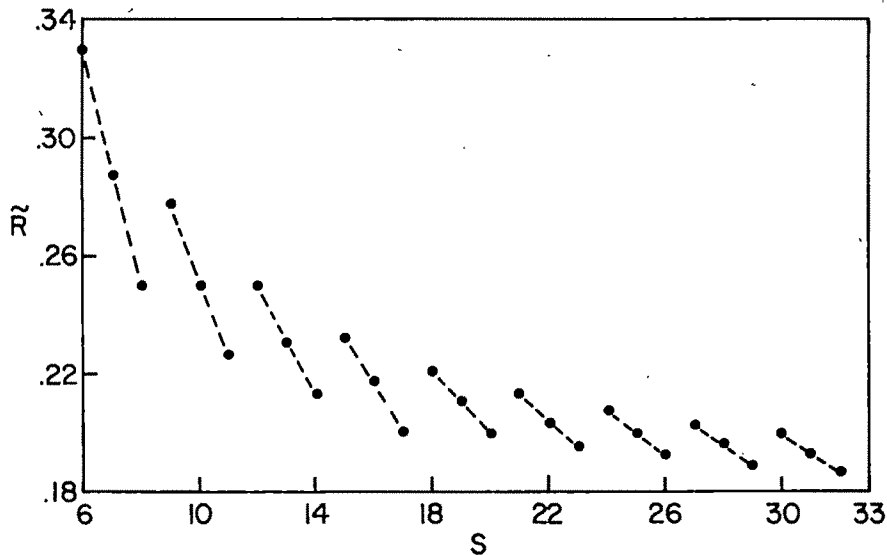


FIGURE 2. RELATION BETWEEN EXPECTED RELATIVE SIZE OF THE ADMINISTRATIVE COMPONENT (R) AND SYSTEM SIZE (S) IN ORGANIZATIONS IN WHICH THE MAXIMUM OBSERVED r IS $\frac{1}{3}$, FOR $6 \leq S < 33$.

Not only can we use the observed fact of minority size for the administrative component, we can actually use the maximum observed value of r to establish a more refined model for the set of data in question.⁵ This is accomplished by defining a uniform probability density function over the integer range $2 < E < (r_{\max}) S$ for each value of S , where r_{\max} is the maximum observed r in the data.⁶ Following the same procedure indicated in construction of the original baseline,

text of McFarland's (1969) discussion of centralized and decentralized decision structures, no confusion should arise. And, the proposition places Michels' prediction in the more specific context of recent theory and research on the subject and indicates that supervisory personnel (as used in Blau, 1970 and Blau and Schoenherr, 1971) correspond to the administrative component for which this minority prediction of a ruling elite is made.

Meyer (1971) notes that the assumption (or finding) of hierarchical symmetry in organizations (assuming a span of control greater than 1) yields a negative relation between relative size of administrative components and size. I note that the same assumption makes all administrative components minorities. Further, assuming a uniform span of control within each level, a downward increasing span of control also yields an administrative minority.

⁵ The reader will appreciate the fact that in any case where r cannot vary over the full range $0 \leq r \leq 1$, a restricted sample space of integers serves as the basis for models of the type discussed here. My concentration on instances of $r < \frac{1}{2}$ comes from Mosca's minority definition and Michels' minority prediction.

⁶ For continuity and simplicity of illustration I

we can arrive at an expected value for the relative size of the administrative component (symbolized R) for any given S .

Figures 2, 3, and 4 indicate the relations between R and S for sets of hypothetical data in which r_{\max} is $\frac{1}{3}$ (the apparent maximum reported in Blau and Schoenherr, 1971: 86), $\frac{1}{4}$, and $\frac{1}{5}$, respectively ($\frac{1}{3}$ is the approximate r_{\max} reported by Klatzky, 1970:

am continuing to use a minimum value of $E > 1$. However, for administrative components in organizations, this is not necessary. Students of the subject do not require that administrative components have more than one person. Changing the sample space to include a minimum value of $E = 1$ gives an interesting result for the original form of the general model (that is, for the restriction that $r < \frac{1}{4}$). This is that $R = \frac{1}{4}$ for all even values of $S > 2$, while behaving in the usual manner—a decline at a decelerating rate—for odd values of $S > 1$. Since the least squares regression line created by these values over the complete range of S is a curvilinear decay function, the model yields the same general expectation for empirical trends, but one which depresses the expected strength of the relationship between the variables (a relation which improves under a logarithmic transformation of S). This form of the model is, perhaps, even more consistent with the results of empirical studies on organizations than some of the illustrations provided subsequently in the text. However, the constant value of R for even values of $S > 2$ will occur only in the instance cited. The extension of the integer range to include $E = 1$ does not affect the outcomes of the modified models based upon observed r_{\max} subsequently illustrated in the text. The general results will be the same; only the specific values of R and the range of S over which the models can be defined will differ.

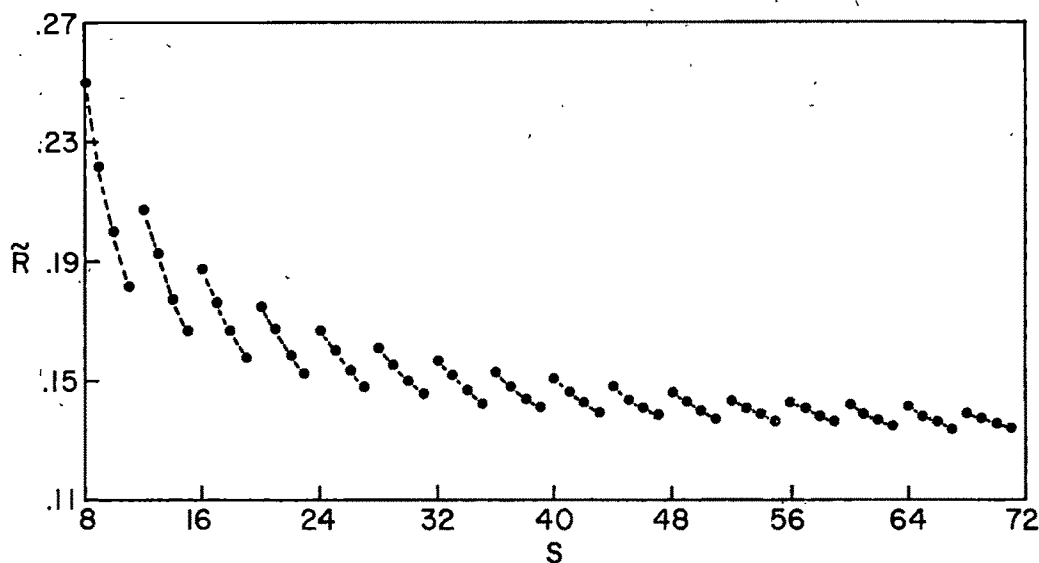


FIGURE 3. RELATIONSHIP BETWEEN EXPECTED RELATIVE SIZE OF THE ADMINISTRATIVE COMPONENT (R) AND SYSTEM SIZE (S) IN ORGANIZATIONS IN WHICH THE MAXIMUM OBSERVED r IS $\frac{1}{4}$, FOR $8 \leq S < 72$.

437).⁷ One obvious result of the illustrations is the decreasing amplitude of the bounce in R for models based upon smaller and smaller values of r_{\max} . The bounce created in R and

⁷ The (approximate) values of r_{\max} cited here from Blau and Schoenherr (1971) and Klatzky (1970) are different because I am citing the value for the supervisory ratio at headquarters from Blau and Schoenherr (1971:86), and not their staff ratio (1971:88) which is used by Klatzky.

the shifting slopes of the indicated sequences of values created by these modified models may help to account for the relatively weak correlations reported in much of the literature. They could help to account for the better fit of data to a logarithmic transformation of size, possibly clearing up some theoretical questions posed by Klatzky (1970). Indeed, the scattergram of Figure 4 looks

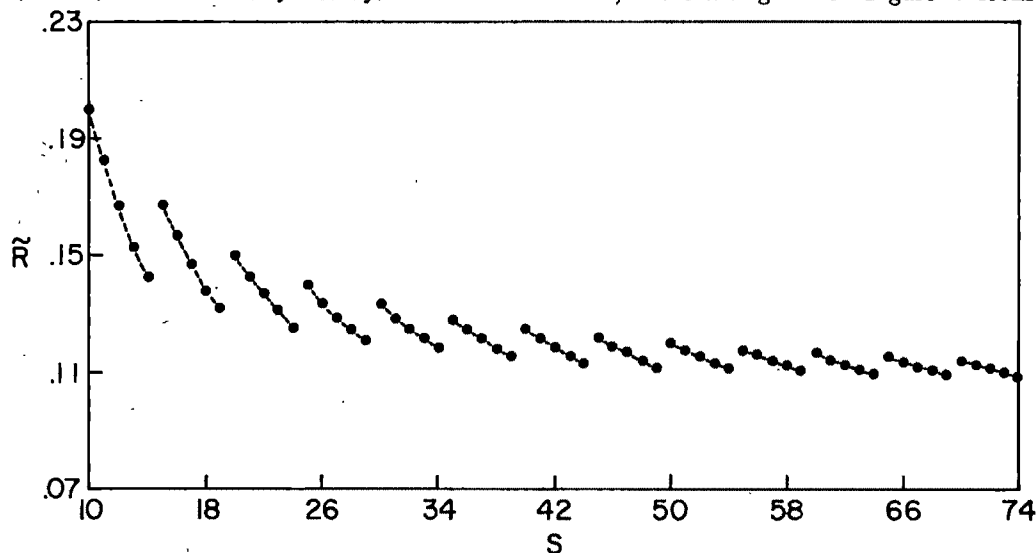


FIGURE 4. RELATIONSHIP BETWEEN EXPECTED RELATIVE SIZE OF THE ADMINISTRATIVE COMPONENT (R) AND SYSTEM SIZE (S) IN ORGANIZATIONS IN WHICH THE MAXIMUM OBSERVED r IS $\frac{1}{5}$, FOR $10 \leq S \leq 74$.

strikingly like those constructed by Klatzky (1970:432) to indicate possible confounding effects of other variables. The results she observed may be due as much to (1) the observed fact of an r_{\max} of approximately $\frac{1}{2}$ and (2) chance variation as indicated in the present modified model, as to the apparent confounding effects of other variables. I do not claim that this is so; I am merely pointing out the possibility of this interpretation. An answer can only be obtained by comparing the actual data with the appropriate baseline. However, one thing is quite clear; given verification of Michels' prediction, the general nature of the results obtained by Blau and Schoenherr (1971) and Klatzky (1970) can be logically deduced from the baseline models presented here.

To summarize the specific implications for the relation between the relative size of the administrative component and organizational size, we cannot apply the original baseline model by virtue of the minority size definition, but we can apply it by virtue of the observed fact of minority size of the administrative component. The illustration of modified models constructed from hypothetical data indicates new possibilities for interpretation of the relation between the relative size of administrative components and organizational size.

REFERENCES

- Akers, R. and F. L. Campbell
1970 "Size and the administrative component in occupational associations." *Pacific Sociological Review* 13 (Fall):241-51.
- Blau, P. M.
1970 "A formal theory of differentiation in organization." *American Sociological Review* 35 (April):201-18.
- Blau, P. M. and R. A. Schoenherr
1971 *The Structure of Organizations*. New York: Basic Books.
- Bryce, J.
1924 *Modern Democracies*. New York: Macmillan.
- Cassinelli, C. W.
1953 "The law of oligarchy." *American Political Science Review* 47 (September):773-83.
- Childers, G. W., B. H. Maybaw and L. N. Gray
1971 "System size and structural differentiation in military organizations: Testing a baseline model of the division of labor." *American Journal of Sociology* 76 (March):813-30.
- Eisenstadt, S. N.
1963 *The Political Systems of Empires*. New York: Free Press.
- Hendershot, G. E. and T. F. James
1972 "Size and growth as determinants of administrative-production ratios in organizations." *American Sociological Review* 37 (April):149-53.
- Heldaway, E. A. and T. A. Blowers
1971 "Administrative ratios and organization size: a longitudinal examination." *American Sociological Review* 36 (April):278-86.
- Klatzky, S. R.
1970 "Relationship of organizational size to complexity and coordination." *Administrative Science Quarterly* 15 (December):428-38.
- Lipset, S. M.
1962 "Introduction." Pp. 15-39 in Robert Michels (ed.), *Political Parties*. New York: Free Press.
- McFarland, A. S.
1969 *Power and Leadership in Pluralistic Systems*. Stanford: Stanford University Press.
- Meyer, M. W.
1971 "Some constraints in analyzing data on organizational structures: a comment on Blau's paper." *American Sociological Review* 36 (April):294-7.
- Michels, R.
1962 *Political Parties*. New York: Free Press.
- Mills, C. W.
1956 *The Power Elite*. New York: Oxford University Press.
- Mosca, G.
1939 *The Ruling Class*. New York: McGraw-Hill.
- Prewitt, K.
1969 "From the many are chosen the few." *American Behavioral Scientist* 13 (November/December):169-87.
- Rapoport, A.
1949 "Outline of a probabilistic approach to animal sociology: I." *Bulletin of Mathematical Biophysics* 11 (September):183-96.
- Sjoberg, G.
1950 *The Preindustrial City*. New York: Free Press.
- Svalastoga, K.
1954 "Social differentiation." Pp. 530-73 in R. E. L. Faris (ed.), *Handbook of Modern Sociology*. Chicago: Rand McNally.
- Terrien, F. W. and D. L. Mills
1955 "The effect of changing size upon the internal structure of organizations." *American Sociological Review* 20 (February):11-13.
- Thompson, J. D.
1967 *Organizations in Action*. New York: McGraw-Hill.
- Zeigler, H. and T. R. Dye
1969 "Editors note." *American Behavioral Scientist* 13 (November/December):167-8.

SIZE AND RULING ELITES: EFFECTS OF SYSTEM GROWTH ON POWER STRUCTURES

MAXIMILIAN H. VONBROEMSEN

LOUIS N. GRAY

Washington State University

Washington State University

American Sociological Review 1973, Vol. 38 (August):476-478

Mayhew's baseline model predicting the relative size of the elite in a social system comprised of two stratified sub-classes is extended to the area of social power. Two basic measures of social power, a compliance ratio, and a coefficient of asymmetry (a measure of differentiation) are discussed. It is noted that the average compliance ratio for the elites is essentially a constant, while the average compliance ratio for non-elites is decreasing monotonic function of system size. The coefficient of asymmetry is an increasing monotonic function of system size. Social-psychological bases for system growth are suggested.

MAYHEW (1973) develops a formalized model which predicts the relative size of a ruling elite in a social system composed of two stratified subclasses. Two of the variables that Mayhew considers are (1) system size (S), and (2) size of the ruling elite, (which we shall designate s_1). In order to derive his results a ceteris paribus condition is imposed, which in this case implies a uniform distribution for all possible legitimate dichotomised subdivisions of the system, according to class membership rules specified by Mayhew.

Mayhew demonstrates that the ratio of s_1 to S is a monotonically decreasing function with limit $1/4$; i.e. the relative size of the ruling elite is a decreasing function of the total system.

We will show that Mayhew's model actually has a broader applicability, and may be used to develop further models concerned with aspects of the relative amount of control the elite (s_1) exercise over the others (which we designate s_2)—this can be thought of as a predictive model of power structures. Thus, we shall extend Mayhew's baseline model to the area of social power and indicate the effects of system size on two aspects of power structures.

Following Rapoport (1949a, 1949b, 1950) we consider group interaction to be composed of a set of dyadic encounters between participants in which one individual dominates. While other forms of interaction could be considered, this is the simplest and most basic form and the only one we shall analyse.

In undifferentiated situations we assume that each individual in an encounter has a

probability of $1/2$ of dominating that encounter. Rapoport and others use this assumption in developing some of the best known models of dominance. The situation discussed by Mayhew, however, involves a group (society) divided into two stratified groups, "elite" (s_1) and "non-elite" (s_2).

In stratified situations three basic types of encounters between individuals may occur: 1) a member of the elite encounters a member of the elite, 2) a member of the elite encounters a member of the non-elite, or 3) a member of the non-elite encounters a member of the non-elite. Clearly if the terms elite or non-elite mean anything in the area of power or dominance, we can make predictions about the results of such encounters.

The following simplifying assumptions are made: 1) encounters between members of the elite are equally likely to be dominated by either individual, i.e., the probability of either member of the dyad dominating is $1/2$; 2) encounters between members of the elite and members of the non-elite are always won by a member of the elite, i.e., the probability that a member of the elite will dominate a member of the non-elite is 1.00; and, 3) encounters between members of the non-elite are equally likely to be dominated by either individual, i.e., the probability that either member of the dyad will dominate is $1/2$. An individual dominates another when he receives compliance from the other.

In applying these assumptions to Mayhew's model we shall use two basic measures of power suggested by Gray et al. (1968), Richardson, et al. (1969), and Mayhew et al. (1960). The first of these, the *Compliance*

Ratio, C_{ij} , a measure of relative power, is defined as

$$C_{ij} = \frac{c_{ji}}{c_{ij} + c_{ji}}, \quad (1)$$

where c_{ji} indicates the number of encounters in which i dominates j , and where c_{ij} indicates the number of encounters in which j dominates i . From the preceding assumptions it is clear that $C_{ij} = 1$ whenever i is a member of the elite and j is not, $C_{ij} = 1/2$ when i and j are members of the same group (either elite or non-elite), and $C_{ij} = 0$, when j is a member of the elite and i is not. In the explication to follow we shall derive the expected value of the compliance ratios for members of either group under the assumptions stated.

A second measure of power structure suggested in the above references may be termed the *Coefficient of Dyadic Asymmetry for Compliance*, $K(C)$, and is defined by

$$K(C) = \frac{\sum_{i,j} |C_{ij} - .50|}{S(S-1)}, \quad (2)$$

where S indicates system size. $K(C)$ is a measure of the extent to which group members differ in gaining compliance from others; if $K(C)$ is 0, all members of a group receive identical levels of compliance, indicating a "flat" power structure; if $K(C)$ is 1, members of the group differ in compliance to the maximum degree possible, indicating a highly differentiated power structure.

Effects of System Growth on Power Structures

Mayhew (1973) derives the following expressions for the expected number of members of the elite,¹ $E(s_1)$, in groups of size S :

$$E(s_1) = \frac{S+3}{4}, \quad \text{for } S \text{ odd} \quad (3)$$

$$E(s_2) = \frac{S+2}{4}, \quad \text{for } S \text{ even} \quad (4)$$

By using Mayhew's results and the definitions and assumptions presented earlier, one can derive expressions of the expected values of compliance ratios and asymmetry coefficients for groups of any size. This may be done by noting the possible dyadic forma-

tions of a group of size S : thus there are $\frac{S(S-1)}{2}$ possible dyads which can be

formed, or which $\frac{s_1(s_1-1)}{2}$ are dyads in which a member of the elite encounters another member of the elite, $\frac{s_2(s_2-1)}{2}$

are dyads in which a member of the non-elite encounters a member of the non-elite (s_2 indicates the number of non-elite persons in the group), and of which $s_1(s_2)$ are dyads in which a member of the elite meets a member of the non-elite (where $S = s_1 + s_2$). Since we can specify, according to our assumptions, the compliance ratios for each of the possible types of encounters, we may use Mayhew's results to show that the mean compliance ratio for members of the elite, (\bar{C}_{s_1}), is given by

$$\bar{C}_{s_1} = \frac{1}{(S-1)} (S - .50 (s_1 + 1))$$

and the mean compliance ratio for members of the non-elite (\bar{C}_{s_2}) is given by

$$\bar{C}_{s_2} = \frac{1}{2(S-1)} (S - (s_1 + 1))$$

As can be seen from Figure 1 the values for the elite are essentially constant (with slight variation for even sized groups) and reach an asymptote at .875, which can be shown to be the limit of equation (5). It is important to note that while the mean compliance ratio for members of the elite is nearly constant regardless of group size, there is a clear tendency for the mean compliance ratio of non-elite members to increase with group size, though the limit is reached with group size of approximately 1,000. It is clear from Figure 1, and the equations, that limiting values of mean compliance for elites will be .875, and the limiting mean compliance for non-elite members will be .375 (which is the limit of equation 6).

Again using Mayhew's results we may derive expressions for the expected asymmetry of group power structures, $E(K(C))$. In this situation we must distinguish between odd and even size groups; the resulting equations are

$$E(K(C)) = \frac{S+4}{3S}, \quad \text{for } S \text{ odd} \quad (7)$$

¹ Our term $E(s_1)$ is identical to Mayhew's term A.

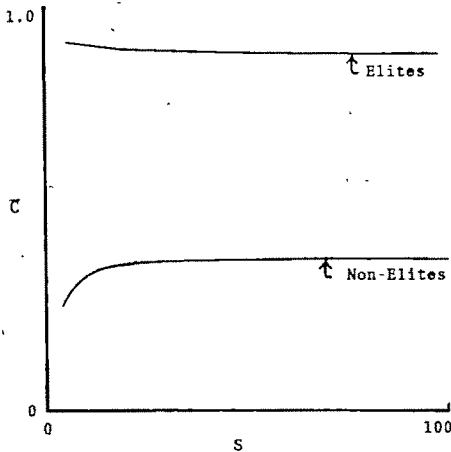


Figure 1

The relationship between mean compliance ratios (\bar{C}) and group size, averaged over odd and even sized groups

$$E(K(C)) = \frac{2S^2 + 5S - 6}{6S(S-1)}, \text{ for } S \text{ even (8)}$$

The limiting value of the two equations above is .333 (one-third) and the function decreases monotonically, as shown in Figure 2.

Implications

Two related important points emerge from this extension of the Mayhew findings: 1) the mean (expected) compliance ratio for members of the non-elite is a monotonically increasing function of group size (reaching limit .375 at approximately 1,000) and 2) the expected asymmetry value of a group power structure is a monotonically decreasing function of group size. If we assume that most persons attempt to increase their receipt of compliance or reduce the differences be-

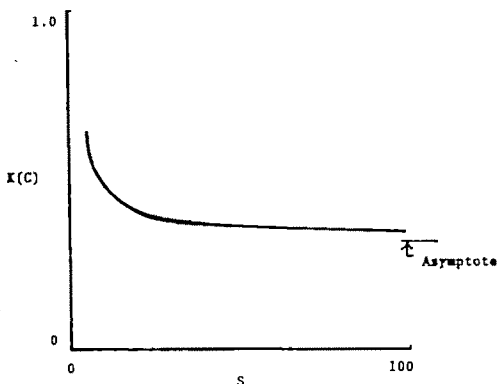


Figure 2

The relationship between the asymmetry of compliance and group size, averaged over odd and even sized groups.

tween individuals (i.e., reduce asymmetry), then it appears that growth is a positively reinforced behavior in groups with size under approximately 1,000.

Clearly, under the model, the elite can do little to increase their compliance ratios and still maintain the structure Mayhew suggests; however, the average compliance ratio for the non-elite increases (up to asymptotic value) with increases in system size. Similarly, if we are willing to assume that low asymmetry is reinforcing, i.e., persons prefer groups which are relatively equitable in the outcomes of potential encounters among members, then it follows that larger groups provide more reinforcement for members than smaller ones.

The implication here seems particularly important. While we usually attribute the growth of organizations, groups, and societies to their greater efficiency, people's ignorance, etc., the reason suggested here may be more important. If our notions about the reinforcing effects of compliance and lower asymmetry are correct, then we should expect organizations, groups, and societies to grow simply because the probability or extent of reinforcement for most members is greater in larger groups. Obviously this notion needs further examination.

REFERENCES

- Gray, L. N., J. T. Richardson and B. H. Mayhew
1968 "Influence attempts and effective power: a reexamination of an unsubstantiated hypothesis." *Sociometry* 31(September):245-58.
- Mayhew, Bruce H.
1973 "System size and ruling elites." *American Sociological Review* 38(August):468-75.
- Mayhew, B. H., L. N. Gray and J. T. Richardson
1969 "Behavioral measurement of operating power structures: characterizations of asymmetrical interaction." *Sociometry* 32(December):474-89.
- Rapoport, A.
1949a "Outline of a probabilistic approach to animal sociology: I." *Bulletin of Mathematical Biophysics* 11(September):183-96.
1949b "Outline of a probabilistic approach to animal sociology: II." *Bulletin of Mathematical Biophysics* 11(December):273-82.
1950 "Outline of a probabilistic approach to animal sociology: III." *Bulletin of Mathematical Biophysics* 12(March):7-17.
- Richardson, J. T., B. H. Mayhew and L. N. Gray
1969 "Differentiation, restraint and the asymmetry of power." *Human Relations* 22(June):263-74.

SYSTEM SIZE AND STRUCTURAL DIFFERENTIATION IN FORMAL ORGANIZATIONS: AN ALTERNATIVE BASELINE GENERATOR

DAVID A. SPECHT

Iowa State University

American Sociological Review 1973, Vol. 38 (August):479-480

The baseline model of Mayhew et al. (1972) which generates Blau's (1970) two major theoretical propositions relating size and structural differentiation in formal organizations is shown to be based on an assumption which limits the model's applicability to only hierarchical organizations. An alternative baseline model is developed which does not make this assumption and which fails to yield Blau's second proposition as a logical consequence.

BLAU'S (1970) propositions that structural differentiation is a monotonically increasing function of size and that the rate of increase is monotonically decreasing are derived by Mayhew et al. (1972) from a probability model of system size and system structure. The purpose of this paper is to develop an alternative baseline model not based on Mayhew's implicit assumption that organizations have hierarchical structures. The reformulated model does not imply Blau's second proposition.

Following Mayhew et al. (1972), size (S) is defined as the number of people in the organized labor force of the formal system and external structural differentiation (D) as the number of system parts to which employees may be formally assigned.

Mayhew's Model

Given a system of size S, differentiation can occur in exactly as many ways as there are integer sequences (which are generated ignoring order) that sum to S.

For a specified value of S, let s be a distinct sequence, i.e., structure, N be the number of such possible structures given S, p_s be the probability of s , and D_s be the value of D assigned to s .

Then assuming that each N structure is equally likely,

$$p_s = 1/N \quad (\text{Eq. 1})$$

so that

$$(\text{Eq. 2})$$

$$\Delta = E(D_s) = (1/N) \sum_{s=1}^N D_s$$

Equation 2 gives the baseline prediction, the

expected value for D if only random factors operate to produce structural differentiation. Mayhew et al. derive Blau's (1970) propositions that structural differentiation is a monotone increasing function of size and that the rate of increase is a monotone decreasing function of size from equation 2.

Reformulation of the Model

The critical assumption of Mayhew et al. (1972) is that the integer sequences should be generated *ignoring* order. An organization with two superiors and one subordinate is thus by assumption identical to one composed of one superior and two subordinates. If these two organizations are different in some sense, then the order of the integer sequences must be considered by a baseline model.

The modification is straightforward. Hall (1967) gives the number of possible structures considering order for fixed S and D_s as

$$(S-1)!$$

$$(D_s-1)!(S-D_s)!$$

Therefore the number of possible structures is

$$(\text{Eq. 4})$$

$$N = \sum_{D_s=1}^S \frac{(S-1)!}{(D_s-1)!(S-D_s)!}$$

and the expected value of D if all structures are equally likely is given by

$$(\text{Eq. 5})$$

$$\Delta = (1/N) \sum_{D_s=1}^S \frac{D_s(S-1)!}{(D_s-1)!(S-D_s)!}$$

It can be shown the Δ is a monotonically

increasing function of S but the rate of increase is not monotonically decreasing.

Letting $T = S - 1$ and $K = D_s - 1$, equation 3 becomes

$$\frac{T!}{K!(T-K)!} = \binom{T}{K} = \tau C_K \quad (\text{Eq. 3A})$$

and equation 4 becomes

$$N = \sum_{K=0}^T \frac{T!}{K!(T-K)!} = \sum_{K=0}^T \binom{T}{K} \quad (\text{Eq. 4A})$$

But the τC_K are the coefficients of the binomial expansion and therefore

$$N = 2^T = 2^{S-1}. \quad (\text{Eq. 4B})$$

Then the expected value of K is equivalent to the expected number of successes in T independent Bournoulli trials and hence

$$\Delta = E(D_s) = E(K+1) = T/2 + 1 = (S+1)/2 \quad (\text{Eq. 5A})$$

Since Δ is a linear function of S , differentiation is a monotonically increasing function of size. However, the rate of increase is constant not monotonically decreasing; and Blau's proposition is not a consequence of the model.

Equation 5a is identical to the results of Childers et al. (1971), but their model does not consider all structural forms. They assume that the distribution of D is uniform over the range $1 \leq D \leq S$. The model developed here implies that the distribution of D is binomial.

Discussion

The differences between Mayhew's model and the model proposed here are due to a change of one assumption. Mayhew et al. (1972) assumed that standardized sequences of integers should be used to generate structures, while the revised model assumed that the internal order of the sequences was a relevant component of the system. Consequently Blau's second proposition, an inference in Mayhew's model, becomes an empirical hypothesis in the revised model.

As Meyer (1971) notes, Blau implicitly assumes an hierarchical structure which may or may not be symmetrical (Blau, 1971). Since hierarchical structures are structures in which a subordinate reports directly to only one superordinate, they tend to have a decreasing number of positions at higher levels. Mayhew's assumption that only standardized sequences are admissible is a representation of the assumption that organizations must have such a structure. Thus, the choice of which baseline model to use depends on the definition of the relevant population of organizations.

On the other hand, both models specify that the two specific structures (138, 2) and (126, 9, 3, 2) are equally likely, though they differ on the relative likelihood of $D_s = 2$ and $D_s = 4$ for organizations of a given size. This suggests that neither these models nor the model of Childers et al. (1971) can serve as a representational model rather than as a baseline model since another reasonable assumption about organized social behavior is that span of control has a finite upper limit.

REFERENCES

- Blau, P. M.
 1970 "A formal theory of differentiation in organizations." *American Sociological Review* 35(April):201-18.
 1971 "Comments on two mathematical formulations of the theory of differentiation in organizations." *American Sociological Review* 36(April):304-7.
- Childers, G. W., B. H. Mayhew and L. N. Gray
 1971 "System size and structural differentiation in military organizations: testing a baseline model of the division of labor." *American Journal of Sociology* 76(March):813-30.
- Hall, M.
 1967 *Combinatorial Theory*. Waltham, Massachusetts: Blaisdel Publishing Company.
- Mayhew, B. H., J. M. McPherson, R. L. Levinger and J. F. James
 1972 "System size and structural differentiation in formal organizations: a baseline generator for two major theoretical propositions." *American Sociological Review* 37(October):629-33.
- Meyer, M. W.
 1971 "Some constraints in analyzing data on organizational structures: a comment on Blau's paper." *American Sociological Review* 36(April):294-7.

CAUSAL CHAIN MODELS FOR THE SOCIOECONOMIC CAREER *

JONATHAN KELLEY

Columbia University and The Center for Policy Research

American Sociological Review 1973, Vol. 38 (August):481-493

This paper develops a theoretical model for the socioeconomic career in which income and occupational attainment at successive stages in the life cycle are both modified causal chains. Income is affected only by education, current occupation, and income in the immediate past; occupation is affected only by family background, education, and occupation in the immediate past. Neither is affected by attainments in the more remote past. This dual causal chain model is an extension of Blau and Duncan's single causal chain model for occupational attainments over the life cycle.

Previous research suggested that both causal chain patterns were inadequate and so provided support for the alternative "historical" models proposed by Featherman. But when corrected for attenuation due to measurement error, the crucial data in fact provide strong support for the two causal chain models and evidence against the alternatives.

The Blau-Duncan Paradigm

OUR understanding of social stratification has advanced dramatically with the emergence of a standard paradigm (Blau and Duncan, 1967), advancing the field from its eclectic and only partly cumulative beginnings toward the more systematic and cumulative state that Kuhn (1962) has called normal science. In the basic theoretical model (Blau and Duncan, 1967:167-77), family background, measured by father's education and father's occupational status, has a causal influence on educational attainment. Both education and family background in turn influence occupational attainment. This basic model has recently been extended in three main directions.¹ 1) More extensive measures of family background have been incorporated, notably race, national origin, religion, urbanization, and number of siblings (Blau and Duncan, 1967;

Duncan and Duncan, 1968; Duncan, Featherman and Duncan, 1972: Ch. 4; Featherman, 1971b, 1971c). 2) In addition to education, other links between family background and subsequent occupational attainment have been explicitly included in the model. These include motivation, ability, appearance, migration, marriage and fertility (Bayer, 1969; Featherman, 1972; Blau and Duncan, 1967: Chs. 7-11; Duncan, Featherman and Duncan, 1972: Chs. 6-8; Elder, 1969; Sewell, Haller and Ohlendorf, 1970). 3) The basic model has also been extended to include additional outcome variables, notably occupation and income at several successive stages in the life cycle (Blau and Duncan, 1967:177-88; Duncan, Featherman and Duncan, 1972:37-45, 205-24; Featherman, 1971a). This last extension is the focus of the present paper.

The Occupational Career. Two different extensions of the basic paradigm to cover occupational attainment at several successive points in the life cycle have been suggested. 1) Blau and Duncan proposed an attractive modified causal chain in which occupation at any one time in the life cycle is causally affected by occupation at the time immediately preceding but not directly affected by occupations held in the more remote past. Occupational status at successive points in the life cycle is thus a causal chain—a Markov chain—but modified in that educa-

* I am grateful to Peter M. Blau, David L. Featherman, John L. Hammond, Seymour Spillerman, and Donald J. Trieman for helpful comments. The remaining errors are my own. This research was supported in part by grant GS-2378 from the National Science Foundation.

¹ The paradigm is beginning to be applied cross-culturally with results now available for Australia, Britain, Chile, Dickens' England, and Toro, a traditional Bantu society (Jones, 1971; Treiman and Terrell, 1972; Treiman, 1969; Tyree *et al.*, 1971; Kelley and Perlman, 1971; Kelley, forthcoming).

tion and family background continue to have direct effects at each successive stage. Blau and Duncan were unable to test this model directly (key correlations between occupational status at different stages in the life cycle were not available in their data) but were able to make indirect estimates by constructing synthetic cohorts. The original evaluation of this evidence suggested that the causal chain model was tentatively acceptable. But a subsequent evaluation by Duncan and his associates led them to definitely reject their synthetic cohort construction and to suggest that the causal chain pattern might simply be incorrect (Duncan, Featherman and Duncan, 1972:205-9). In the most definitive test, Featherman (1971a) later analyzed the PFS (Princeton Fertility Study) data set—which allows direct measurement of the heretofore missing correlations—and also tentatively rejected the model. 2) As an alternative to the causal chain model, Featherman then proposed a more elaborate “historical model” in which occupation at any one point in the life cycle is directly affected by occupations held in the remote past. In the causal chain model the influence of the remote past was only indirect, but in the historical model it has a direct influence as well. However, in this paper I will show that, appropriately analyzed, the PFS data actually fit the causal chain model and argue against Featherman’s historical model; data from the Six Cities Study of Labor Mobility also support the causal chain model.

The Socioeconomic Career. A further extension of the basic Blau-Duncan paradigm is to treat income in addition to occupation at several points in the life cycle. For this Featherman (1971a) proposed a second historical model in which income and occupation at any one point in the life cycle are directly affected by both income and occupation at each of several points in the past. As an alternative I would like to propose a dual causal chain model in which income and occupation at successive points in the life cycle are both modified causal chains. This model is compatible with Blau and Duncan’s causal chain model for the occupational career. A reanalysis of the PFS data provides good support for my dual causal chain model and

substantial evidence against Featherman’s alternative model.

Attenuation. This paper will also emphasize a disagreeable, although hardly unknown, fact. When applied in the usual way to correlations uncorrected for measurement error, path analysis (Duncan, 1966; Hauser and Goldberger, 1971)—the methodological arm of the Blau-Duncan paradigm—produces biased estimates of the size, and occasionally the sign, of causal forces even in the apparently simple case where all variables have the same amount of measurement error (Blalock, 1961:147-50; Bohrnstedt and Carter, 1971:130-40). The basic difficulty is that indirect effects are more heavily attenuated by measurement error than are direct effects, since they are attenuated by measurement error in the intervening variable (or variables) while the direct effects are not. This difficulty is directly relevant to evaluating causal chain models since estimates based on observed correlations can show—and in the PFS data do show—a significant direct effect where in fact there is only measurement error. This suggests that models uncorrected for measurement error should in general be viewed with considerable apprehension, if not outright suspicion.

THEORY

Models for the Occupational Career

As a model for occupational attainment at successive points in the life cycle, Blau and Duncan proposed a modified causal chain. The model is shown in Figure 1 for three time periods but can readily be extended. The heart of the model is a causal chain linking occupational status at different points in the life cycle. 1) Occupational status at any one point in time is directly affected by the occupation held in the immediate past. Although Blau and Duncan don’t go into the matter in detail this effect probably reflects at least two main processes. First, being in a job gives customary, and sometimes contractual tenure rights. So incumbents are likely to be able to continue in the same job or at least in similar jobs in the same organization. Second, experience in an occupation is valuable; skills and knowledge are acquired, contacts made, and the like. This experience gives job incum-

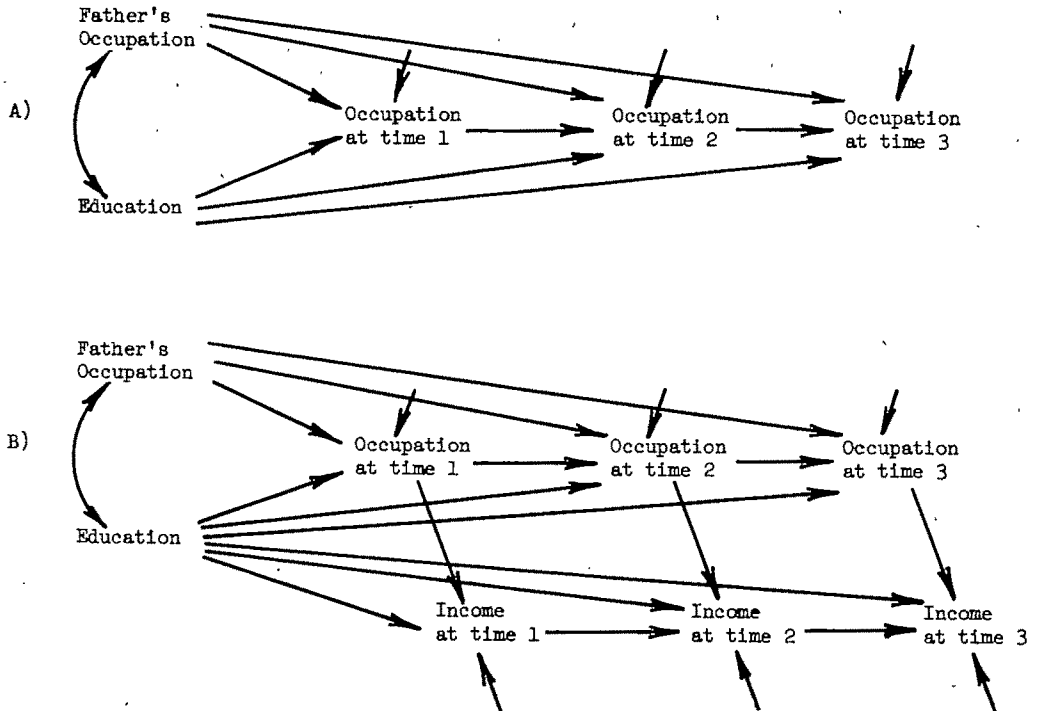


Figure 1. A) Blau and Duncan's causal chain model for the occupational career.
B) A dual causal chain model for the socioeconomic career.

bents a marked advantage over outsiders, encouraging them to stay in their present occupations (where they know the ropes) and making it harder for them to switch to other occupations (where they don't know the ropes). 2) The causal chain hypothesis posits that occupational status at any one point in time is only indirectly affected by occupations held in the more remote past. Thus if two men of similar background and education are in quite different occupations at time 1 but enter the same occupation at time 2, the claim is that they will compete on an equal footing at time 3. The fact that one man was in a better job at time 1 will no longer be an advantage; in this sense occupations have only a limited history. This seems theoretically reasonable. First, since the tenure rights acquired in a job typically assume more or less continuous employment, tenure acquired at time 1 would either carry over to time 2 (and so count as part of the effect of occupation at time 2) or be lost altogether. Similarly, experience in an occupation at time 1 should be irrelevant at time 3. Unless they are exercised in the interim and so are part of the effect of occupation

at time 2, specialized skills, knowledge, and contacts are in time lost or outdated. This might not be true in the short run (e.g. two or three years between time periods) but should be true over the time scale of interest here (e.g. eight to ten years between periods so that time 1 and time 3 are sixteen or more years apart). 3) The model is only a modified causal chain since educational attainment and family background directly influence the kind of job a man gets throughout his career.

This causal chain model is attractive in part because of its simplicity but also because it makes predictions which could be empirically falsified—a cardinal virtue (Popper, 1968; Ayer, 1952) not shared by Featherman's historical model or, indeed, by most models in sociology. It means that the model, and the theory underlying it, can be tested against reality.

As an alternative to the causal chain model, Featherman (1971a:296-9) proposes an "historical" model. This includes all the effects in the causal chain model and, in addition, posits that occupation at any point in the life cycle depends not only on occupa-

tion in the immediately preceding period but also directly on occupations held in the more remote past. Thus two men with the same occupation at age thirty will have differing prospects at age forty, depending on the status of jobs they had twenty or more years in the past. There seems to be no persuasive theoretical argument for these effects—their existence is argued on empirical grounds. But, once measurement error is taken into account, we will see later that there is no empirical justification either.

Models for the Socioeconomic Career

The basic Blau-Duncan paradigm for status attainment could be extended in many ways to cover both income and occupation at each stage in the life cycle. On theoretical grounds I would suggest the dual causal chain shown in Figure 1 as Model B. This model is essentially a pair of modified causal chains, one for occupation and the other for income; the two chains are linked together with occupation at each time period a direct cause of the income earned in that period. The chains are modified in that education has a direct causal effect on both occupational status and on income at each time period. Finally, father's occupation has a direct effect on son's occupation but not son's income at each time period.

Occupation. 1) Occupational status is determined exactly as it was in Blau and Duncan's causal chain model of the occupational career; occupation at one time period is directly affected by education, father's occupation, and own occupation in the immediately preceding time period. Occupations at two or more removes in time have no direct effect. 2) Occupational status is not influenced by past income, either directly or indirectly; the correlation between occupation and past income is entirely a consequence of common antecedents. Early in the life cycle income surely does have an indirect effect on occupational status since it can be invested in education and other forms of training which eventually pay off in occupational status (Schultz, 1963; Becker, 1965). But when the son has completed his training, there are no such opportunities for investment. This was not always true; until early modern times many occupations were routinely sold

(Mousnier, 1945; Göhring, 1938; Reader, 1966). But now income is almost entirely a consumption good, except for a few occupations which can still be discreetly purchased (e.g. ambassador). On the whole then, in modern societies one would not in theory expect income to influence occupational attainment in the middle and later periods of the life cycle.

Income. 1) The model asserts that income at any one time period depends on income in the period immediately preceding. This is only because income serves as a partial proxy for a number of variables not explicitly included in the model. First and probably most important, income measures characteristics of jobs. At any one level of occupational status, income varies appreciably from occupation to occupation (e.g. typists and bricklayers have similar prestige but quite different incomes) and also from job to job, even within a given occupation. Insofar as people stay in the same or similar jobs, past income will therefore predict current income. Second, income in part measures need, ability, motivation, and other individual characteristics which are relatively persistent over time. Third, past income is to some extent a minimum floor below which present income is not allowed to fall; many people are reluctant to move to a lower paying job. 2) Income at one period is not directly affected by income at two or more removes in time. None of the variables for which income is a proxy seem to have such effects. And at least through the early and middle stages of the life cycle, income from investments is probably negligible—only 6% of monetary income is received from investments, pensions and the like (U.S. Bureau of the Census, 1971:19). Income (or occupation) from the remote past have influence only very late in life, if at all. 3) Income at any one time period depends on occupational status at that time since high status occupations generally pay better than low status occupations. Past occupational status has only an indirect effect on current income (via its influence on past income and on current occupational status); having had a high status job in the remote past doesn't increase one's current salary. 4) Education affects income both because better educated men perform better (but see Collins, 1971:1005-6)

and because educational certification is a factor in hiring, promotions and raises. There is an indirect effect because education leads to higher status jobs which generally pay more. Education also has a direct effect at each time period. Even among occupations of the same status, educated men are more likely to be in the better paying occupations and in the better paying jobs in each occupation. 5) Father's occupation has no direct effect on income. High status fathers do not simply give their sons money, rather they help their sons get more education and higher status occupations and so, indirectly, more money.

An Alternative Model. Featherman (1971a:300-4) also proposed an historical model for the socioeconomic career. It includes all the connections in my causal chain model and, in addition, allows all past achievements to influence current occupation and income directly. In this model current income has a long history, directly affected not only by income in the immediate past but also by income in the distant past and by all past occupational statuses, however remote. Occupational status at any one time period is directly affected by past income as well as by all past occupational statuses.²

METHOD

Correcting for Attenuation Due to Measurement Error

In order to test these competing models empirically, measurement error must be explicitly taken into account. Estimates of paths and other regression parameters based on uncorrected correlations are biased (Bohrnstedt and Carter, 1971:130-40). And the biases are crucial here; correcting for measurement error leads to quite dif-

ferent substantive conclusions. The problem is that estimates of indirect effects are especially vulnerable to measurement error. All estimates are attenuated by measurement error in the independent and dependent variables; but estimates of indirect effects are, in addition, attenuated by measurement error in the intervening variables. The three variable case (Blalock, 1961:147-50), of interest in itself, shows the logic clearly. Suppose that A is a cause of B and B in turn a cause of C and that A actually has no *direct* effect on C. This, in the absence of measurement error, implies that the correlation r_{AO} is equal to the indirect effect $r_{AB} r_{BO}$ and that the path linking A to C will be zero (since its numerator, $(r_{AO} - r_{AB} r_{BO})$, is zero). But this is not true of correlations uncorrected for measurement error; on the usual assumptions the observed correlation, r_{ac} , is equal to the true correlation, r_{AO} , times a discounting factor: $r_{ac} = r_{AO} \sqrt{r_{aa} r_{cc}}$, where r_{aa} is the reliability with which A is measured.³ The observed indirect effect, $r_{ab} r_{bc}$, will be similarly discounted:

$$r_{ab} r_{bc} = (r_{AB} \sqrt{r_{aa} r_{bb}}) (r_{BO} \sqrt{r_{bb} r_{cc}}) = r_{AB} r_{BO} (r_{bb} \sqrt{r_{aa} r_{cc}}).$$

Both the direct effect and the indirect effect are discounted by $\sqrt{r_{aa} r_{cc}}$ while the indirect

³ This is based on the well known correction for attenuation, a result which can be conveniently derived from path analysis (e.g. Heise, 1969:94-5). First the path from a true score, A, to its fallible measure, a, can be derived from the reliability. By definition, the reliability is the correlation between two equivalent measures, and a', of the true score. We assume that a and a' are correlated only because they are both measures of A; i.e. $a \longleftrightarrow A \longleftrightarrow a'$. Since there is only one independent variable, $r_{aa} = P_{aa}$ and since a and a' are assumed to be equally good measures of A, $r_{a'a} = r_{aa} = P_{aa}$. By the usual path analysis procedures, $r_{aa'} = P_{aa} r_{a'a} = (P_{aa})^2$ and so $P_{aa} = \sqrt{r_{aa}}$, where following conventional notation we have dropped the prime from $r_{aa'}$. Now suppose we have fallible measures, a and c, and are willing to assume they are correlated only because the true scores, A and C, are correlated; i.e.

$$a \longleftrightarrow A \longleftrightarrow C \longleftrightarrow c.$$

Then the observed correlation between fallible indicators is $r_{ac} = P_{aa} r_{a'c} = P_{aa} r_{AO} P_{cc}$. But the two paths are known, so

$$r_{aa} = \sqrt{r_{aa} r_{AO} \sqrt{r_{AO} r_{cc}}} = r_{AO} \sqrt{r_{aa} r_{cc}};$$

this last is the formula used in the text. The correction for attenuation, i.e. the correlation between true scores, is then $r_{AO} = r_{ac} / \sqrt{r_{aa} r_{cc}}$.

² This historical model, like other fully recursive models, is basically a conceptual scheme; it lays out a set of variables and specifies a causal order but makes no firm predictions about the value of the paths. The causal chain model is, however, a theory in the strict sense of making assertions (about the absence of direct connections) which could be empirically falsified (Popper, 1968). By sociological standards it is actually a rather strong theory, making explicit predictions about eleven of twenty-eight paths.

effect is, in addition, discounted by r_{bb} . Consequently the observed (uncorrected) indirect effect will generally be smaller than the observed direct effect and the magnitude of the observed path linking A and C will be greater than zero—more so when there is more error in B. Thus the existence of a non-zero observed path, even a statistically significant one, in no way implies that there is a path between the true variables; the usual test of a causal chain (or the algebraically equivalent test for spuriousness) is wrong unless measurement error is explicitly taken into account. And in the general case with more than three variables, applying the usual procedures without correcting each correlation for measurement error is wrong for analogous reasons. Unbiased estimates can be obtained simply by correcting each correlation for attenuation and then proceeding as usual.

For the problem at hand, these corrections are straightforward. Siegel and Hodge (1968: 37) give the necessary reliability estimates (shown in Table 1) for respondent's occupational status, education, and income. No direct estimate is available for father's occupational status but Treiman and Hauser (1970) have developed a reasonable indirect estimate.⁴

DATA

The basic data are from Featherman's (1969, 1971a, 1971b, 1971c) reanalysis of the longitudinal Princeton Fertility Study (PFS). The original sample (Westoff et al., 1961, 1963; Bumpass and Westoff, 1970) was a stratified random sample of white couples whose second child had been born just before the initial interview in 1957 and

who lived in the New York, Philadelphia, Pittsburgh, Detroit, Chicago, Los Angeles or San Francisco Standard Metropolitan Areas; the men were, on the average, about thirty years old at the time of the initial interview. The present analysis is based on the 715 couples out of the original 1,165 who stayed through to the last wave of interviews. Approximately eight years separate each of the time periods analyzed here. Despite the limitations of the sample, an extensive analysis indicates that it produces quite reasonable estimates of the parameters of interest (Featherman, 1969; Duncan, Featherman and Duncan, 1972:170-6). One great advantage of these data is that they include information on occupations held over much of the life cycle; and, since the data are largely longitudinal, errors of recall are at a minimum.

The basic uncorrected correlations (Featherman, 1971a:297) are corrected for attenuation using the reliability estimates⁵ shown in Table 1. The data pertain only to men. Father's occupation and respondent's occupation at each time period are coded in intervals of the NORC prestige scale (NORC, 1947). Education is years of schooling at the time of the initial interview. Income at each time period is the annual income, in dollars, earned from salaries and wages during the previous year. Both corrected and uncorrected correlations among these variables are shown in Table 1 for three time periods.⁶ Time 1 is time of marriage; the average age then was a bit under twenty-two years. These data are retrospective. The time 2 data were collected when the second child was born, about eight years later. The time 3 data were collected, on the average, some eight years after that but (because

⁴ The derivation is complex; since our results are not at all sensitive to the exact value of this particular reliability, the details will be omitted. The basic idea is that correlation between father's occupation and father's education is lower than the correlation between son's occupation and son's education only because sons report their fathers' characteristics less reliably than they report their own—alternative explanations (secular change, differential fertility) can be discounted. This gives enough empirical leverage to allow quite believable estimates (viz. .718 for father's occupation and .778 for father's education). Details are available on request.

⁵ In another context Featherman (1972) corrects the PFS data for attenuation, using the same reliability estimates for son's characteristics. The minor discrepancies between his results and mine are due to different reliability estimates for father's occupation.

⁶ To simplify the presentation, Featherman's terminology has been modified. Time 1 is his time of marriage, time 2 is his Panel I and time 3 is his Panel III. His Panel II is omitted both because it was only three years after Panel I and in order to conform to his presentation of the Blau-Duncan model in Figure 1.

Table 1. Correlations Corrected for Attenuation^a (above the diagonal) and Uncorrected (below the diagonal). Reliabilities are given in the diagonal.

	Fs Occ	Ed	Occ1	I1	Occ2	I2	Occ3	I3
Fs Occ: Father's Occupation (.718)		.414	.369	.122	.420	.193	.434	.306
Ed: Own Education	.339	(.933)	.665	.162	.710	.365	.668	.536
Occ1: Occupation at time 1	.290	.596	(.861)	.258	.847	.429	.671	.493
I1: Income at time 1	.095	.144	.221	(.852)	.127	.403	.103	.229
Occ2: Occupation at time 2	.330	.636	.729	.109	(.861)	.395	.799	.549
I2: Income at time 2	.151	.325	.367	.343	.338	(.852)	.316	.474
Occ3: Occupation at time 3	.341	.599	.578	.088	.688	.271	(.861)	.600
I3: Income at time 3	.239	.478	.422	.195	.470	.404	.514	(.852)
Means	6.39	12.96	6.60	5096	7.01	6407	7.32	12822
Standard Deviations	2.29	2.69	2.22	2069	2.11	3829	2.10	6670

Source: Uncorrected correlations are from the PFS data set (Featherman, 1971a: 297). The reliability for Father's Occupation is from Treiman and Hauser (1970) and other reliabilities from Siegel and Hodge (1968).

these interviews were collected over a period of four years) there is again no constant interval between the two time periods. The three time periods cover the early and early middle stages of the adult life cycle—from about twenty-two to about thirty-eight years of age.

TESTING THE MODELS

The Occupational Career

When measurement error is ignored, Blau and Duncan's causal chain model appears empirically inadequate. The clearest test is Featherman's (1971a:296-300) analysis of the PFS data; the key issue is the correlation, .588, between occupational status at time 1 and occupational status at time 3. The causal chain model implies a correlation of .539, an underestimate of +.039. Featherman correctly notes that this small discrepancy might be due to correlated errors of measurement or to differences between the sample estimate and the true population parameter.⁷

⁷ The model can be made formally correct by

It might also be due to variables (e.g. motivation, intelligence) which affect occupational status at each time period but are not explicitly included in the model. But on balance Featherman concludes that the causal chain model is not an accurate representation of the occupational career—reaching by direct means the same conclusion Duncan, Featherman and Duncan (1972: 205-9) reached with an indirect test using the OCG data. Featherman then proposes his historical model as an alternative.

But the causal chain model should not in fact be rejected. 1) When corrections are made for attenuation due to measurement error, the PFS data both support the causal chain model and argue against the historical model. The parameters for both models, corrected for attenuation, are shown in Table 2. The data fit the causal chain model quite satisfactorily. The correlation between occu-

postulating a correlation of +.071 (not Featherman's +.089) between the residuals for occupation at time 1 and occupation at time 3. This would be significant at the .05 level if the estimate were unbiased.

Table 2. Standardized Partial Regression Coefficients for Blau and Duncan's Causal Chain Model (I) and the Historical Model (II) of the Occupational Career. Corrected for Attenuation.

Independent Variables	Dependent Variables:			
	Occ1	Occ2	Occ3	
	I, II	I, II	I	II
Fs Occ: Father's Occupation	.113	.078	.094	.093
Ed: Own Education	.618	.239	.182	.191
Occ1: Occupation at time 1		.659	0	-.066*
Occ2: Occupation at time 2			.631	.681
Variance explained (R^2)	.453	.760	.666	.667

*Coefficient less than twice its standard error.

pation at time 1 and occupation at time 3, .690, is just a little larger than the .671 predicted by the model—an error of only -.019. The already small error in prediction is cut in half; statistically it is not significantly different from zero, even at the .05 level.⁸ There is no reason to postulate an additional historical effect; that path would be very small (-.066, not significant even at the .05 level) and add only one tenth of one percent to the explained variance. So the causal model is preferable, if only on grounds of parsimony. 2) But even if the small historical effect is taken at face value, it is substantively implausible since the path is negative. It seems highly unlikely that being in a high status occupation at time 1 is a disadvantage at time 3. 3) Data from an unpublished analysis of the Six Cities Study of Labor Mobility, graciously supplied by Featherman, also support the causal chain model.⁹

⁸ Making the model formally correct implies a correlation of -.027 between the residuals for occupation at time 1 and occupation at time 3.

⁹ This major study, described elsewhere (e.g. Lane, 1968:742), was conducted in 1951. Featherman gives the following correlations for 2,054 non-Negro males aged 35-44 in the experienced civilian labor force:

	(1)	(2)	(3)	(4)	(5)
(1) Father's occupation475	.403	.392	.397
(2) Education	.389600	.606	.619
(3) Occupation in 1940	.317	.538884	.815
(4) Occupation in 1945	.308	.543	.761915

As in the PFS data, when measurement error is ignored there is an appreciable historical effect, .197, from occupation at time 1 to occupation at time 3. But when corrected for attenuation, the causal chain model predicts the correlation between occupation at time 1 and occupation at time 3 almost perfectly (.815 actual, .816 predicted). There is no evidence for an additional historical effect; the path, -.005, would be quite insignificant. On balance, then, it is reasonably clear that the causal chain model of the occupational career is preferable to the historical model.¹⁰

(5) Occupation

in 1949 .312 .555 .702 .788 ...
Occupations are socioeconomic status scores (Duncan, 1961). Correlations above the diagonal are corrected for attenuation using the reliabilities given in Table 1; those below the diagonal are uncorrected. The crucial estimates, corresponding to those given in the last column of Table 2, for the historical model are: father's occupation, .019; education, .095; occupation in 1940, -.005; occupation in 1945, .854. The figures for the causal chain model are virtually identical and the percent of variance explained by the two models is, to three decimal places, identical.

¹⁰ This is probably true in the OCG data set although the issue is of limited significance since these data, lacking key correlations between occupational status at different times, permit only very problematic estimates. 1) Correcting for attenuation reduces the already very small error in prediction from +.023 to -.017. The additional path postulated by the historical model is again negative—which is theoretically implausible—and adds only 0.06% to the variance explained. 2) There are however other reasons for doubting that a causal chain model

The Socioeconomic Career

We have seen that Blau and Duncan's causal chain model gives a satisfactory account of occupational attainment at several points in the life cycle and will now show that my dual causal chain model—which is one possible extension of the Blau-Duncan model—gives a satisfactory account of both income and occupation at successive stages of the life cycle. It fits the PFS data quite nicely, and the alternative historical model is unpersuasive. Estimates, corrected for attenuation, for both models are shown in Table 3.¹¹ 1) The connections posited by the causal chain model account for almost all the explained variance. The additional connections posited by the historical model jointly explain, on the average, less than four tenths of one percent additional variance. In four (of five) cases this increment in explained variance is not statistically significant even at the .05 level. With this possible exception, there is little support for any of the additional effects posited by the historical model.¹² So the causal chain model is prefer-

able on grounds of parsimony. 2) The eleven empirically falsifiable predictions made by the causal chain model correspond quite closely to the observed correlations; the differences between the observed and the predicted correlations are:¹³

$r_{\text{Focc}, 11} + .029$	$r_{\text{Occ1}, \text{Occ3}} - .019$
$r_{\text{Focc}, 12} - .010$	$r_{\text{Occ1}, 13} - .008$
$r_{\text{Focc}, 13} + .009$	$r_{11, \text{Occ2}} - .091$
$r_{\text{Occ1}, 12} + .034$	$r_{11, \text{Occ3}} - .018$
	$r_{11, 13} + .045$
	$r_{\text{Occ2}, 13} - .000$
	$r_{12, \text{Occ3}} - .017$

The differences seem quite small, although there is no rigorous standard for judging just what is small and what is large. The average magnitude of these errors is only .025; the largest by far involving the correlation between income at time 1 and occupation at time 2. The other errors are not only small

would fit the OCG data (Duncan, Featherman and Duncan 1972:205-9). If the causal chain model is true and if there have been appreciable historical changes—a debatable point for first job—the correlation between first job and current occupation should decline in older cohorts. In the OCG data it doesn't but there are reasons to think it would in genuine cohort data. In the longitudinal PFS data and in Jones' (1971:536) cross-sectional data for Australia, the correlations do decrease. And there are substantial difficulties with the OCG measure of first job (Duncan, Featherman and Duncan, 1972:210-24). Many respondents report jobs that they held before their education was complete. And particularly for those who eventually completed college—an increasing fraction in younger cohorts—first job is not highly correlated with subsequent occupation. Overall, although caution is indicated, the OCG data provide no good reason for rejecting the causal chain model.

¹¹ Data for time 1 are retrospective and presumably somewhat more subject to measurement error. This problem is not serious for occupation—which is easy to recall—but is more serious for income. Insofar as recall is simply inaccurate, observed correlations involving income at time 1 will underestimate the true correlations. But there is probably an offsetting bias since some respondents may estimate past income partly from current income ("I was getting so and so much less than I am now.") or from past occupation ("I was only a salesman then, so I would have been earning . . .").

¹² The main difficulty, involving occupation at

time 2, shows up either as a loss of a bit under one percent in the variance explained or as an error in prediction of moderate (-.091) size. The statistical difficulty could be removed by allowing a direct path from income at time 1 to occupation at time 2, to do so is substantively dubious. The resulting path is negative, which would indicate that having higher income at time 1 is a disadvantage resulting in lower occupational status at time 2. While this is possible (some men may forgo income by taking up jobs that have low salary but good prospects of advancement), on the whole it seems unlikely. It may be more plausible to suppose that the true path is zero, although there is clearly room for doubt. A second dubious point involves income at time 2. Although the historical model as a whole does not explain significantly more variance than the causal chain model, it posits one link, from occupation at time 1, which is significant. This might reflect an historical influence although there is no obvious substantive interpretation and no corresponding effect at time 3. Pending further information from other data sets, it may be more persuasive to suppose that the true path is zero, although there is again clear room for doubt. But the uncertainties involve only these two paths; there is no evidence whatsoever for any of the nine other historical effects posited by the model.

¹³ There are two ways of computing predicted values. The prediction for the correlation between income at time 1 and income at time 3, for example, depends in part on the value of the correlation between income at time 1 and occupation at time 3. And for that correlation either the empirically observed value, or the value predicted by the model could reasonably be used. I have used the observed value but in practice it doesn't really matter. Using the first procedure, the magnitude of the average error is .025 compared to .026 for the alternative.

Table 3. Standardized Partial Regression Coefficients for the Dual Causal Chain Model (I) and the Historical Model (II) of the Socioeconomic Career. Corrected for Attenuation.

Independent Variables	Dependent Variables:									
	Occ1	II		Occ2	I2		Occ3	I3		
	I, II	I	II	I	II	I	II	I	II	
Fs Occ: Father's Occupation	.113	0	.036*	.078	.081	0	-.012*	.094	.094	0 .008*
Ed: Own Education	.618	-.018*	-.029*	.239	.236	.120	.108	.182	.193	.164 .165
Occ1: Occupation at time 1		.270	.264	.659	.685	0	.134	0	-.060*	0 -.053*
I1: Income at time 1				0	-.097	.349	.331	0	-.006*	0 .061*
Occ2: Occupation at time 2						.265	.167	.631	.679	0 .042*
I2: Income at time 2								0	-.012*	.288 .268
Occ3: Occupation at time 3										.399 .397
Var. Explained (R ²)	.453	.067	.068	.760	.769	.289	.294	.666	.668	.464 .467
Difference in R ²		.001		.009		.005		.001		.003
Significance (F test)		NS		p<.01		NS		NS		NS

*Coefficient less than twice its standard error.

but also unsystematic, the model slightly overestimating some correlations and slightly underestimating others in a seemingly random way. With the one exception, the dual causal chain model predicts the observed data quite nicely. Since the historical model posits eleven additional paths without fitting the data appreciably better, the dual causal chain model is preferable on grounds of parsimony.

There are also more direct grounds for rejecting the historical model. The additional causal links it postulates are not only small but often implausible on substantive grounds—many are negative in contexts in which being negative makes little theoretical sense. 1) The effect of father's occupational status on income is positive at time 1, negative at

time 2 (which is implausible) and positive at time 3. The fluctuating pattern seems theoretically unlikely, and the effects are all very small. 2) Occupation at time 1 has a positive effect on income at time 2 but small negative effects on both occupation and income at time 3. It seems unlikely that having a high status occupation is a disadvantage at any later time. And if it were a disadvantage at time 3, then it should be one at time 2 as well. 3) The effects of income at time 1 also show an unlikely pattern—sometimes negative, sometimes positive and always small. 4) Finally income at time 2 has a small negative effect on occupation at time 3. This effect again seems unlikely. Overall, the historical model should probably be rejected since it implies a variety of substantively dubious

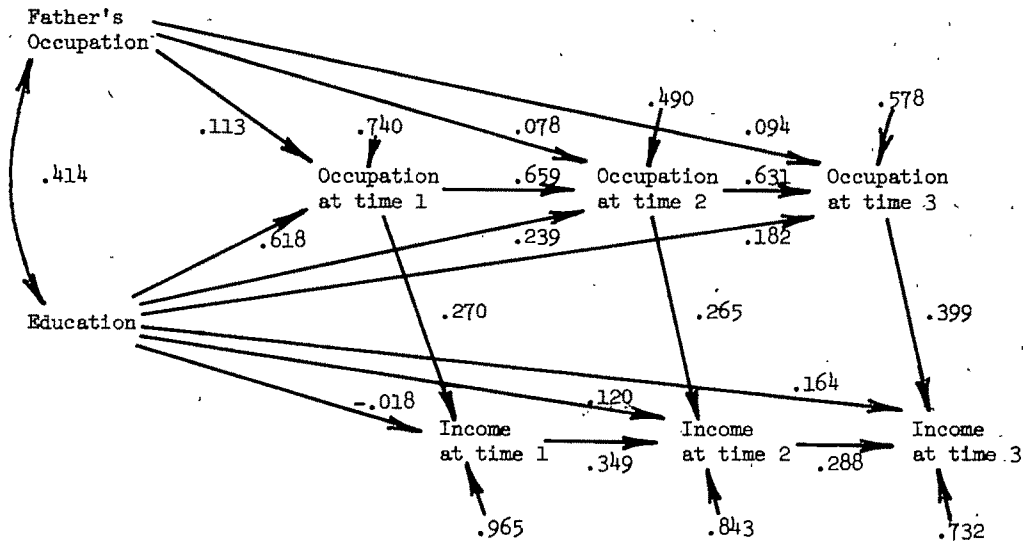


Figure 2. The dual causal chain model for the socioeconomic career. Parameters (from Table 3) corrected for attenuation.

effects of conflicting direction and uniformly small magnitude. The dual causal chain model assumes that the value of these effects is zero and that assumption seems more persuasive.

DISCUSSION

There is, then, good reason at least tentatively to accept both Blau and Duncan's causal chain model for the occupational career and the dual causal chain model I have proposed for the socioeconomic career. Both are consistent with the data and preferable to the main alternative. The theoretical implications of this result were described earlier, but a few comments on the relative importance of various effects are useful. Figure 2 gives the basic results in a convenient form—only one figure is shown since the two causal chain models lead to identical results for the occupational career.

The interpretation of occupational careers is generally similar to Blau and Duncan's (1967) and differs from Featherman's (1971a) primarily in the absence of historical effects. Occupational attainment at each stage in the life cycle depends mainly on occupational status in the period immediately preceding, to a lesser extent on education, and very slightly on family background. It is not directly affected by occupational status in

the remote past and neither directly nor indirectly affected by income; occupations have a very limited history.

Income at any one point in the life cycle is determined more or less equally by occupation at that time and income in the immediately preceding period; education, irrelevant at first, becomes moderately important later on.¹⁴ 1) Featherman's interpretation of the determinants of income (1971a: 302-4) seems generally appropriate except that there are no historical effects. Income has neither direct nor indirect effects on occupational status but does have a substantial direct effect on income in the period immediately following.

As the life cycle progresses, there is a clear tendency for income to be more rigidly determined; the monetary rewards of education and occupational attainment are largely deferred until the middle of the life cycle. 2) Family background has no direct effect on income. But the indirect effect, negligible early on, increases steadily; that happens because family background operates through education and occupation, and these have

¹⁴ The question of trends over the life cycle must be treated with some reserve for several reasons—the PFS data only extend to the middle of the career, there is a good deal of multicollinearity, and the interval between each time period varies somewhat from respondent to respondent.

strong effects only after a man's career is well under way.

Basically, we have shown that once a man's career is launched, his future depends mainly on his present and very little on his past; careers have no long history. 1) A man's current occupation is the key to his future status; nothing else matters much. His earlier career no longer matters, nor does his income, current or past—that is a consumption good, not a resource that can be invested in occupations. Once his career is under way, family background has only a marginal bearing on his future status. Education matters a little more, but not much, and progressively less as time goes on. 2) A man's future income depends almost equally on his future occupation and on his current income. His family background and his occupation and income at other times don't matter. Except for education—which becomes moderately important by the middle of the life cycle—the past is irrelevant. In short, as a man's career progresses, past failures are forgiven and past successes forgotten.

REFERENCES

- Ayer, A. J.
1952 *Language, Truth and Logic*. New York: Dover.
- Bayer, Alan F.
1969 "Marriage plans and educational aspirations." *American Journal of Sociology* 75 (September):239-44.
- Becker, Gary S.
1965 *Human Capital: A theoretical and Empirical Analysis, With Special Reference to Education*. New York: National Bureau of Economic Research.
- Blalock, Hubert M., Jr.
1961 *Causal Inferences in Nonexperimental Research*. Chapel Hill: The University of North Carolina Press.
- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: Wiley.
- Bohrnstedt, George W. and T. Michael Carter
1971 "Robustness in regression analysis." Pp. 118-46 in Herbert L. Costner (ed), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Bumpass, L. and C. W. Westoff
1970 *The later years of Childbearing*. Princeton: Princeton University Press.
- Collins, Randall
1971 "Functional and conflict theories of educational stratification." *American Journal of Sociology* 36(December):1002-19.
- Cummings, William K. and Atsushi Naoi
1972 "Education and mobility: an international comparison with special reference to Japan and the United States." Paper read to the Annual Meetings of the American Sociological Association, New Orleans. (August)
- Duncan, Beverly and Otis Dudley Duncan
1968 "Minorities and the process of stratification." *American Sociological Review* 33 (June):356-64.
- Duncan, Otis Dudley
1966 "Path analysis: sociological examples." *American Journal of Sociology* 72(July):1-16.
- 1961 "A socioeconomic index for all occupations." Pp. 109-38 in Albert J. Reiss and others, *Occupations and Social Status*. New York: Free Press.
- Duncan, Otis Dudley, David L. Featherman and Beverly Duncan
1968 *Socioeconomic Background and Achievement*. New York and London: Seminar Press.
- Elder, Glen H.
1969 "Appearance and education in marriage mobility." *American Sociological Review* 34(August):519-33.
- Featherman, David L.
1972 "Achievement orientations and socioeconomic career attainments." *American Sociological Review* 37(April):131-43.
- 1971a "A research note: a social structural model for the socioeconomic career." *American Journal of Sociology* 77(September):293-304.
- 1971b "Residential background and socioeconomic career achievements in metropolitan stratification systems." *Rural Sociology* 36(June):107-24.
- 1971c "The socioeconomic achievement of white religio-ethnic subgroups: social and psychological explanations." *American Sociological Review* 36(April):207-22.
- 1969 *The Socioeconomic Achievement of White Married Males in the United States: 1957-1967*. Unpublished Doctoral Dissertation, Ann Arbor: The University of Michigan.
- Göhring, Martin
1938 *Die Amterkäuferlichkeit im Ancien Régime*. Berlin: Verlag dr. Emil Ebering.
- Hauser, Robert M. and Arthur S. Goldberger
1971 "The treatment of unobserved variables in path analysis." Pp. 81-117 in Herbert L. Costner (ed), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Helss, David R.
1969 "Separating reliability and stability in test-retest correlation." *American Sociological Review* 34(February):93-101.
- Jones, F. Lancaster
1971 "Occupational achievement in Australia and the United States: a comparative path analysis." *American Journal of Sociology* 77(November):527-39.

- Kelley, Jonathan
Forth- *Social Mobility in Traditional Society: Re-coming sources, Modernization and Mobility.* (Revised version of Ph.D. dissertation, University of California, Berkeley, 1971) New York and London: Seminar Press.
- Kelley, Jonathan and Melvin L. Perlman
1971 "Social mobility in Toro: some preliminary results from Western Uganda." *Economic Development and Cultural Change* 19 (January):204-21.
- Kuhn, T. S.
1962 *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.
- Lane, Angela
1968 "Occupational mobility in six cities." *American Sociological Review* 33(October):740-9.
- Mousnier, Roland
1945 *La Vénalité des Offices sous Henri IV et Louis XIII.* Rouen: Editions Mangard.
- National Opinion Research Center
1947 "Jobs and occupations: a popular evaluation." *Opinion News* 9(September):3-13.
- Popper, Karl R.
1968 *The Logic of Scientific Discovery.* New York: Harper & Row.
- Reader, W. J.
1966 *Professional Men: The Rise of the Professional Classes in Nineteenth Century England.* New York: Basic Books.
- Schultz, Theodore W.
1963 *The Economic Value of Education.* New York: Columbia University Press.
- Sewell, William H., Archibald O. Haller and George W. Ohlendorf
1970 "The educational and early occupational status attainment process: replication and revision." *American Sociological Review* 35(December):1014-27.
- Siegel, Paul M. and Robert W. Hodge
1968 "A causal approach to measurement error." Pp. 28-59 in Hubert M. Blalock, Jr., and Ann B. Blalock (eds.), *Methodology in Social Research.* New York: McGraw-Hill.
- Treiman, Donald J.
1970 "Industrialization and social stratification." Pp. 207-34 in Edward O. Laumann (ed.), *Social Stratification: Research and Theory for the 1970's.* Indianapolis: Bobbs-Merrill.
- 1969 "Societal development and social mobility: a cross-national comparison of systems of social stratification." Unpublished research proposal, Department of Sociology, University of Wisconsin, Madison.
- Treiman, Donald J. and Robert M. Hauser
1970 "On the intergenerational transmission of income: an exercise in theory construction." Unpublished manuscript. New York: Columbia University.
- Treiman, Donald J. and Kermit Terrell
1972 "The role of education in status attainment: a comparison of the United States and Great Britain." Paper read to the Annual Meeting of the American Sociological Association, New Orleans, (August).
- Tyree, Andrea, et al.
1971 "The Dickensian occupational structure." Unpublished manuscript. Department of Sociology, University of California at Los Angeles.
- U.S. Bureau of the Census
1971 *Current Population Reports, Series P-60, No. 80, "Income in 1970 of families and persons in the United States."* Washington, D.C.: U.S. Government Printing Office.
- Westoff, C. F., R. G. Potter and P. C. Sagi
1963 *The Third Child.* Princeton: Van Nostrand.
- Westoff, C. F., R. G. Potter, P. C. Sagi and E. L. Mishler
1961 *Family Growth in Metropolitan America.* Princeton: Van Nostrand.

COMMENTS

"ORDINAL REGRESSION?" A COMMENT *

Three prestigious outlets have recently given their imprimatur to articles recommending with praise a nonsense to which one has given the name "ordinal regression"; the intent of this note is to trip up that imagined animal before it becomes rampant.

The trouble begins with Boyle's (1970, 1972) suggested strategy for "decomposition of an ordinal variable into dummy variables." He writes: "In order to retain the *ordinal* property of the categories, dummy variables are constructed as shown in Table 1" (1970:465, 1972: 437). Boyle's Table 1 is reproduced here:

Table 1

	Translation into Dummy Variables:			
	Decomposition I		Decomposition II	
Parent variable X	D_1	D_2	D'_1	D'_2
2	1	1	1	0
1	0	1	0	1
0	0	0	0	0

NOTE: This comes from Boyle (1970, p. 466; 1972, p. 437). Decomposition II is the orthodox decomposition; Decomposition I is the one Boyle recommends "to retain the ordinal property of the categories."

Using Decomposition I, Boyle claims that "all of the original information is retained," and further emphasises this point by a contrasting footnote: "Dummy variables can *also* be constructed so that *only nominal* scale information about the categories is retained." (1970:465n, 1972:437n, emphasis mine).

The Boyle article has been extensively criticized by Lyons and Carter (1971), but they do not seem distressed by Boyle's discussion of his Decomposition I as "*maintaining* assumed ordinal distinctions" (Lyons and Carter, 1971: 1126, emphasis mine). Blalock moreover, without criticism, reprints the original paper, and views it thus: "Boyle's paper suggests an alternative to the use of ordinal measures of association, namely the extension of dummy-vari-

able analysis in such a way that the *ordering among categories is preserved*" (Blalock 1972: 411-12, emphasis mine).

Finally Lyons (1971) talks at length about "ordinal regression" and appears to regard Boyle's Decomposition I as an instantiation of "the possibility of incorporating assumed-ordinal variables into the regression analysis framework while *retaining ordinality* of the variable" (Lyons 1971:168, emphasis mine).

I submit that these statements by Boyle, Blalock and Lyons are dangerously misleading. As a little thought will reveal, the recommended decomposition does not preserve ordinality; it merely provides coefficients which, were the

variable ordinal, would be more immediately interpretable than those provided by the standard decomposition. Lyons in particular should have been aware of this since he spends some time demonstrating the isomorphism of the decompositions. Indeed I am puzzled as to what Lyons might mean by "retaining ordinality" since his example (Lyons 1971:153) reveals non-ordinal effects, and our knowledge of ordinality enters only into the interpretation. The Boyle Decomposition I would only retain the ordinality information were we able to impose, upon our regression procedure, the constraint that the coefficients for D_1 and D_2 be of equal sign (or, if we are satisfied with simple monotonicity, be not of opposing sign). This constraint is not imposed, so there are no grounds for claiming retention of ordinality; Decomposition I and Decomposition II are, in this context, identical in the information they record.

Whether a variable is ordinal is our decision,

* I am grateful to P. Doreian and O. D. Duncan for critical response to this note.

and not a product of its behaviour in an estimation equation. But having made that decision we surely can only be said to be analysing that variable as ordinal variable if our analysis is constrained to take account of that ordinality (in the present instance, return ordinal effects). We, for example, call Goodman and Kruskal's gamma, but not their tau, a measure of ordinal association. By contrast, whether we opt for Decomposition I or Decomposition II, or whether we re-order the categories makes no difference to the estimated effect for each category (it merely expresses it differently) or to the predicted values of the dependent variable.

In short, all that these writers are telling us specifically about ordinal independent variables, is that we can incorporate them into our regression equations by treating them as merely nominal variables and observing the structure of their unconstrained effects. One feels this could have been said with less fuss.

K. I. MACDONALD

*University of Essex
Colchester, England*

REFERENCES

- Blalock, H. M.
1972 "Other Complications." Pp. 411-13 in H. M. Blalock (ed.), *Causal Models in the Social Sciences*. London: Macmillan.
- Boyle, R. P.
1970 "Path analysis and ordinal data." *American Journal of Sociology* 75 (January):461-80.
1972 "Path analysis and ordinal data." (Reprinted.) Pp. 432-52 in H. M. Blalock (ed.), *Causal Models in the Social Sciences*. London: Macmillan.
- Lyons, M.
1971 "Techniques for using ordinal measures in regression and path analysis." Pp. 147-71 in H. L. Costner (ed.), *Sociological Methodology 1971*. San Francisco: Jossey-Bass.
- Lyons, M. and Carter, T. M.
1971 "Further comments on Boyle's 'Path analysis and ordinal data.'" *American Journal of Sociology* 76(May):1112-32.

"HARDSHIP AND COLLECTIVE VIOLENCE IN FRANCE": A COMMENT*

Snyder and Tilly (1972) seek to discredit the "expectation-achievement gap" theory of collec-

* I wish to thank Neil Henry and Marshall Meyer for their helpful comments on an earlier draft of this paper. Neil Henry deserves special credit, and he has graciously accepted full responsibility for whatever is wise and accurate in this paper. He insists, however, that I alone am responsible for the errors which remain.

tive violence and to admit in its place an explanation which assigns causal primacy to a set of factors associated with struggles for political power. Support for the expectation-achievement hypothesis among sociologists is owing to the dearth of persuasive alternatives rather than to its continuing success in "critical experiments." In this context Snyder and Tilly display a healthy skepticism, and the political process model they propose is a welcome departure, in direction if not substance, from previous work. Nevertheless, serious methodological errors involving the choice of variable indicators and the execution of statistical tests impair their treatment of both models, and warrant a hasty retreat from their conclusions. This note will critically assess the methods they use to test each theory.

THE EXPECTATION-ACHIEVEMENT GAP MODEL

Proponents of the expectation-achievement gap hypothesis argue that the level of collective violence is determined by the disparity between expected and achieved welfare.¹ Collective violence is hypothesized to decrease or increase, respectively, depending on whether actual welfare outcomes exceed or fall short of welfare expectations.² The theory takes the general form,

$$(1) \quad Z_t = f((X_t - X^*_t)),$$

$$\frac{dZ}{d(X - X^*)} < 0,$$

where t is time, Z_t collective violence, X_t achieved welfare, and X^*_t unobserved welfare expectations formed at time $t-1$ regarding the value of X_t .

Aside from the problem of specifying the form of f , equation (1) quite clearly indicates that a test of the theory requires two things. First, it requires a time series of observations on Z and X . This involves the obvious but essential task of selecting or constructing indicators that are valid and reliable surrogate measures of the

¹ The neutral, more flexible term "welfare" will be employed where Snyder and Tilly use "achievements" and "hardship."

² Snyder and Tilly are somewhat ambiguous on this point. The source of the ambiguity is their failure to distinguish clearly between two very different hypotheses. The article as a whole suggests they are testing the hypothesis that collective violence is a response to the gap between expectations and achievements. Yet the title of the article and scattered sentences throughout seem to imply they are testing the hypothesis that collective violence is a response to hardship per se. We assume that the former hypothesis accurately reflects their intention, and the latter is merely "loose talk."

theoretical variables collective violence and welfare. Second, it requires that explicit account be taken of the difference between expected and achieved welfare. This means devising some method by which the gap between observed welfare values and unobserved welfare expectations can be represented in terms of the observable variables.

Snyder and Tilly do not adequately treat either of these problems. Not only are the indicators of welfare they employ of uncertain validity, but their confused and ultimately futile effort to somehow measure relative deprivation by taking first differences leads them to estimate an equation which does not accurately describe the expectation-achievement gap theory.

Measures of Welfare. Two of the measures Snyder and Tilly use to capture the welfare of a population are price indices. They contend that an increasing price index is evidence of decreasing welfare, presumably on the grounds that prices enter the consumers' utility function as a cost; hence positive increments in price signal a reduction in welfare. This line of reasoning is clearly erroneous, for it overstates the part that price, or cost, plays in determining welfare. Welfare emerges from the interplay of both costs and benefits, but a price index taps only the cost side of the welfare equation.

By definition welfare is the difference between costs and benefits; and therefore it is trivially true that for any given level of benefits, welfare and costs are negatively related. But Snyder and Tilly seem to mistakenly regard this purely definitional relation as a statement about the empirical association between welfare and prices. In fact, the definition places no restrictions whatsoever on the empirical relation between price and welfare; and therefore the zero-order correlation can assume any value in the range from -1 to $+1$. In the absence of income data, price data alone permit no inferences with regard to the empirical level of welfare. To claim that the time path of a price index inversely reflects the movement of welfare is to assume that income remains constant.

Contrary to Snyder's and Tilly's assumption, an increasing price index is perfectly consistent with increasing welfare. Consider an average family whose income is I dollars at time t and $I + .10I$ dollars at time $t + 1$. If the price index of the cost of living rose, but by less than 10%, between these years, then the family would still enjoy a higher standard of living at time $t + 1$. But this only illustrates what is by now obvious. After all, who would claim that the level of welfare of the population of the United States has been steadily declining over the last four decades on the grounds that the national price index has

been generally rising since 1933 (Samuelson, 1967:174)?

Snyder and Tilly do not rely exclusively on the price index to indicate welfare, but supplement it with an index of industrial production. Unfortunately, the two types of indices together only raise additional doubts regarding validity. For if, as Snyder and Tilly claim, indices of price and production are, respectively, inverse and direct measures of welfare, we would expect them to be negatively correlated; whereas in fact the correlation is approximately zero ($-.04$). Whatever it is these indexes measure, the data do not support the claim that they are indicators of the same underlying variable.

Problems of Model Construction. An unfortunate choice of indicators is not, however, the weakest link in Snyder's and Tilly's analysis. A more serious problem, quite independent of the question of validity, is their failure to translate accurately the expectation-achievement gap hypothesis into an estimable regression equation. Two related points are at issue. First, the equation they estimate is neither the expectation-achievement hypothesis, nor the model described in their verbal remarks. By confusing the use of first differences as a statistical technique used to reduce serial correlation with its use in introducing the notion of relative deprivation, they arrive at an equation which is at odds with their verbal discussion. Second, the test model described in the text, but never analyzed, assumes that the first difference of a welfare variable is a suitable measure of the expectation-achievement gap. In fact, the expectation-achievement disparity cannot be unambiguously identified with observed first differences in a welfare variable without a model of the mechanism generating expectations; and the mechanism implied by the use of first differences is theoretically untenable.

Tilly and Snyder adopt the hypothesis that collective violence ensues "when a population compares current experience with that of the immediate past, and therefore suffers 'relative deprivation' when [welfare decreases]." Further, they assume that the method of first differences, in addition to reducing serial correlation, "[measures] relative deprivation as change from one year to the next."

Holding prior objections in abeyance, assume that X_t is a perfect measure of welfare and $\Delta X_t = X_t - X_{t-1}$ is used as a measure of relative deprivation. Snyder and Tilly estimate an equation of the general form

$$(1.1) \quad \Delta Z_t = b_0 + b_1 \Delta X_t + \Delta u_t,$$

where Δ is the first difference operator, Z_t collective violence, and u_t the disturbance. Were the first difference operator applied *only* to re-

COMMENTS

duce the serial correlation, the original theoretical specification would have had to be

$$(1.2) \quad Z_t = a + b_0 t + b_1 X_t + u_t,$$

where a is the intercept and t is a linear time trend. That this is the case can be seen by simply taking first differences of (1.2): $Z_t - Z_{t-1} = b_0 + b_1 (X_t - X_{t-1}) + (u_t - u_{t-1})$, and we have (1.1). The intercept term b_0 in (1.1) is equivalent to adding a linear time trend to the necessary theoretical specification, as displayed in (1.2).

Equation (1.2), the hypothesis Snyder and Tilly actually tested, states that the level of collective violence depends on the level of welfare and time, a specification which neither represents the expectation-achievement proposition nor is consistent with the stated intention of using ΔX_t as a measure of relative deprivation. Moreover, there are no theoretical grounds for entering a time trend in equation (1.2).

Had Tilly and Snyder followed their suggestion of using ΔX_t to indicate relative deprivation, they would have specified the structural equation,

$$(1.3) \quad Z_t = a + b_1 \Delta X_t + u_t.$$

This model states, quite straightforwardly and simply, that the level of collective violence depends on the degree of relative deprivation. But Snyder and Tilly never did state equation (1.3), though their preliminary remarks clearly imply it, because they mistakenly assume that a single application of the first difference operator can do double duty by both reducing the serial correlation and producing the desired ΔX_t . The correct procedure would have been to specify the theoretical model, (1.3), and only then attend to the statistical matter of taking first differences to manage the serial correlation. Proceeding in this manner, the final estimation equation would have been

$$(1.4) \quad \Delta Z_t = b_1 (\Delta X_t - \Delta X_{t-1}) + \Delta u_t$$

instead of (1.1). Had Snyder and Tilly specified (1.3) and estimated (1.4), they would have more closely approximated a test of the expectation-achievement hypothesis.

Nevertheless, model (1.3) is far from satisfactory—implausible in its implications—resting as it does on the simplifying assumption that ΔX_t measures relative deprivation. This can be shown by comparing (1.3) with the general form of the expectation-achievement hypothesis, $Z_t = f((X_t - X^*))$. If f is linear the stochastic model is

$$(1.5) \quad Z_t = a + b_1 (X_t - X^*) + u_t.$$

A comparison of (1.3) and (1.5) reveals that the two models are identical if the mechanism generating expectations is

$$(1.6) \quad X^*_t = X_{t-1};$$

i.e., the expected level of welfare is equal to the

actual level of welfare achieved at the time $t-1$ that expectations are formed. In other words, only present experience informs the calculus of welfare expectations. Therefore, if between time $t-1$ and t actual welfare decreases, $X_t < X_{t-1}$, then by (1.6) expectations X^*_t will exceed X_t , producing relative deprivation. As Snyder and Tilly suggest, ΔX_t does indeed measure relative deprivation, but only on the assumption that expectations are generated by (1.6).

Though (1.6) formally permits a test of the theory in the form of model (1.3), it is an assumption few sociologists should endorse. Whatever it provides in the way of simplicity it sacrifices in theoretical substance; expectations are not independent of past experience. Given that (1.6) is implausible, then even if Snyder and Tilly had made a smooth transition from verbal formulation to regression equation, model (1.3) would not have provided an acceptable test of the expectation-achievement hypothesis.

A Distributed Lag Model. Snyder's and Tilly's failure to model the expectation-achievement gap theory of collective violence does not imply that they tried to accomplish an impossible task. Were there no acceptable procedure for representing unobserved "expectations" in estimable regression equations, many important theories would remain untested. But the situation is not so bleak, for "expectations" play an important role in many branches of economic theory; and econometricians have developed methods of testing "expectations" hypotheses. The formulation and estimation of models of this sort is contained in the literature on "distributed lags." Since excellent surveys of this literature are available (Griliches, 1967; Wallis, 1969), and, indeed, an entire text is devoted to the topic (Dhrymes, 1971), I shall merely display a model which in some respects accurately represents the expectation-achievement hypothesis.

The model proposed is a variant of the adaptive expectations model well-known to econometricians. The theoretical specification is, as shown in (1.5),

$$(2.1) \quad Z_t = a + b_1 (X_t - X^*_t) + u_t,$$

where all variables are as previously defined and the stochastic structure of the disturbance u_t remains, for the moment, unspecified. Equation (2.1) states that the level of collective violence is a linear function of the deviation of achieved from expected welfare. As it stands equation (2.1) is unestimable; it requires an assumption about the mechanism generating expectations X^*_t . Assume X^*_t is generated by

$$(2.2) \quad X^*_{t+1} - X^*_t = (1-g)(X_t - X^*_t), \\ 0 < g < 1$$

which means that expectations are revised lin-

early per period of time in proportion to the difference between achieved welfare and the value previously expected. Rearranging (2.2) gives,

$$X^*_{t+1} = (1-g)X_t + gX^*_t,$$

which on repeated substitution yields,

$$(2.3) X^*_{t+1} = (1-g) \sum_{i=0}^{\infty} g^i X_{t-i+1}.$$

Thus the current expected level of welfare is a weighted sum of past achieved levels of welfare, where the weights (lag coefficients) are $w_i = (1-g)g^i$.

Some interesting implications derive from the condition $0 < g < 1$. In the first place it means that the lag coefficients w_i are non-negative and sum to unity, $\sum_{i=0}^{\infty} w_i = 1$; therefore (2.3) represents an average. More important, the restriction of g to the interval $(0, 1)$ implies that the w_i form a geometrically decreasing sequence, thereby incorporating the assumption that the maximal impact of achieved welfare on welfare expectations is registered instantaneously, and thereafter declines geometrically to zero. A value of g close to zero implies that the lag coefficients w_i decline rapidly as i increases, so that expectations depend strongly on recent experience; a value of g close to unity means the w_i decline more gradually, so that the values of X from the more distant past enter expectations (Wallis, 1969).

Since (2.3) gives welfare expectations in terms of achieved welfare levels, substituting for expectations in the theoretical model will yield an equation in terms of observable variables only. Lagging (2.3) once, and substituting for X^*_t in (2.1) gives

$$(2.4) Z_t = a + b_1(X_t - (1-g) \sum_{i=0}^{\infty} g^i X_{t-i+1}) + u_t.$$

Equation (2.4) is a variant of the so-called geometric lag distribution model. A procedure for reducing this model was introduced by Koyck (1954). Lag (2.4) once, multiply through by g , and subtract the resulting equation from (2.4), to yield

$$(2.5) Z_t = a(1-g) + b_1(X_t - X_{t-1}) + gZ_{t-1} + v_t,$$

where $v_t = u_t - gu_{t-1}$.

Equation (2.5) is the final estimation equation of the expectation-achievement gap model proposed in (2.1) and (2.2).⁸ Its relation to the

⁸ Neil Henry pointed out that equation (2.5) can be derived more simply by combining (2.1) and (2.2) and applying the Koyck transformation. But this would mean sacrificing the intuitive appeal of viewing expectations as a weighted average of past experiences and the substantive content of assuming a geometric lag distribution.

models considered previously is worth mentioning. Observe that if we let $g=0$, then (2.2) reduces to (1.6) and (2.5) reduces to (1.3), and we have the model derived from Snyder's and Tilly's suggestion that ΔX_t be used as a measure of relative deprivation. If, on the other hand, we let $g=1$, then (2.5) reduces, with the exception of the intercept, to (1.1), and we have the equation Snyder and Tilly actually estimated. In connection with the latter model, substituting $g=1$ into (2.2) reveals that (1.1) assumes that expectations remain constant through time.

The presence of the lagged endogenous variable Z_{t-1} on the right-hand side of (2.5) creates some problems in estimating the parameters of the model. In particular, the method of estimation, and thus the properties of the estimators, will depend on the stochastic structure of the disturbances u_t . For example, under the classical specification, where the u 's are assumed to be independently distributed random variables, having mean zero, constant variance, and independent of X_t for all t , applying ordinary least squares methods will yield inconsistent estimators. The complication arises because this condition on the u 's implies that the v 's are serially correlated, and thus correlated with the lagged endogenous variable Z_{t-1} . For an extensive treatment of this and other problems associated with estimating lag models consult Dhrymes (1971).

Though equation (2.5) may pose serious estimation problems, the model advanced here is probably the simplest of a class of models that could realistically represent the expectation-achievement hypothesis in testable form. Equation (2.1) could be modified to include additional welfare variables, either additively or multiplicatively; equation (2.2) could be stochastic. In any case, modifications in the direction of greater complexity are likely to entail more difficult problems of estimation, and could quite possibly introduce nonlinearity in the parameters. Such problems may be unavoidable if the expectation-achievement gap theory is to be rigorously tested.

THE POLITICAL REPRESSION MODEL

Tilly and Snyder failed to test, let alone reject, the expectation-achievement gap hypothesis; have they succeeded in confirming their political repression model? They propose that collective violence be viewed as a negative function of political repression, arguing that governmental repression, by increasing the cost of collective action, indirectly reduces the incidence and magnitude of collective violence. They test

Table 1. Political Repression Model Estimates

Model I:

$$P = 4838.6 + 6.60Q - .002R - .027S + 30.97T$$

(**) (*)

Model II:

$$P = 6104.3 - .003R - .050S + 33.3T$$

(*) (*)

Model III:

$$P = 88.3 + 4.13Q - .215R + 45.15T$$

(***) (*)

* Significant

** Wrong sign

*** Significant and wrong sign

P--collective violence

Q--"excess arrests"

R--annual governmental expenditures

S--man-days in jail

T--national election

(dummy: 1 = yes, 0 = no)

this hypothesis by estimating three separate regression equations, each of which takes collective violence as the dependent variable and various indicators of repression and national political activity as the regressors. The estimated equations are displayed in Table 1. Apart from considerations of substantive validity, there exist strong statistical grounds for questioning their claim that the data support the hypothesis.

The most general shortcoming of their model is that it includes only one equation of what is in fact a simultaneous system in which repression and political violence exert reciprocal determining effects. Though many models appearing in the literature are open to this criticism, in this instance it is especially plausible. Snyder and Tilly explicitly entertain the possibility of feedback, and reciprocal causation is an implicit component of one of their indicators of repression (Q, "excess arrests"). Yet they fail to make a preliminary test for direction of causality, and apply ordinary least squares to estimate the parameters.⁴ If repression and collective violence are reciprocally determining in a

simultaneous fashion, the condition that the regressors be strictly exogenous, central to the statistical theory of time series regressions, is violated. In the absence of the one-way causality condition, the estimators will not possess desirable properties, e.g., consistency.

Turning to a more specific aspect of their analysis, consider the indicators of repression: "Excess arrests" (Q), "man-days in jail" (S), and annual governmental expenditures (R). Snyder and Tilly acknowledge that these indicators are weak measures of repression, and in this judgment they are correct. In particular, the use of annual governmental expenditures, R, as an indicator of political repression is forced. Though it is certainly true that the wealthier a government the greater is its "capacity" for violence, since it can afford large quantities of the most modern military hardware, and can maintain and equip a large police force, to assume that "capacity" is automatically translated into actual repression is unjustified. That the validity of R rests on such a dubious assumption is especially damaging in view of the fact that the only consistent finding that superficially confirms the theory is that the coefficient of R is statistically significant in the predicted direction in each of the three equations estimated.

From a different view, only model II approximately confirms the political repression theory; models I and III contain so many insignificant coefficients and wrong signs that they do not merit consideration. The claim that the data support the theory rests entirely on the narrow base provided by the performance of R and S in equation II, i.e., their coefficients appear to be statistically significant in the predicted direction. Appearances notwithstanding, certain statistical considerations warn against too hasty an acceptance of these results.

The major obstacle impeding the use of ordinary least squares to estimate the parameters of a time series regression is the serial correlation of the disturbances. Though ordinary least squares will provide unbiased estimates of the parameters even in the presence of serially correlated disturbances, the least squares formula for the sampling variances of the regression coefficients is biased toward zero, resulting in a serious underestimate of the "true" standard error. Since the estimated standard errors appear in the denominator of the "t" statistics, the value of the "t" statistics will be overstated, resulting in over-optimistic statements about the statistical significance of the regression coefficients.⁵ Therefore it is important to test for

⁴ A formal definition and test of casual ordering in a dynamic context has been developed by Granger (1969), and extended and applied by Sims (1972). To quote Sims, "If and only if causality runs one way from current and past values of some list of exogenous variables to a given endogenous variable, then in a regression of the endogenous variable on past, current, and future values of the exogenous variables, the future values of the exogenous variables should have zero coefficients."

⁵ Strictly speaking, in the presence of serial correlation the conventional "F" and "t" tests are no longer valid (Johnston, 1972:246).

serial correlation because it represents an alternative hypothesis accounting for the statistical significance of the regression coefficients.

Snyder and Tilly display a token awareness of the problem of serial correlation, and do not seem to appreciate the serious obstacle it poses for estimation with ordinary least squares. This is best evidenced by their failure to compute and report the Durbin-Watson "d," the standard statistic used to test for the presence of serially correlated disturbances. The "d" statistic tests the null hypothesis $p = 0$ in the first order autoregressive model $u_t = \rho u_{t-1} + e_t$ against the alternative hypothesis $p \neq 0$. Sufficiently small values of "d" permit rejection of the null hypothesis in favor of the alternative hypothesis $p > 0$, i.e., positively autocorrelated disturbances.

The problem of serially correlated disturbances informs Snyder's and Tilly's analysis as much as it does other time series regressions. For example, consider model III, the only equation for which the first order autocorrelation coefficient r (.306), the estimate of p , is reported. The Durbin-Watson "d" is closely related to r ; for large n , "d" is approximately $2(1-r)$ (Theil, 1971:201). Using this approximation the degree of serial correlation in model III can be evaluated. With $r = .306$, $d = 1.39$, which is less than 1.44, the lower bound 1% significance point of "d" with $n > 100$ and six regressors. We can therefore conclude that the disturbances in model III are positively serially correlated, and thus the statistical significance of b_2 is suspect.

Unfortunately the r for model II is not reported, so no such approximation can be achieved. Nevertheless the plausibility of positively serially correlated disturbances is well-established; and, until Snyder and Tilly demonstrate otherwise, the prudent conclusion would be that the standard errors and "t" statistics have been contaminated in such a way that the regression coefficients b_2 and b_3 appear significant when in fact they are not. This suggests that until the statistics required to make a judicious evaluation of the findings are reported, the null hypothesis $b_2 = b_3 = 0$ in model II must be retained.

This conclusion does not rely on the former objections concerning the possibility of simultaneity and the validity of the indicators; the three are independent. Taken together they imply that Snyder's and Tilly's methodology precludes any inferences whatsoever regarding the explanatory power of their political repression theory of collective violence.

CONCLUSION

In choosing to determine the relative explanatory power of two theories of collective violence, Snyder and Tilly selected a task of obvious theoretical significance, yet grossly underestimated the methodological obstacles preventing their success. The special problem of measuring "welfare," the unobserved nature of "expectations," and the statistical difficulties that invariably accompany the analysis of time series combine to render the expectation-achievement gap hypothesis intrinsically resistant to empirical test; the added problem of simultaneity makes the political repression model no less intractable.

Underlying all these problems is Snyder's and Tilly's failure to consider seriously and to resolve the logically prior issue of the legitimacy of treating the two models as mutually exclusive alternatives. They do not argue convincingly for such a partition, and a single model incorporating the essential elements of both theories has considerable intuitive appeal. Spilerman (1971:428) suggests just such a model when, in accounting for the significance of a dummy term for South in explaining racial disturbances, he notes that "past experiences of Negroes (in the South) probably operate to reduce their expectations regarding the likelihood of rapid improvement in racial or economic matters, while the remembrance of past repression may lower the rate at which they permit their frustration to be translated into hostile outbursts."

If this view is taken seriously, it implies a model in which both welfare and repression enter as expectational variables. The theory would take the general form

$$\begin{aligned} Z_t &= f((X_t - X^*), R^*), \\ f'(x - x^*) &< 0 \\ f'_{R^*} &< 0, \end{aligned}$$

where R^* is not observed repression, but rather expected repression, or the threat of violent reprisals. Translating this theory into an estimable regression equation will require not only hard decisions regarding the form of f , but also the construction of two auxiliary models of the mechanism generating X^* and R^* , possibly with each governed by a different parameter. Suffice it to say that the methodological demands of this task will amount to something more than the sum of the problems encountered in treating each explanation separately. But then, the latter approach is not without its hazards too.

CHARLES N. HALABY

Cornell University

REFERENCES

- Dhrymes, Phoebus J.
1971 *Distributed Lags: Problems of Estimation and Formulation*. San Francisco: Holden-Day.
- Granger, C. W. J.
1969 "Investigating causal relations by econometric models and cross-spectral methods." *Econometrica* 37(July):424-38.
- Griliches, Zvi
1967 "Distributed lags: a survey." *Econometrica* 35(January):17-49.
- Johnston, J.
1972 *Econometric Methods*. New York: McGraw-Hill.
- Koyck, L. M.
1954 *Distributed Lags and Investment Analysis*. Amsterdam: North Holland.
- Samuelson, Paul A.
1967 *Economics: An Introductory Analysis*. New York: McGraw-Hill.
- Sims, Christopher A.
1972 "Money, income, and causality." *American Economic Review* 62(September):540-52.
- Snyder, David and Tilly, Charles
1972 "Hardship and collective violence in France, 1830 to 1960." *American Sociological Review* 37(October):520-32.
- Spillerman, Seymour
1971 "The causes of racial disturbances: tests of an explanation." *American Sociological Review* 36(June):427-42.
- Theil, Henri
1971 *Principles of Econometrics*. New York: Wiley.
- Wallis, Kenneth F.
1969 "Some recent developments in applied econometrics: dynamic models and simultaneous equation systems." *Journal of Economic Literature* 7(September):771-96.

HOW TO GET FROM HERE TO THERE¹

Taste sits even at the wheel of econometrics. Mr. Halaby's runs to elegant, purring models, whether they reach their destination or not. We prefer a simple, lumbering conveyance pretty sure to get us there and back. In our paper (Snyder and Tilly, 1972), we tried to test a plausible version of the relative deprivation argument, one its proponents themselves used. We did not try to present the variant of the argument which we, ourselves, would find most promising. If one is dubious about a widely-accepted argument and thinks others ought to share those doubts, the wise thing to do is chal-

lenge the form of the argument actually going the rounds.

After questioning the applicability of the relative-deprivation thesis to French collective violence, we presented an alternative explanation; it centered on mobilization, contention for power, and repression. We tested one piece of it. On departing from our vehicle, we freely admitted that, until a) other parts of the arguments were represented and tested and b) the relative-deprivation thesis was tested further, the political explanation was far from conclusive. Nonetheless, our results favored it.

Mr. Halaby argues that we tested neither the "relative deprivation" nor the "struggles for political power" arguments. In any case, he says, errors in methodology invalidate our results. If that is true, owners of the October ASR had better expunge the pages containing our article . . . or cut them out for the same scrapbook that contains old ads for the Edsel.

Hold the scissors! Mr. Halaby has overdone what could have been a useful review of alternatives to our argument and to our method. First, he has mistaken our attempt to treat existing relative deprivation arguments for a failure to apply appropriately elegant models. Second, he has magnified his technical criticisms out of all proportion to their worth.

Halaby first questions the validity of our deprivation measures. He argues both that 1) the exclusive use of prices, or costs, ignores the benefits side of the welfare equation, and that 2) the low correlation of the prices indexes with the index of industrial production (as an indicator of the business cycle)² raises "additional doubts regarding validity." He says we claim these are inverse and direct measures of welfare.

Let's consider the first point first. We did, of course, run many analyses which we did not report. A number included various measures of real wages (thus we took into account both the benefits and costs of Halaby's welfare equation). Table 1 reports the intercorrelations over time of Singer-Kérel's (1961:534-5) index of real wages and the number of participants in incidents of collective violence, using both our original method (Equation (1.1) in Halaby's comment) and Halaby's preferred equation (1.4). In no case do any lagged wage coefficients significantly differ from zero at a .05 critical

¹ We are grateful to Gavin Wright and Ronald Lee for comments on an earlier draft of this note.

² Incidentally, subsequent gathering of a national income series indicates that the zero-order correlation between industrial production and the more common national income indicator of the business cycle is 0.99.

Table 1. Intercorrelations over Time of Real Wage Index and Number of Participants in Disturbances

	Time (Lag/lead)	ΔR_t Real Wages Equation (1.1)	$\Delta R_t - \Delta R_{t-1}$ Real Wages Equation (1.4)
AZ _t Number of Participants in Disturbances	5-	-.0083	.0460
	4-	.0253	.0237
	3-	-.0190	-.0313
	2-	-.0063	.0089
	1-	-.0658	-.0422
	0	.1221	.1327
	1+	.0345	-.0612
	2+	-.1953	-.1621
	3+	-.0117	.1298
	4+	-.0434	-.0223
	5+	-.0905	-.0331

NOTE:

Minus signs following entries in time column indicates that the real wage variable is lagged on, or precedes, the participants variable by the designated number of years. A plus sign following the time entry designates a lead.

level; nor are any corresponding simple regression coefficients significant. In fact, the coefficients at lag zero are the largest of any lagged wage coefficients. But their signs are opposite those expected in the relative deprivation formulation. In any case, we chose not to report these analyses (and a number of others whose results were similarly damaging to the relative-deprivation argument) because we wished to concentrate on deprivation measures which widely-cited proponents of relative deprivation had used explicitly (see the respective list of variables on our page 521). Not using such variables would have opened us to other, more serious, charges of misrepresentation.

The second point is a straw man. That we chose the industrial production and price indexes was not due to a conviction that these were either the best possible indicators of deprivation, or indicators of a single underlying dimension. Rather, as we indicated in our analysis, these variables are both common to different proponents of relative deprivation and amenable to representation in time-series format.

Halaby goes on to chide us for problems in model construction. He charges 1) that we seriously erred in including time as an independent variable; 2) that even a model respecification which excludes time [Equation (1.4)] assumes unrealistically that expectations are generated

only by the experience of the recent past; and 3) that the distributed lag model he proposes is a far better test of the relative deprivation argument. Implicit in Halaby's statements are the arguments 1) that his reformulations of the model should make an empirical difference; and 2) that the simplicity of the model we used in itself gives the relative deprivation argument less a priori chance to succeed than this more sophisticated model.

As table 1 shows, the difference between results generated from Equation (1.1) and those from Equation (1.4) is empirically slight; for substantive interpretation, it does not matter at all. Other comparisons of intercorrelations over time, and regression equations in which the price and industrial production indicators were used also manifest minimal differences. In any case, including time as an independent variable is standard econometric practice when there is substantive justification for doing so.⁸

The justification may sometimes be as vague

⁸ For the use of a procedure similar to ours in another analysis of collective action, see Ashenfelter and Johnson 1969. Recent issues of econometric journals contain plenty of analyses applying the same general logic to other problems; in *The Review of Economics and Statistics*, for example, see Pyle 1972, Toyoda 1972, Turnovsky and Wachter 1972.

as a general increase in scale, or long run secular effects in the economy. While such a justification is possible, the empirical results make this issue less pressing than Halaby suggests.

Halaby maintains that even his (1.4) depends on the assumption that current expectations are a function of the immediate past only, "an assumption few sociologists should endorse." He argues that a distributed lag model (one in which expectations are determined by the experience of the past t time periods, and in which the experience of time $t-1$ is weighted the most, etc.) would be far more appropriate. While Halaby attributes the short-run argument to us, he should have realized that we would be the last people to endorse the theory. We modeled it as stated in the literature. Hence, we tried to represent it in a fair, straightforward manner, at the cost of econometric elegance.

The simplicity of the model that we tested does not undermine the theory. Kramer (1971), for example, uses a similar model to explain a large proportion of variance in voting behavior. Given the low magnitudes and inconsistent signs of the lagged indicators of deprivation, we also doubt that the distributed lag model Halaby so confidently proposes would yield conclusions different from ours. Still, we welcome his specification and testing of the model.

Mr. Halaby then moves on to the tests of a portion of our "political struggles" argument. From the pedestal of classical econometrics, he views with scorn our awkward efforts. Our equations, he asserts, leave out the possible simultaneous effects of repression and violence on each other; and, he claims, we make no preliminary tests of causality. We recognize the simultaneity issue, but we could not represent enough of the exogenous variables we proposed to identify such a simultaneous system. Our intention was to test a portion of the argument. We thought our major purpose was clear. We wished to show the relationship between indicators of repression and indicators of violence to be plausible; we were not wedding ourselves to a given set of estimates of the model's parameters.

Regarding the indicators of repression, Mr. Halaby ignored what we wrote. He singles out the use of R , annual governmental expenditures, as forced because "Though it is certainly true that the wealthier a government the greater is its 'capacity' for violence . . . to assume that 'capacity' is automatically translated into actual repression is unjustified." The first part of this assertion agrees completely with the justification we proposed for using R ; the second part ignores our point that capacity is but a single dimension of repression, is not necessarily translated into

propensity, but should have a negative effect net of the other dimensions. Indeed, that is a primary reason for including the measure of capacity in the same model with several more direct measures of performance.

Mr. Halaby next, on the basis of a single reported first order serial correlation coefficient, holds forth at length on the effects of serial correlation on the coefficients' significance levels. We should, it is true, have reported the serial correlations and corresponding "d" statistic for all the models; their calculation shows negligible first order serial correlation for models I and II, and significant correlation for model III. No determinate statement can be made for model IV, as the "d" statistic is between the upper and lower bounds. In any case, most analysts consider the "d" statistic a guideline of the extent to which the specification of the model has gone wrong (that is the sense in which we used it). Mr. Halaby is correct in his delineation of the effects of serial correlation, but assigns too much importance to the fact that a single "d" statistic is five points below the lower boundary.

Finally, Mr. Halaby declares that "only model II approximately confirms the political repression theory; models I and II contain so many insignificant coefficients and wrong signs that they do not merit consideration." His case here rests primarily on the fact that in models I and III a single coefficient has the "wrong" sign (and one of these is significant). The point is not worth the haggling: we feel, as we did then, that (p. 529) "All things considered, the political explanations of collective violence remain plausible, while the expectation-achievement arguments lose credibility." Handsomer models of both the relative-deprivation and political-process arguments will call forth our applause. A successful synthesis of the two will fill us with admiration. The sound testing of either or both against appropriate data will delight us. Only more nit-picking disguised as major surgery could destroy our enthusiasm for further work along these lines.

DAVID SNYDER
CHARLES TILLY

University of Michigan

REFERENCES

- Ashenfelter, Orley and George E. Johnson
1969 "Bargaining theory, trade unions, and industrial strike activity." *American Economic Review* 59(March):35-49.
- Kramer, Gerald H.
1971 "Short-term fluctuations in U.S. voting behavior, 1896-1964." *American Political Science Review* 65(March):131-43.

- Pyle, David H.
1972 "Observed price expectations and interest rates." *The Review of Economics and Statistics* 54(August):275-80.
- Singer-Kérel, Jeanne
1961 *Le coût de la vie à Paris de 1840 à 1954*. Paris: Armand Colin.
- Snyder, David and Charles Tilly
1972 "Hardship and collective violence in France, 1830 to 1960." *American Sociological Review* 37(October):520-32.
- Toyoda, Toshihisa
1972 "Price expectations and the short-run and long-run Phillips Curves in Japan, 1956-1968." *The Review of Economics and Statistics* 54(August):267-74.
- Turnovsky, Stephen J. and Michael L. Wachter
1972 "A test of the 'expectations hypothesis' using directly observed wage and price expectations." *The Review of Economics and Statistics* 54(February):47-54.

EXCHANGE AS SYMBOLIC INTERACTION: FOR WHAT? *

It is difficult to determine exactly what Singelmann had in mind in his paper, "Exchange as Symbolic Interaction" (1972:414-624). At times, he calls for an integration of the exchange and symbolic interaction theories, a "dialectic unification" (p. 423). At other times he seems to be suggesting that exchange theory can simply incorporate symbolic interactionists' concepts. Whichever position Singelmann is in fact arguing for, we must disagree. The theoretical integration which Singelmann proposes would destroy both perspectives; and incorporating the concepts seems of little benefit, at least to the exchange theorist.

Singelmann begins with the argument that exchange theorists fail to recognize the "subjective meaningfulness" with which actors interpret their decision alternatives, i.e., the cognitive interpretations made between antecedent stimuli and response behavior, and that this omission costs them in predictive efficiency. He suggests that symbolic interaction theory provides the needed understanding of cognitive-interpretive processes and, therefore, that it would be useful to integrate assumptions of the two perspectives.

First, we disagree with the charge that exchange theorists fail to consider subjective meaning.¹ Singelmann, himself, cites numerous

instances in the theories of Blau (1964), Homans (1961), and Thibaut and Kelley (1959) in which cognitive-interpretive assumptions already exist. Homans, for example, freely uses the cognitive concepts of expectations and perceptions throughout his analysis. And his theoretical model is based on a complex cognitive operation involving assigning values to alternatives, weighing those alternatives, and forming a preference based on the weighting. Although Homans avoids mention of these processes in his five postulates, they are certainly explicit in his formulation.

It is true that the cognitive-interpretive assumptions in current exchange theory are incomplete, and the mediating process between stimulus and behavior needs greater specification. We doubt, however, that integrating symbolic interactionist assumptions as Singelmann proposes would do the job. Indeed, such an integration would be useless and apt to diminish predictive efficiency.

Each theory rests, in the final analysis, on its basic premises. By comparing the basic premises of exchange and symbolic interaction theories, we can begin to understand the futility of an integration. The major premises in exchange theory are: (a) that individuals will assign reward and cost values to objects in their environment, (b) that these values derive from cognitive interpretations or subjective evaluations of the objects based on past learning experiences, and (c) that the individuals will then choose or behave so as to maximize their rewards and minimize their costs (Homans, 1967). The major premises in symbolic interaction theory are: (a) that individuals will assign meanings to objects in their environment, (b) that these meanings arise out of interaction with others and are modified through a cognitive-interpretive process, and (c) that the individuals will then construct their behavior to fulfill the interpreted meanings (Blumer, 1969a).²

While these premises may appear similar on the surface, in practice they reflect fundament-

mann seems to recognize the distinction (p. 417); and his criticism is not directed at this group.

² We recognize that we caricature symbolic interaction theory in limiting ourselves to the Mead-Blumer position, while neglecting recent extensions of the theory. However, it is the Mead-Blumer position which Singelmann proposed to integrate with exchange theory; and the recent extensions of symbolic interaction theory exist as eclectic bits and pieces which cannot be systematically gathered for comparison. Thus, all we can really say is that exchange theory is incompatible with the Mead-Blumer symbolic interaction theory. But, then, this is the compatibility Singelmann was suggesting.

* The authors wish to thank Richard O. Hawkins and Robert A. Nisbet for their helpful comments.

¹ Behavioral sociologists, a group which often identifies itself with exchange theory, do tend to de-emphasize subjective meaning (cf. Burgess and Bushell, 1969; Emerson, 1972). However, Singel-

ally different assumptions as to the locus of behavior. In exchange theory, behavior is seen to be antecedently motivated by the values placed on objects in the environment and is directed towards maximizing these values. Interaction is seen more or less as a forum for gaining rewards. In symbolic interaction theory, behavior is spontaneously created, rather than motivated, by the meanings placed on objects, and is directed towards fulfilling meaning—"the imaginative completion of an act" (Meltzer, 1972:8). Interaction is treated as a process which *forms* behavior, rather than as a forum for behavior (Blumer, 1969a:8). The creation of behavior in symbolic interaction is seen as irrelevant to and, at times, even in opposition to maximizing rewards.

The central constructs of "value" and "meaning" simply do not have the same referents, nor can they be theoretically linked. In exchange theory, value refers to the expected benefit or detriment attributed to an object relative to the individual's needs. In symbolic interaction theory, meaning refers to the way in which the individual is disposed to act towards an object as it has been defined by others, e.g., "to use a chair as something in which to sit gives it the meaning of a chair" (Blumer, 1969b:69). Value cannot be systematically derived from the assignment of meaning nor can meaning be understood from the assignment of value—the constructs are independent. For example, the value of a chair will be influenced by the desire to rest or to extend one's reach, its momentary benefits or detriments, and not by the fact that others have come to define the chair as something to sit in. By the same token, the meaning of a chair is independent of its momentary utility.

Given what is clearly a conceptual independence, we cannot begin to see how the symbolic interactionist's premises would enhance the exchange theorist's understanding behavior. Obviously, exchange theorists need to know more about subjective evaluation, but it would seem that integrating symbolic interaction theory would hinder this objective. Nor do we presume that the exchange theorist's premises would enhance the theoretical understanding of symbolic interactionists.

But if an exchange theorist were willing to accept symbolic interactionist premises, then it seems he would be bound, by the nature of the beast, to accept symbolic interactionist methodology. This clearly non-experimental methodology (Blumer, 1969a; Denzin, 1969) holds that behavior "cannot be accounted for by factors which precede the act" (Blumer, 1969c:82). And while this methodology might increase the

exchange theorist's post hoc intuition, as would Freudian psychology, it would most certainly diminish predictive efficiency since there can be no a priori prediction.

If the exchange theorist cannot integrate symbolic interactionists' basic premises, might he at least adopt some of their concepts? The most promising, according to Singelmann, would be the concept of "self." Again, our answer must be no. For, though the self may be an important element in the cognitive-interpretive process (cf. Gergen, 1971; Walster, 1965), we do not believe that the symbolic interactionists' concept(s) of self would benefit the exchange theorist.

We must recognize here the important divergence in symbolic interaction theory over the self, between the Meadian traditionalists who treat it as a process and the neo-symbolic interactionists who treat it as a pseudo-structure (Meltzer and Petras, 1972). The traditionalists, led by Blumer, treat the self as a reflexive process—"a form of communication, with the person addressing himself as a person and responding thereto" (Blumer, 1969a:13). Theoretically, this self is an important mediator of behavior; but it is spontaneous and indeterminant, and thus, of little value to the exchange theorist in mapping determinant relations between antecedents and behavior. The neo-symbolic interactionists, led by Kuhn (Hickman and Kuhn, 1956), treat the self as an organization of attitudes the individual holds towards himself before interaction. Though both positions recognize a prior self, the neo-symbolic interactionists see this self as more directly related to and more determinant of subsequent behavior than the traditionalists. While this view of self which determines behavior seems more compatible with the exchange theorist's perspective, it remains of little theoretical value. The assumption of a determinant relation between self-concept and behavior is itself indeterminant, for how the self affects behavior is not specified. Rarely do proponents of this school go beyond the ambiguous statement in Kinch's (1963) postulate system that "the individual's self-concept functions to direct his behavior." Certainly, nothing in these works specifies how the self might influence evaluations. Yet, exchange theorists require precisely this specification to make use of the self-concept in understanding and predicting behavior. Thus, not even in this deterministic branch of symbolic interaction would incorporating concepts be of use.

In short, the "dialectic unification" of exchange and symbolic interaction theories proposed by Singelmann, whether through complete theoretical integration or simple incorporation

of concepts, is impractical and of no benefit to the exchange theorist. Though the two theories do seek to explain many of the same behaviors, and hence may seem compatible, their assumptions about the locus of behavior differ fundamentally and their constructs cannot be theoretically linked. Where it may seem useful to incorporate concepts as in the case of neo-symbolic interactionists' concept of self, the specification is simply insufficient. Both exchange and symbolic interaction theories need further development, but this will not be achieved by treating exchange as symbolic interaction.

CARRELL W. ABBOTT

CHARLES R. BROWN

PAUL V. CROSBIE

University of Arizona

REFERENCES

- Blau, P. M.
1964 *Exchange and Power in Social Life*. New York: Wiley.
- Blumer, H.
1969a "The methodological position of symbolic interactionism." Pp. 1-60 in H. Blumer (ed.), *Symbolic Interaction: Perspective and Method*. Englewood Cliffs: Prentice-Hall.
1969b "Sociological implications of the thought of George Herbert Mead." Pp. 61-77 in H. Blumer (ed.), *Symbolic Interaction: Perspective and Method*. Englewood Cliffs: Prentice-Hall.
1969c "Society as symbolic interaction." Pp. 78-89 in H. Blumer (ed.), *Symbolic Interaction: Perspective and Method*. Englewood Cliffs: Prentice-Hall.
- Burgess, R. L. and D. Bushell, (eds.)
1969 *Behavioral Sociology*. New York: Columbia University Press.
- Denzin, N. K.
1969 "Symbolic interactionism and ethnomethodology: a proposed synthesis." *American Sociological Review* 34:922-34.
- Emerson, R. M.
1972 "Exchange theory, Part I: a psychological basis for social exchange." In J. Berger, B. Anderson, and M. Zelditch (eds.), *Sociological Theories in Progress*, Volume II. Boston: Houghton-Mifflin.
- Gergen, K. J.
1971 *The Concept of Self*. New York: Holt, Rinehart and Winston.
- Hickman, C. A. and M. H. Kuhn
1956 *Individuals, Groups and Economic Behavior*. New York: Holt, Rinehart and Winston.
- Homans, G. C.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace and World.
- Kinch, J. W.
1963 "A formalized theory of the self-concept." *American Journal of Sociology* 68:481-6.
- Meltzer, B. N.
1972 "Mead's social psychology." Pp. 4-22 in J. G. Manis and B. N. Meltzer (eds.), *Symbolic Interaction: A Reader in Social Psychology*, second edition. Boston: Allyn and Bacon.
- Meltzer, B. N. and J. W. Petras
1972 "The Chicago and Iowa schools of symbolic interaction." Pp. 43-57 in J. G. Manis and B. N. Meltzer (eds.), *Symbolic Interaction: A Reader in Social Psychology*, second edition. Boston: Allyn and Bacon.
- Singelmann, P.
1972 "Exchange as symbolic interaction." *American Sociological Review* 37(August):414-24.
- Thibaut, J. W. and H. H. Kelley
1959 *The Social Psychology of Groups*. New York: Wiley.
- Walster, E.
1965 "The effect of self-esteem on romantic liking." *Journal of Experimental Social Psychology* 1:134-97.

ON THE REIFICATION OF PARADIGMS: REPLY TO ABBOTT, BROWN, AND CROSBIE

I am grateful to Abbott, Brown, and Crosbie for raising several important issues which space considerations did not permit me to address in "Exchange as Symbolic Interaction." Their main argument is: (1) symbolic interactionism and exchange theory rest on different constructs and assumptions which cannot be brought together without destroying either perspective, and (2) these perspectives have different utilities and therefore ought not be integrated. However, they have made several arbitrary, incorrect, and needless assumptions about the constructs and premises of both symbolic interactionism and exchange theory. The arguments for the incompatibility of the two perspectives, based on these assumptions, is equally unconvincing.

Take, for example their initial statement of the basic differences between exchange theory and symbolic interactionism. In exchange, they say, behavior is "antecedently motivated" and provides "the maximization of rewards," whereas in symbolic interaction behavior is "spontaneously created" and directed "toward the fulfillment of meaning." But there is no reason inherent in exchange theory why behavior must be completely motivated antecedently. Most exchange theorists, while recognizing the existence of prior motivations and past learning experiences, also insist that the actual processes of exchange entail constructions, bargaining, crea-

tive adjustments, etc. which define situationally how and to what extent prior motivating parameters are activated (e.g., Blau, 1964:69-85; Thibaut and Kelley, 1967:64-79, even Homans, 1958:601). And if the spontaneity of creative action in symbolic interaction is "irrelevant" to maximizing rewards, how can it be "at times even in opposition to" maximizing rewards? Could the authors of the comment have been confused by the multidimensionality of human goals (cf. Blau, 1964:18-19; Firey, 1969)? Furthermore, exchange theory does not assume that actors strive to maximize rewards. "Distributive justice" (Homans, 1961:72-8) and "fair exchange" (Blau, 1964:151-60) clearly entail the proposition that there are normatively defined limits as well as satiation points beyond which individuals do not pursue individual gain (cf. also Alexander and Simpson, 1964; Adams, 1965). As to the uniqueness of meaning in the symbolic interactionist perspective, authors such as Mead and Blumer (as recognized by Abbott, Brown, and Crosbie) have defined the concept in essentially behavioral terms which are easily transferred into exchange theory; just as "to use the chair as something in which to sit gives it the meaning of the chair," the things an employee can buy with the salary for which he exchanged his labor constitutes the meaning of the salary. Moreover, symbolic interactionists are not so esoteric as to stipulate that behavior is essentially directed toward the "fulfillment of meaning" independent of the goals, wishes and anticipations of action-outcomes (Blumer, 1969:15). I fail to see why whatever concept of meaning is selected could not apply to the mutual rewards sought in interaction. For example, a conceptualization of "meaning" in non-behavioral (such as cognitive-affective) terms can be readily integrated with the notion of "value" outlined by my critics. The fact that "value" and "meaning" cannot be "derived" from one another, which I grant readily, does not mean that the concepts cannot be linked within a common framework (as far as the logic of conceptualization is concerned, any two concepts can be linked within a larger analytical scheme). Abbott, Brown, and Crosbie define value mainly in utilitarian terms, which is questionable (the value of an "antique" chair clearly exceeds its practical utility, "momentary" or otherwise) but will do here for the sake of the argument. What they have actually accomplished is to conceptualize meaning and value interdependently: the meaning of Blumer's chair is derived from its defined utility, and in the exchange-theoretical conception the chair's value "is influenced by the desire to rest or to extend one's reach, the momentary benefits or detri-

ments of the chair. . . ." What difference does it make whether self or others make the appropriate definitions or have the appropriate desires? The meaning is still derived from the value and utility of the object. While I agree that value entails other dimensions besides meaning and meaning entails more than value, the two can be related in the way Abbott, Brown, and Crosbie unintentionally propose.

While I do not consider the self "the most promising" symbolic interactionist concept to be incorporated into exchange theory, I do think it has heuristic utility. First, if the self is conceived as spontaneous and indeterminate, it can aid us in analyzing the relationships between "antecedents" and "subsequent" behavior precisely because these relationships are largely indeterminate (although the degree of indeterminacy may certainly vary). Why should we force reality into a deterministic straitjacket to preserve the "rigor" or "elegance" of a theoretical construct created at a desk or in a laboratory? If, secondly, we conceive of the self as relatively structured, the same argument applies. The relationships between even a relatively structured self and action are relatively indeterminate because action is still constructed in terms of defined situational contingencies. To the extent that the self is predisposed to behave in given ways (which may or may not be situationally activated), the assertion that "there is no specification as to just how the self affects behavior" is simply inaccurate. Witness the growing body of self research published in journals and readers (e.g., Manis and Meltzer, 1967; Gordon and Gergen, 1968, especially the twelve articles in the section on "Self-Conception and the Determination of Behavior"; and in Educational Psychology). Even if these attempted specifications are incomplete, the intrinsic unspecifiability of relations between self and conduct is not demonstrated.

These questions raise the more general issue of determinacy and indeterminacy in social relations.¹ Abbott, Brown, and Crosbie's position seems to be that exchange theory is a relatively determinate scheme with satisfactory predictive efficiency which should not be muddled with the indeterministic elements of symbolic interactionism. But they are not consistent. In their summary of the main premises of exchange theory they mention that individuals assign re-

¹ The issue of determinacy and indeterminacy in exchange theory and symbolic interactionism was recently brought to my attention by David Maines. I am grateful for his probing questions which have helped me clarify the implications of my argument, although he may not agree with my answer.

ward and cost values to objects and that these values "are derived from cognitive interpretations or subjective evaluations. . . ." While they add that these evaluations and interpretations are "based on past learning experiences" (which symbolic interactionists need not deny), the fact remains that these (objective) learning experiences are mediated and transferred by subjective constructive processes. Soon after the authors again stress that "obviously, exchange theorists need to know more about subjective evaluation," but the criticisms they leveled against symbolic interactionism would have to apply to any approach seriously concerned with subjective processes. Similarly, they concede that "the self may be an important element in the cognitive-evaluative process," yet they "do not believe that the symbolic interactionists' conception(s) of self would be of any benefit to the exchange theorists."

In a related argument, the authors contrast a symbolic interactionist methodology which is "non-experimental" and holds (paraphrasing Blumer) that "behavior cannot be accounted for by factors which precede the act" but "might increase . . . post hoc intuition" with the "predictive efficiency" of exchange theory. It is well to recognize, however, that Blumer's radical indeterminism virtually abandons Mead's insistence on the dialectic character of social organization as simultaneously objective and subjective—a position from which a radically indeterministic and subjectivistic methodology can not be deduced. Symbolic interactionists (e.g., of the Iowa School) *are* conducting experiments. My disagreement with Abbott, Brown, and Crosbie, however, goes beyond rejecting their adherence to the position of Blumer. Their argument tends to reify certain theoretical and methodological positions, to regard them as the only valid, useful, or worthwhile positions, and to derive one-sided conceptions of reality from them. My point of departure is the reality to be theoretically grasped, rather than the theoretical constructs to be "verified." Prediction is as important as "understanding"; and when aspects of reality cannot be predicted because they are spontaneously emergent, I am quite content with understanding. I would not deny realities because they are inaccessible to a particular methodology. Homans has in this fashion reified the human animal organism which "behaves" regardless of what it says or thinks. And, indeed, he often says one thing ("I am going to be a pure behaviorist") and does another (refers to the "symbolic" significance of sentiments, to evaluations, perceptions, etc.). Blumer has reified the homo constructor at the expense of the reality

of social organization. Abbott, Brown, and Crosbie have fallen into the traps laid by both by reifying exchange theory, symbolic interactionism, and the positivistic paradigm of inquiry. Within that framework, they are correct: *that* symbolic interactionism would indeed not benefit *those* exchange theorists.

Let me conclude by restating for the critics what I "had in mind" in my paper. The notion of exchange in social life existed long before it was claimed for "exchange theory." It is fundamental for understanding many aspects of social organization. De facto exchange theory was first "claimed" by behaviorists (Homans, Emerson, e.g.) and then modified through the introduction of institutionalization, power (Blau) and cognitive processes (Thibaut and Kelley). Throughout this development, the subjective and symbolic processes which mediate exchanges have been hinted at and partly discussed but never systematically stated in general concepts and assumptions. Assuming that such processes are real and ought to be examined by social scientists, I have made a modest attempt in this direction. Exchange theory and symbolic interactionism are not monolithic constructs, and I reject any claim by a particular school to either. I have violated some assumptions of authors such as Homans and Blumer, but I have not violated the assumptions of "Exchange Theory" and "Symbolic Interactionism." Ritzer² suggests that what is called exchange theory originated within the behavioral paradigm and that my article transformed it into the "definitional" paradigm. Apparently Abbott, Brown, and Crosbie see my paper in a similar manner and deny the utility of transforming exchange theory from one paradigm into another. However, I am really not very interested in paradigms beyond their utility in making sense out of the world.³ They have no right of their own but are bound to the interests of scholars. If we are interested in the world as it is, let us select our constructs according to whether they help us elucidate that world. We do not have to swallow "the whole beast." Abbott, Brown, and Crosbie say that my "synthesis" is destructive. But so is any synthesis. And if this destruction entails the break-up of reified theoretical and methodological constructs, then so be it.

PETER SINGELMANN

University of Missouri-Kansas City

² George Ritzer: *Sociology—A Multiple Paradigm Science* (Boston: Allyn and Bacon, forthcoming). I am grateful to Professor Ritzer for his permission to preview his manuscript and cite from it.

³ Note that the title of my article was not "Exchange theory as symbolic interactionism" but "Exchange as symbolic interaction."

REFERENCES

- Adams, J. Stacey
1965 "Inequity in social exchange." Pp. 267-300
in L. Berkowitz (ed.), *Advances in Experimental Psychology*, Vol. 2, New York: Academic Press.
- Alexander, C. Norman and Richard L. Simpson
1964 "Balance theory and distributive justice." *Sociological Inquiry* 24(Spring):182-92.
- Berger, Peter L. and Thomas Luckmann
1967 *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*. Garden City: Doubleday.
- Blau, Peter M.
1964 *Exchange and Power in Social Life*. New York: Wiley.
- Blumer, Herbert
1969 *Symbolic Interactionism: Perspective and Method*. Englewood Cliffs: Prentice-Hall.
- Firey, Walter
1969 "Limits to economy in crime and punishment." *Social Science Quarterly* 50(June): 72-7.
- Gordon, Chad and Kenneth J. Gergen (eds.)
1968 *The Self in Social Interaction*. New York: Wiley.
- Homans, George C.
1958 "Social behavior as exchange." *American Journal of Sociology* 63(May):597-606.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt.
- Manis, Jerome G. and Bernard N. Meltzer
1967 *Symbolic Interaction: A Reader in Social Psychology*. Boston: Allyn and Bacon.
- Thibaut, John W. and Harold H. Kelly
1967 *The Social Psychology of Groups*. New York: Wiley.

COMMENTS ON HOPE'S MOBILITY
AND FERTILITY PAPER

Hope (1971) presents a sophisticated econometric analysis of the Berent (1952) mobility and fertility data. Berent's approach is primarily atheoretical; and while Hope (1971) intends to discuss only the impact of occupational mobility on fertility, the paper raises issues which go beyond fertility and mobility to a more general topic, *viz.* the construction of composite indicators from the interaction of two or more independent variables. Specifically, Hope (1971) fails to address the following questions: (1) how adequate are Berent's (1952) occupational categories as "classes" with distinct life styles and fertility patterns; and (2) what evidence do we have that occupational mobility effects are constant over the occupational scale or between occupational classes and not dependent on the nature of the class of destination and class of origin?

On inspecting the original study, we find that

Berent employed a peculiar measure of class, occupation only, which organized occupations into categories which only roughly correspond to status groupings, let alone classes. These occupational grades and subgrades were as follows.

- I. (A). Professional and High-Administrative
(B). Managerial and Executive
- II. (C). Inspectional and Supervisory and Other Non-Manual (higher grade)
(D). Inspectional Supervisory and Other Non-Manual (lower grades)
- III. (E). Skilled Manuals and Routine Grade of Non-Manuals
- IV. (F). Semi-Skilled Manual
Unskilled Manual

Hence, category III includes both white collar and blue collar positions; and the assumption then must be made that the social circumstances of these two groups are identical with respect to fertility. This assumption appears faulty, and Hope should have demonstrated the lack of relevance of the white collar-blue collar distinction. Class distinctions in most societies form around the association of an individual, and his work provides a context of such associations.

Berent's paper reveals a further complication for both manual and non-manual; the differences between age cohorts far exceed differences between the two occupational categories. Given the possibility that the demand structure may have changed over time, it is quite possible that the different mobility cells are overrepresented by different cohorts; and the interaction picked up by Hope (1971) may simply reflect the cohort differences in fertility. This possibility could be ruled out by comparing the mean and variance of age within cohorts. If the means differ significantly, the effects of the cohorts could be covaried out.

The preceding example suggests our final issue. Hope immediately assumes that mobility between categories I and II is moderated by the same process as mobility between I and III or II and III. Neither Hope nor his predecessors discuss the possibility that no universal or unitary mobility effect up or down is in force and that the mobility effect depends on or is conditioned by the characteristics of each combination of classes. In attacking what he calls the "halfway hypotheses" (additive effects) advanced by Duncan (1966) for fertility, and Hodge and Treiman (1966) for prejudice, Hope proposes two more complex models, one which includes polynomial terms and a second which has three mobility parameters such that: $Y = C_1 + C_2 - m + (-d_1 + d_2)$. Here C_1 and C_2 refer to the class of origin and class of destination respectively, and m is a value added if the individual

is mobile in either direction. The $(-d, +d)$ parameters are added given the direction of mobility: $+d$ for downward mobility and $-d$ upward mobility. Hence, any mobility (m) tends to suppress fertility; upward mobility ($-d$) suppresses it further; and downward mobility ($+d$) suppresses it less.

The model supposes that class of origin and class of destination do not alter the parameters; and hence, d and m are constant throughout the table. Simpson (1970) found that this need not be the case when mobility and normlessness was examined in Costa Rica. Here, mobility between white collar and blue collar produced mobility effects $(+d, -d)$. But mobility between unskilled and skilled blue collar occupations had no effect independent of the additive effects of class of origin and class of destination. Given the heterogeneity of his classes, Hope has little reason to assume that the forces governing mobility between, say, Classes I and II are the same forces governing mobility between Classes II and III.

Goodman (1969) suggests that mobility processes are complex and should not be considered as uniform throughout the occupational structure. To examine mobility tables, Goodman (1969) devises a ransacking procedure in which he examines status categories, two at a time. Here, I will extend Goodman's ransacking technique to examine the effects of mobility by using an unweighted means analysis of variance on each pair of status categories in the Berent Table. In essence, we have simply adopted an older form of analysis, unweighted means analysis of variance factorial, to examine the impact of mobility between two classes.

Our first comparison is between levels I and II (see Table 1). Here we find no statistically significant effects despite the large n 's in each cell. When examining the effects of movement between Status Levels I and II, we get a significant interaction which on inspection appears to underline a multiplicative relationship. On the other hand, when level II and level III are compared, we have a significant main effect, that of present occupation: $F=10.64$, $p<.001$.

Next, we compare levels II and IV and find that both main effects are significant: $F=13.42$ and $F=7.55$, $p<0.001$. Here the effect of Father's Class is considerably greater than present social status: $SS_{HO}=66.58$ and $SS_{PSC}=37.47$. Lastly, when we compare levels III and IV, we find that present social class has a strong effect: $F=25.24$, $p<.001$ and a small interaction: $F=3.89$, $p<.001$. We cannot compare levels I and IV due to the extremely small n 's in the mobile cells.

In all we have demonstrated that the effect

Table 1. Mean Number of Live Births per Couple, by Present Social Class and Class of Origin of Husband, Two Classes at a Time

Husband's Origin	Present Social Class	
	I	II
I	1.74	1.79
n	(65)	(43)
II	2.05	2.14
n	(33)	(197)
	PSC - N.S.	
	HO - N.S.	
	PSC HO - N.S.	
	I	
	I	III
I	1.74	1.96
n	(65)	(23)
III	1.87	2.67
n	(37)	(431)
	PSC $F = 4.68$; $p<.05$	
	HO - N.S.	
	PSC HO - N.S.	
	$\eta^2 = .013$	
	PSC	
	II	
	II	III
II	2.14	2.51
n	(197)	(150)
III	2.01	2.67
n	(154)	(431)
	PSC $F = 10.64$; $p<.001$	
	HO - N.S.	
	PSC HO - N.S.	
	$\eta^2 = .03$	
	PSC	
	II	
	II	IV
II	2.14	2.97
n	(197)	(68)
IV	3.20	3.68
n	(45)	(220)
	PSC $F = 7.55$; $p<.01$	
	HO $F = 13.42$; $p<.001$	
	PSC HO - N.S.	
	$\eta^2 = .039$	
	PSC $\eta^2 = .02$	
	HO	
	III	
	III	IV
III	2.67	3.69
n	(432)	(244)
IV	3.22	3.68
n	(162)	(220)
	PSC $F = 25.34$; $p<.001$	
	HO - N.S.	
	PSC HO $F = 3.89$; $p<.05$	
	$\eta^2 = .073$	
	PSC $\eta^2 = .011$	
	PSC HO	

of mobility differs in Berent's data depending on which status levels are involved. Hope's (1971) claim for an overall lower mobility effect (m) and upward and downward mobility effects (+d, -d) is to be questioned. Instead different combinations of strata produce different effects which cannot be attributed to chance fluctuations of the means as Hope intimates. Had Hope (1971) begun by carefully examining the impact of movement in and out of the specific pairs of classes and had he looked carefully at the relative effects of different kinds of mobility: income, occupational, educational, and geographic, we would be in a better position to specify mechanisms which link mobility to fertility. Unfortunately, with its modest sample size, the Berent (1952) study is inadequate for such a program. While a considerable effort has been put into empirical and theoretical development, the mobility-fertility question requires far more cases than previous studies have employed, and a thorough re-thinking of the contextual aspects (structural and cultural) of individual mobility and its consequences.

MILES E. SIMPSON

Texas Tech University

REFERENCES

- Berent, J.
1952 "Fertility and social mobility." *Population Studies* 5(March):244-60.
- Duncan, O. D.
1966 "Methodological issues in the analysis of social mobility." Pp. 51-97 in N. J. Smelser and M. S. Lipset, eds., *Social Structure and Mobility in Economic Development*. Chicago: Aldine.
- Goodman, L. A.
1969 "How to ransack social mobility tables and other kinds of cross classification tables." *American Journal of Sociology* 75(July): 1-40.
- Hodge, R. W. and D. J. Treiman
1966 "Occupational mobility and attitudes toward Negroes." *American Sociological Review* 31(February):93-102.
- Hope, Keith
1971 "Social mobility and fertility." *American Sociological Review* 36(December):1019-32.
- Simpson, Miles
1970 "Social mobility, normlessness and powerlessness in two cultural contexts." *American Sociological Review* 35(December): 1002-13.

ITEMS (Continued)

■ The final article is authored by **Jonathan Kelley**, Assistant Professor at Columbia and Senior Research Associate at the Center for Policy Research. He is completing a study of social mobility in primitive societies and, with Herbert Klein, is studying stratification and revolution in rural Bolivia. He is also interested in the cross-cultural study of mobility and in the structure and inter-generational transmission of political and social ideology.

■ Other comments on previously published arti-

cles, two with exchanges, conclude the issue. Authors of articles on which comments are published are customarily given an opportunity to reply to those comments. Comments on more than one published source pose special problems, and some authors either cannot be located (by us) or they choose not to respond. I am reminded of Andy Henry's response to my suggestion that we reply (somewhat heatedly I admit) to a devastating (un-fair?) review of one of our collective efforts: "Next time let's just write a better book."

J.F.S.

JOURNAL OF SOCIAL POLICY

The Journal of the Social Administration Association

Editor: D.E.G. Plowman

Professor of Social Administration

London School of Economics and Political Science

Designed to play an important role in helping to define and extend social administration as a discipline, *Journal of Social Policy* deals with the historical and theoretical analysis of social policy. A feature of the journal is the *Social Administration Digest* devoted to analyzing major changes in legislation and related matters which affect social policy.

Volume 2 Part 3 (July)

Social Policy and Social Change—Explanations of the development of social policy by John Carrier and Ian Kendall

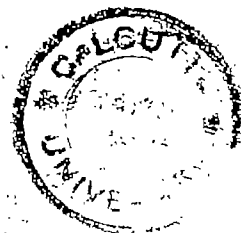
Social Administration and Sociology by Joyce Warham

Quarterly. Volume price by subscription \$15.00



Cambridge University Press

32 East 57 Street, New York, N.Y. 10022



AMERICAN SOCIOLOGICAL REVIEW
Volume 38 Number 5

Mazur	Cross-Species Status Comparison	Bonacich	A Theory of Middleman Minorities
Kunkel Nagasawa	Behavioral Model of Man	Hermalin Farley	Potential for Residential Integration
Smith	Developmental Interpersonal Dynamics	Morgan Clark	Causes of Racial Disorders
Simmons et al	Self-Image and Adolescence	Brannon et al	Attitude and Action
Jackman Jackman	Objective and Subjective Status	Williams et al	Voluntary Associations and Minority Status

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit three copies, and retain a copy for your own files.* Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—doublespaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for *Ibid.*, *op. cit.*, or *loc. cit.*; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated within reference. ["... have claimed that this is so (but see Jones, 1952:99 for conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semi-colons. ["... as many have noted (Johnson, 1942; Perry, 1947; Linquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

- Davis K.
1963a "The theory of change and response in modern demographic history." *Population Index* 29 (October):345–66.
1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.
- Goode, W. J.
1967 "The protection of the inept." *American Sociological Review* 32 (February):5–19.
- Moore, Wilbert E., and Arnold S. Feldman.
1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt (ed.)
1962 *The American College*. New York: Wiley.

* Manuscripts will not be returned unless accompanied by a self-addressed, stamped envelope.

A CROSS-SPECIES COMPARISON OF STATUS IN
SMALL ESTABLISHED GROUPS¹ALLAN MAZUR
Syracuse University

American Sociological Review 1973, Vol. 38 (October):513-530

Seven status characteristics of small established human groups are listed and then compared to the characteristics of a chicken pecking order. Chickens and humans share, at most, three of the seven characteristics. Problems of comparing human and nonhuman behavior are discussed, and a method is suggested which compares behaviors along this series of primates: tree shrew, lemur, squirrel monkey, baboon and macaque, gorilla and chimpanzee, man. The successive primates in the series are increasingly physically similar to man. The seven original status characteristics either appear throughout the primate series or emerge as we move along the series toward man. The cross-species comparison serves as a basis for criticizing several sociological theories of status.

MOST human behavior patterns vary widely from one culture to another. Many other patterns, however, appear in every culture known to history and ethnography (Murdock, 1968). One such pattern is status differentiation in small established groups.²

One school of thought assumes all universal patterns to be culturally based behaviors which, through some set of remote historical circumstances, diffused throughout the human population of the world. Another school considers many of these patterns to have evolved during the biological development of the human species and to be independent of any particular culture. In this view, behavior patterns are noncultural.

I will argue against a cultural explanation

as the sole basis for status patterns in small established human groups and draw implications for current social theory. Let me state at the outset that rejecting the cultural school of thought (in this instance) does not imply that the evolutionary school is correct. Other explanations are feasible.

STATUS CHARACTERISTICS

Small, established human groups typically have status orders with the following characteristics.

1. *Group members are fairly consistently ranked such that higher members have more power, influence and valued prerogatives than lower ranked members* (Whyte, 1955; Homans, 1950, 1961; Törrance, 1955; Strodtbeck, 1951).

2. *Low ranked members show more stress symptoms than higher ranked members; high ranked members can manipulate the stress experienced by, and thereby the performance of, low ranked members* (Rosenberg, 1965; Whyte, 1955; Price, 1967, 1969; Freedman, 1967; Maclay and Knipe, 1972).

3. *Over the long run, most members in-*

¹ I appreciate the comments and advice of Leon Robertson, Louis Kriesberg, Omar Hottenstein and Mark Abrahamson.

² "Small group" is not a precise numerical specification in the sociological literature. It implies that the group is small enough so that all members may take part in face-to-face interaction with each other. Membership may number from two to as high as one hundred.

interact more with others of similar rank ("near-peers") than with members of dissimilar rank (Whyte, 1955; Blau, 1955; Homans, 1950, 1961). There may be short periods, analogous to Bales' (1953, 1970) short-term discussion groups, when most interaction focuses on a leader. Over the long run, however, interaction occurs most often with near-peers.

4. *High ranked members usually participate more than low ranked members in group interactions*³ (Blau, 1955; Caudill, 1958). Some writers divide participation into "acts initiated" and "acts received," claiming that each increases with status, as in Bales' (1953, 1970) short-term discussion groups. But this is almost certainly a consequence of the reciprocal nature of the conversation in these contrived groups. Blau (1955) found that high-status experts in a bureaucracy received many acts but initiated few, because the interaction in that situation usually involved nonexperts requesting aid. The present generalization is expressed in terms of "participation" without specifying whether the high-status members initiate or receive acts.

5. *High ranked members—particularly the leader—perform service and control functions for other members and for the group as a whole*, e.g. controlling internal conflict, providing expertise, providing access and communication outside the group (Whyte, 1955; Blau, 1955; Gibb, 1969).

6. *An individual's rank depends, in part, on "external" attributes which are not obvious prerequisites for status in the group*. Thus a person may have high status in the small group simply because he is older, or male, or comes from a wealthy family. Berger et al. (1972) have labeled these external attributes "diffuse status characteristics."

7. *Status rank is usually established and maintained without physical fights or overt gestures of threat or submission* (Maclay and Knipe, 1972; Potter, 1970).

³ In a ranked triad, it is logically impossible for each person to interact more with his near-peer than with the other member, while at the same time the high-ranked member interacts more (in total) than the lower-ranked members. In order to keep the theoretical structure consistent, the qualifying words "most" and "usually" are included in characteristics 3 and 4.

To what extent are these seven characteristics uniquely human?

PECKING ORDER

Since Schjelderup-Ebbe (1935) first discovered the pecking order in fowls, some form of status (or dominance) behavior has been found in virtually all mammals which have been studied, in most birds, many fish and some insects. One sees frequent allusions to the human pecking order (e.g., Maclay and Knipe, 1972), and so it will be useful to examine similarities and differences between human status and the chicken pecking order.

Chicken flocks typically have one hen which can (and does) peck any other hen without being pecked in return. There is a second hen which can peck all but the top hen, a third hen which can peck all but the top two, and so on down to the last hen which is pecked by all but pecks no one in return. There is some deviance from this transitive order, particularly in large flocks, where "triangles" occur in which "beta" pecks "gamma," "gamma" pecks "delta," but "delta" pecks "beta." A flock's pecking order is generally stable although changes in rank do occur. Allee, et al. (1939) were able to induce changes by injecting male sex hormone in low-ranking hens, producing a rise in social status. Cocks always dominate hens (though they usually do not peck hens), so a breeding flock usually has separate pecking orders for each sex.

Two adult chickens who are strange to each other will usually establish relative status in their first few encounters. They may fight, in which case the first to retreat will become submissive. One chicken may submit without actual fighting, or even without receiving any overt threat. In flocks reared together from hatching, two unisexual peck orders develop at ten to fifteen weeks:

Some factors which influence success in encounters between strangers are (a) body weight or strength, (b) state of health, fatigue or severity of molt, (c) age, which is related to skill and experience, and (d) the fact that birds in their home area or in the presence of flockmates are more likely to be successful.

Once the pecking order is established, a threat, such as raising the head, is sufficient

to emphasize dominance. The frequency of pecking then decreases, though it persists at a lowered level. High-ranked chickens have priority to the food trough, dusting areas, the roost, nest boxes and choice of mates (Guhl, 1956, 1962).

We may now compare status in chickens and humans. Both species show fairly consistent ranking in terms of power, influence and valued prerogatives. Chickens, unlike humans, establish and maintain their hierarchies mainly through physical attack and overt threat. A chicken's status-relevant attributes seem to be related to physical domination (strength, experience, sex hormone level, etc.); this is less true for humans. While the top-ranked chicken is most likely to confront a strange bird introduced into the flock (Douglass 1948:155), beyond that it apparently serves no social control function for the other flock members.

Do chickens interact more with near-peers than with those dissimilar in rank? Allee et al. (1939:414) note that high-ranked hens sometimes peck their immediate inferiors more than those much lower in rank; however published peck-interaction matrices indicate that this is not a general phenomenon (Allee et al., 1939; Guhl, 1956). It therefore seems doubtful that interaction—at least peck-interaction—tends to take place more between near peers.

Is status rank correlated to degree of participation in the group? Since pecking appears to be the major form of interaction in a chicken flock, we may measure each individual's participation by summing its pecks given plus pecks received. Published data show no tendency for peck-order rank to correlate with participation in peck acts (Allee et al., 1939; Guhl, 1956; Douglass, 1948). Humans and chickens clearly differ on this point.

The only generalization for humans still to be considered concerns stress symptoms of low-ranked individuals. Several experienced observers have noted signs of stress in low-ranked chickens (Allee et al., 1939; Guhl, 1956). In one experiment, small groups of cocks were placed in a pen with hens. The dominant cock mated but apparently inhibited the lowest-ranked cock's sexual activity. "One (low-ranked) male was completely suppressed sexually; he failed to

react to the hens he knew even when the other males were removed" (Guhl, 1956: 115).

Table 6 contains a summary of the seven chicken-human comparisons. No more than three of the human status characteristics hold for chickens. The peck order appears far removed from human status processes.

PROBLEMS OF ANALOGY

It is difficult to interpret the chicken-human comparison. Similarities are interesting, but the differences are striking. One can always make analogies between any two species; but one cannot always tell if the analogies are meaningful, particularly when the two species are widely separated on the phylogenetic tree. This is best illustrated by comparing insect and human societies. They both have the most complex social "institutions" to be found in the animal world, with highly coordinated division of labor, social stratification, etc. As we move away from insects, examining animals phylogenetically closer to man, societies become less complex—less "similar" to human society. This is puzzling until, on closer inspection, we see that insect societies are based on mechanistic instinct patterns and explicit chemical stimulus communications; the division of labor is based on physically and functionally differentiated organisms, e.g., worker bees versus queen bees. In short, insect and human societies have little in common; similar behaviors are based on dissimilar mechanisms, so the interesting analogy has little substance.

This weakness is present in much current ethology where analogies are drawn between, say, the aggressive behavior of fish and the aggressive behavior of men (Lorenz, 1966). Perhaps this is meaningful or perhaps it is not; when the analogy is drawn between such widely divergent species, we have no way of knowing.

The difficulty is partly overcome by focusing on our phylogenetically "near" relatives, the Primate Order, which includes man. But there are still pitfalls. We cannot restrict our analogies to a single nonhuman primate species. For example, the gibbon is territorial—that is, a gibbon group will defend the borders of its home range against

intrusion by other gibbons. Can we conclude, by analogy, that human defense of a section of home land has the same noncultural basis as gibbon territoriality? The gibbon, like every other species, has evolved its own idiosyncratic physical and behavioral features. By looking only at gibbons, we cannot tell if territoriality is one of its idiosyncracies or a generally evolved pattern among higher primates. In fact, territoriality turns out to be a gibbon idiosyncrasy because most other "higher" primates are not territorial. The analogy, had we made it, would have been wrong.

On the other hand, we would not want to insist that every primate species be territorial before we would accept some noncultural basis for human territoriality. It would be more convincing, I think, to show that the "higher" primates—the apes—were territorial. On the other hand again, if monkey species tended to be territorial, but apes (who are physically closer to humans) were not, then we would probably not accept noncultural territoriality as a characteristic of the human species.

We may take all these considerations into account by working with a *series* of primate species, extending from those species farthest from man in physical features, to those closest to man, to man himself. I will trace status behaviors along this series, noting whether a particular behavior persists or develops as we move closer to man. If it does, I will argue that it has some noncultural basis in the human species. This method is hardly infallible, but it seems the firmest way of drawing analogies between species. It is essentially the method used by Yerkes and Yerkes (1935), and more recently by Mazur and Robertson (1972).

A PRIMATE SERIES

On morphological grounds, Napier and Napier (1967) arrange several living primate families into this progressive series: treeshrew (five genera), lemur (six genera), New World monkey (sixteen genera), Old World monkey (fourteen genera), great apes (three genera),⁴ and man (one living genus

containing one species). The genera within a given family may be quite diverse; and this complicates the progressive series because, for example, a "high" genus of New world monkey may be "higher" than a "low" genus of Old world monkey. Furthermore, as I have already mentioned, each living species has developed its own idiosyncracies, so they do not all form as smooth and consistent a series as one could ideally wish for. The gibbon, for example, defies all attempts at a neat series placement and must be accepted as an anomaly.

Primate field studies have increased enormously in the last decade, but still only a few species have been well studied (and usually only in one or two environments.) Behavioral data on the prosimians (tree shrews and lemurs) are particularly scarce. All these limitations, taken together, severely restrict the types of primate that can be considered here.

The series I will use (with genus names in parentheses), beginning farthest from man, is: tree shrew (*Tupaia*), lemur (*Lemur*), squirrel monkey (*Saimiri*), baboon (*Papio*) and macaque (*Macaca*), gorilla (*Gorilla*) and chimpanzee (*Pan*), and man (*Homo*). The squirrel monkey is one of the "lower" New World monkeys selected, in part, to insure that it is physically "lower" than the baboon and macaque, which are two closely related genera taken together to represent Old World monkeys. (My personal familiarity with squirrel monkeys and macaques was an additional consideration.) The closely related genera, gorilla and chimp, will be taken together to represent the great apes, the living primates closest to man. This specific series is consistent with recent comparative evidence on protein molecules (Goodman et al., 1970; Sarich, 1968; Goodman and Moore, 1971) and brain structure (Andy and Stephan, 1966; Stephan, 1967; Moore and Moore, 1971).⁵

⁵ The series also correlates well with gestation period (Brandt and Mitchell 1971) and learning ability (Rumbaugh, 1968, 1970; Davis et al., 1967). The original Mazur-Robertson series (1972:9-25) included tarsier and did not include gorilla. Le Gros Clark (1961) places tarsier "above" lemur, but Napier and Napier (1967) reverse the order. Given this ambiguity, and lacking tarsier social data anyway, I have dropped tarsier from the series.

⁴ Napier and Napier (1967) classify gorilla, chimpanzee and orangutan as great apes; and gibbon and siamang as lesser apes.

Recognize that each living species is the present day representative of its own diverging line of evolution. The "higher" primates in the series did *not* develop from the "lower" primates in the series. These living primates are arranged according to their physical similarity to man; they do not form an evolutionary sequence of descent. They do share common ancestors, and it would be correct to say that the present day tree shrew is more similar than man to the earliest ancestral primate (Romer, 1967).

Mazur and Robertson (1972:14-25) show that various social behavior patterns (e.g. complex play, expressive face-to-face interaction, the family bond) become progressively more humanlike as one moves along the series from tree shrew through the "higher" nonhuman primates to man. On the other hand, territorial defense behavior, which Ardrey (1966) has claimed to be a human instinct, is present among the "lower" primates but fades away as one moves up the series toward man. Thus any territorial-like behavior among humans is presumably culturally based. The series will be used here to examine the cultural basis of status in small human groups.

METHOD

I have searched the available literature for evidence of the above seven status-order characteristics among primates of the series. The modern period of primate studies is barely ten years old, so results to date are still scant and of unknown reliability. Those species which have been studied have been seen in only one or two ecological contexts, so it is not clear which behaviors are fairly invariant aspects of the species, and which are largely due to a particular environment (Jay, 1968; Alexander and Roth, 1971; Eisenberg et al., 1972; Davis et al., 1968; Jolly, 1972). Few investigators have intensively studied more than one species; methods and results between species and between investigators are not fully comparable. Practically none of the data were collected to test the characteristics of interest here. I often had to make subjective judgments as to which data were relevant, which were reliable, which were comparable, etc. Under these circumstances it is impossible to be thoroughly unbiased, though I have certainly tried to

avoid a blatantly slanted presentation. I attempted to use the following judgemental guidelines as consistently as possible.

1. Exclude data on interactions between primates of different species.

2. Exclude data on very young animals, or on interactions between adults and very young animals. So far as was practical, my analysis is confined to adult, or near adult, animals.

3. Treat status ranking within each sex separately. Within-sex interaction differs from between-sex interaction both qualitatively and quantitatively. The separation of sexes appeared to be the best way to avoid complications not related to status per se.

4. If possible, adopt the status ranking assigned by the original investigator so long as it was based on relative power, influence and prerogatives.

5. Exclude data from groups having less than four rankable animals of a given sex. In a few cases, I was forced to use groups with only three rankable animals since other data were unavailable.

6. Aggregate data where practical and not misleading. This includes combining data from separate species within a genus, an admittedly questionable procedure but one which appears to be the best way to "smooth out" unsystematic fluctuations in the data.

STATUS IN THE PRIMATE SERIES

I will discuss each of the above seven status-order characteristics in turn.

1. *Consistent Ranking of Power, Influence and Prerogatives.* Several techniques are commonly used to rank-order group members: who aggresses against, or physically displaces, whom; who has priority to food, water, sexual partners, etc.; who makes "dominance gestures" (e.g., lunges, bites, stares), and who makes "submissive gestures" (e.g., avoiding eye contact, presenting for mounting) to whom. These various measures usually correlate highly but not perfectly (Gartlan, 1968; Bernstein, 1970). Rankings are usually, but not necessarily, transitive. The relative status of two interacting individuals may depend, in part, on the proximity of "allies" (e.g., Southwick et al., 1965). Sometimes the highest ranked position is shared by a coalition of two or three animals (Hall and DeVore, 1965).

Primate groups vary in the degree to which rankings are apparent to human observers. It is often easier to identify a male ranking than a female ranking. Rankings are usually less clear in the wild than in captive colonies where animals are forced into close interaction. Also, some species simply show less overt dominance behavior than others. Dominance ranking may become prominent only during the breeding season, as in squirrel monkeys (Baldwin, 1968).

Given all of these qualifications, it is still clear that virtually all primate species studied show fairly consistent rank orders with respect to influence, power and valued prerogatives. These rankings persist over time, though individuals do occasionally change rank (Bernstein, 1970; Sorenson, 1970:160; Jolly, 1966; Nishida, 1970; Van Lawick-Goodall, 1971).

2. *Stress Symptoms in Low-ranked Members.* At the "distant" end of the primate series there is evidence of high stress among both very low- and very high-ranked animals. In a crowded captive population of tree shrews (*T. palawanensis*), the most dominant animals were very aggressive toward the most subordinate individuals who never asserted themselves; middle-ranked animals were relatively uninvolved in these agonistic interactions. Highest and lowest ranked animals died after only a few months in captivity. "The vacated positions are filled by animals next of rank and they in turn die after short periods. The (eventual) result of the continued deaths of high and low-ranked animals is a stable population composed of the original center portion of the hierarchy" (Sorenson, 1970:169, reporting work by P. Thompson).

Relevant data are lacking for lemurs and squirrel monkeys, although Ploog et al. have described the least dominant male of their captive squirrel monkey colony as "inhibited" relative to the other males (1963:63).

Several investigators have described low-ranking macaques as "nervous, insecure" or "cowering" (Southwick, 1967; Rosvold et al., 1954). Bartlett and Meier trained the members of a rhesus macaque colony to press a bar to obtain fruit. "The more dominant animals seemed more 'relaxed' as they bar-pressed, often stopping to eat as the fruit was dispensed, whereas the less dominant animals

ate as they pressed the bar and continually glanced about the room as if 'watching out' for the entrance of a more dominant animal" (1971:217).

In a particularly interesting experiment, four male rhesus macaques were daily allowed to drink as much alcohol as they wanted. The lowest-ranked animal, who had been withdrawn, seemingly fearful, and who defecated and urinated more than other members of the group, consumed the most alcohol. He subsequently became more active and aggressive and moved up to second rank in the group. The new bottom-ranked monkey became withdrawn and lessened his alcohol consumption (Peretti and Lewis 1969). These results are consistent with the notion that low status is related to high stress.

3. *Interaction with Near-peers.* Sorenson and Conaway (1966) have published interaction data on a captive group of tree shrews (three males and four females, all presumably adult.) Both agonistic and nonagonistic interactions were recorded. Table 1 shows separate interaction matrices for males and females in order to avoid cross-sex interaction complications. The top-ranked individual is designated "1," the next-ranked is "2," etc. Thus the matrix intersection 2-3 indicates the number of interactions between the second and third-ranked individuals. (Note that males interact more among themselves than females do.) The male matrix

Table 1. Tree Shrew (*Tupaia longipes*). Interaction Matrices*

		Males				Females		
Rank:		1	2			1	2	3
	2	952				2	79	
	3					3	117	86
						4	45	61
								39

Source: Sorenson and Conaway (1966).

*Matrix entries represent number of agonistic plus nonagonistic interactions between animals holding rank 1, 2, etc. at the time of the interaction.

gives no indication, and the female matrix only slight indication, that tree shrews interact most with those nearest in status. Perhaps near-peer interaction would be more prominent in a larger colony. Restricting ourselves to available evidence, however, the tendency must be doubted.

Adequate data on lemur interactions are not available.

Two careful studies are available on interactions among small groups of captive squirrel monkeys. Plotnik et al. (1968) recorded only agonistic interactions in a four-male group. Ploog et al. (1963) recorded all principal forms of social interaction in a group of four males (three adult) and two females (one adult). Male interaction matrices derived from each study are shown in Table 2. Together the matrices show a fairly consistent tendency for squirrel monkeys to interact preferentially with near-peers. This consistency is particularly striking in view of the small sizes of the groups.

The most dominant males of a macaque troupe—the “central hierarchy”—usually stay in close proximity, form an interactive clique and often act in concert (Imanishi, 1963; Simonds, 1965; Southwick et al., 1965). The lowest-ranked males associate more among themselves than with high or middle-ranked males (Kaufmann, 1967).

Macaques are the most studied of non-human primates, both in wild and laboratory conditions, so several interaction matrices have been published. Unfortunately,

these observations are usually limited to agonistic interactions. I have rearranged the published data, excluding juvenile monkeys and separating the sexes. I equalized the size of matrices (for each sex) obtained from various studies by dropping the lowest-ranked animals from the larger matrices. These equal-sizes matrices were then aggregated and are shown in Table 3. Six of the eleven males (1, 4, 7, 8, 9, 10) and four of the seven females (1, 2, 4, 7) have more interaction with closest-ranked animals than with more distant ones. Additionally, Sade's data on four adult males show a similar pattern (1967:105, 108, 110), and Bernstein and Sharpe (1966) mention that aggression appears to be directed at those closest in status.

The scant available data on macaque within-sex nonagonistic interactions—mainly grooming interaction—does not show a near-peer tendency (Sugiyama, 1971). Neither, however, does it show a counter tendency, indicating that total macaque interaction (agonistic plus nonagonistic) tends to take place between near-peers.

Although I have limited this analysis to interaction between adults of the same sex, Alexander and Bowers (1969) show that if a complete Japanese macaque troop (males, females and juveniles) is arbitrarily stratified into status classes, then there is more agonistic interaction within classes than between classes. Part of their result may be spuriously explained by two facts: (1) adult

Table 2. Squirrel Monkey (*Saimiri*) Interaction Matrices

Males (Plotnik, <i>et al.</i> , 1968)*				Males (Ploog, <i>et al.</i> , 1963)**			
Matrix entries represent number of agonistic interactions.				Matrix entries represent number of all principal forms of social interactions.			
Rank:	1	2	3		1	2	
	621				164		
	344	793			31	114	
	273	420	166				

* Data from seven experimental conditions are aggregated.

** The females and juvenile male are excluded.

Table 3. Aggregated Macaque (*Macaca*) Agonistic Interaction Matrices*

		<u>Males</u>										<u>Females</u>							
Rank:		1	2	3	4	5	6	7	8	9	10		1	2	3	4	5	6	
	2	21											2	96					
	3	21	21										3	54	43				
	4	6	7	18									4	33	37	13			
	5	18	7	13	8								5	20	47	31	43		
	6	13	25	33	5	5							6	24	25	36	23	12	
Rank:	7	2	3	9	3	4	15						7	10	13	18	10	7	31
	8	3	5	6	1	2	7	8											
	9	4	6	7	4	3	6	2	15										
	10	3	1	5	2	1	8		9	10									
	11	2	1	7		3	10	3	6	8	5								

Male sources: *M. mulatta* (Loy, 1971)
M. radiata (Simonds, 1965)

Female sources: *M. mulatta* (Sade, 1967: 105, 108, 110)
M. mulatta (Bartlett and Meier, 1971)
M. mulatta (Loy, 1971)
M. irus (Bernstein, 1968)

* Matrix entries represent the aggregate number of agonistic interactions between animals holding rank 1, 2, etc. in all source studies.

males dominate females, and adult females dominate juveniles; and (2) adult males, adult females, and juveniles all tend to interact among themselves. Thus we would expect the most dominant class in the troop to be mostly males (who interact as a sex cohort) and the least dominant class to be mostly juveniles, (who interact as an age cohort). By considering only adults, and treating the sexes separately, we avoid any spurious "near-peer" effect.

Savannah baboons appear to form central hierarchies as in macaque troops where high-ranking males associate closely together (Hall and DeVore, 1965). On the other hand, the hamadryas baboon species does not form a central hierarchy. Kummer suggests, however, that hamadryas males interact with a stable set of peers (1968:108).

Unfortunately nothing is known of the status ranks of these peers. The baboon data are too scant to draw conclusions here.

The only gorilla datum on near-peer interaction is Schaller's (1963) long observation of a large group which included five mature males. The two most dominant males were often at the center of the group, while the other three males stayed at the periphery.

Adequate chimpanzee interaction data are not available; however, Nishida (1968) has collected relevant data on pairing among six adult males in the wild. Nishida could not differentiate the status of the two top-ranked males, so I shall designate them 1' and 1". The other four males had a linear rank order and are designated 2, 3, 4, 5.

Chimps freely form temporary groups of

Table 4a. Frequency Distribution of Chimpanzee Pairs

Ranks of the Partners	Number of Times Pair Was Observed
2,3	6
3,5	5
1',2	1
2,5	1
1'',3	1
4,5	1
Total	15

Source: Nishida (1968).

variable size which may last a few hours or a few days. Table 4a shows the frequency distribution of two-male groups (i.e., pairs) observed at Nishida's feeding station. We may test whether or not near-peers have a special tendency to form pairs by first assuming the null hypothesis, i.e., that they do *not* have a near-peer tendency. This assumption allows us to calculate the "expected" number of pairings between the *i*th-ranked chimp and a chimp adjacent in status (E_{i1}), two ranks away (E_{i2}), and three or four ranks away (E_{i34}). The formulae for these expected numbers are:

$$E_{i1} = N_i p_{i1}$$

$$E_{i2} = N_i p_{i2}$$

$$E_{i34} = N_i p_{i34}$$

N_i is the number of times that the *i*th-ranked male was observed paired with any other

Table 4b. Null Hypothesis Probabilities

Rank	P_{i1}	P_{i2}	P_{i34}	N_i
1',1''	.4	.2	.4	2
2	.6	.2	.2	8
3	.4	.6	0	12
4	.4	.2	.4	1
5	.2	.2	.6	7
				30

*1' and 1'' each were paired once and are combined here.

male. p_{i1} is the probability that the *i*th-ranked male, if paired, will be with an animal of adjacent status. (p_{i2} and p_{i34} are defined analogously.) For example, the fourth-ranked chimp has two animals adjacent in rank (3 and 5) and three animals not adjacent (1', 1'' and 2), so $p_{41} = .4$. Table 4b shows the values for all p_{i1} , p_{i2} , and p_{i34} . (These probabilities are calculated on the assumptions that 1' is adjacent to 1''; and 2 is adjacent to 1', 1'' and 3.) Data for 1' and 1'' are aggregated.

Table 4c compares "expected" numbers with actual numbers of times the *i*th-ranked chimp was paired with an animal of adjacent status, etc. There is a small but fairly consistent tendency for the null hypothesis to underestimate the number of partners who are adjacent in rank, and to overestimate the number who are three or four ranks away. This indicates some tendency for near-peers to group together. Additionally, Nishida

Table 4c. Expected and Actual Numbers of Pairs

Rank	Partner Is:					
	Adjacent		2 Ranks Away		3 or 4 Ranks Away	
	E_{i1}	Actual	E_{i2}	Actual	E_{i34}	Actual
1',1''	.8	1	.4	1	.8	0
2	4.8	7	1.6	0	1.6	1
3	4.8	6	7.2	6	0	0
4	.4	1	.2	0	.4	0
5	1.4	1	1.4	5	4.2	1
Totals:	12.2	16	10.8	12	7.0	2

(1968) observed two three-male groups, both of these consisting of adjacent ranked chimps (2, 3 and 4).⁶

4. *Rank and Participation.* Table 5 shows available quantitative data on total (non-agonistic as well as agonistic) social participation in troops, as a function of status rank. Males and females are ranked separately, as usual. For each animal of the ranked sex, I have tallied his (her) interactions with all other troop members of both sexes (excluding very young animals). I then summed these tallies over all members of the ranked sex, and that is the column N, which is the basis of the percentages in Table 5.

Macaques, baboons and male tree shrews show clear rank-participation effects; the higher the rank the more participation in troop interaction. Squirrel monkeys fit this pattern to the extent that the last-ranked animal is the lowest participator. The first three female tree shrews also fit the pattern,

⁶ Nishida also observed larger groups; but here, of course, most members necessarily have adjacent ranks.

but the discrepant last ranked female is the highest participator. Tree shrew data are aggregated from two observation periods, and the discrepancy occurred in only one period, so perhaps it is a chance anomaly. Comparable data on lemurs, chimps and gorillas are lacking.

Chimpanzees freely make and break temporary groups, sometimes remaining solitary. If high-ranked chimps participate more than low-ranked chimps, then high-ranked chimps should be alone less often than low-ranked chimps. Nishida (1968) made ninety-one sightings of six male chimps, fifteen of these when they were solitary. The two top-ranked chimps were solitary 7% of the times they were sighted, the two middle-ranked chimps were solitary in 15% of their sightings, and the two bottom-ranked chimps were solitary during 24% of their sightings.

During Schaller's long study of a large gorilla band, the top-ranked male was constantly with the group while all lower-ranked mature males were away for from one to ten months of the year-long period. The leading male is clearly the focus of group attention.

Table 5. Participation in the Troop by Status Rank and Taxonomic Classification

Rank ¹	Tree Shrew ²		Squirrel Monkey ³	Baboon ⁴	Macaques ⁵
	Males (N=5951)	Females (N=2808)	Males (N=439)	Females (N=8798)	Females (N=557) ⁶
1	52%	28%	25%	27%	32%
2	26	21	59	19	26
3	22	20	17	18	24
4		31		22	19
5				14	
Rank-order	100%	100%	101%	100%	101%
Correlation:	+1.0	-.2	+.5	+.7	+1.0

¹ Status rank inferred from dominance-submission interactions.

² *Tupaia longipes*, captive (Sorenson and Conaway, 1966).

³ *Saimiri sciureus*, captive (Ploog, et al., 1963).

⁴ *Papio anubis*, captive (Rowell, 1966).

⁵ *Macaca mulatta*, captive (Bernstein and Sharpe, 1966).

⁶ N is based on number of time-units in which interaction occurred rather than interactions per se.

"Every independent animal in the group, except occasionally subordinate males, appears to be constantly aware of the location and activity of the leader either directly or through the behavior of animals in his vicinity" (Schaller, 1963:237).

5. *Service and Control Functions.* Sorenson and Conaway (1966; Sorenson, 1970) have observed many fights in captive tree shrew colonies; but they report no instance where a dominant animal broke up a fight between subordinates, or performed any other service or control function. Dominant lemurs show no tendency to interfere in spats between troop members nor to protect low-ranking animals (Jolly 1966:110).

An adult male squirrel monkey will sometimes break up agonistic encounters between two others (Ploog et al., 1963), but this is not a conspicuous activity within the troop (Baldwin 1971). Adult males also lead investigations of apparent sources of troop alarm (Baldwin 1968). It is not clear how rank enters into these activities.

Field studies of baboons (Hall and DeVore 1965, Kummer 1968, Saayman 1971) and macaques (Imanishi, 1963; Southwick et al., 1965, Kaufmann 1967) report several service and control functions performed by high-ranking males of the central hierarchy. They stop disputes between lower-ranked animals, provide close protection for mothers with infants, and take a central role in defense against predators. High-ranked males also direct troop movement, not necessarily by leading the procession, but often by supporting or "vetoing" incipient moves. Experiments have demonstrated the dominant macaque's role in controlling aggression within the group (Tokuda and Jensen 1968) and protecting other members from feigned attack by the experimenter (Bernstein 1964).

The dominant male of a gorilla group has pervasive control. "The entire daily routine—the time of rising, the direction and distance of travel, the location and duration of rest periods, and finally the time of nest building—is largely determined by the leader" (Schaller, 1963:237). He additionally plays a controlling role in intergroup contacts.

The dominant male in a chimpanzee group initiates group movement and regulates its speed and direction (Goodall, 1965; Nishida,

1970). These are temporary groups, however, and it is not known if certain individuals consistently control members of the larger population. Chimpanzees may coordinate in defense against leopards (Kortland, 1965) and in hunting small animals (Teleki, 1973); but again, it is not known if dominant males play any special role.

6. *Rank Based on "External" Attributes.* Sex and age are reliable status determinants in nonhuman primate groups, but it seems inappropriate to consider them "external" attributes (as we do for humans) since they are clearly correlated with strength, size, experience and perhaps relevant hormonal differences.⁷ The clearest example of a rank-relevant "external" attribute is when an animal's status rank is largely determined by the rank of its mother, as in Japanese and rhesus macaque troops. Sons of high-ranking mothers may attain high rank without going through the usual peripheral stage of three-to-five year old subadults (Koford, 1963). Sade (1967) observed twenty-four fights among five yearlings in a rhesus troop and found, without exception, that yearlings defeat their age peers whose mothers rank below their own and are defeated by their age peers whose mothers rank above their own. This was true independent of whether the mothers were nearby or far away. Koyama (1967) observed the same thing among Japanese macaques. This general relationship, but with an occasional reversal, exists for each age cohort into adulthood. As females become adult, they rank just below their mothers in the adult hierarchy. When males become adult, they tend to rank near their mothers; but other status-relevant factors have also come into play by this time. Most males eventually move up to rank above all females.

One could argue that the rank-correlation between mother and child is due to genetically-based physical similarities. However, if a mother's rank is raised or lowered through experimental manipulation, her child's success in fighting quickly goes up or down

⁷ These factors may also explain, in part, why sex and age are "external" status attributes for humans. For example, the male hormone testosterone is related to aggression and this, in turn, may relate to status in certain human settings (Persky et al., 1971).

accordingly (Marsden, 1968). It seems more likely that infants are socialized into their status positions. Infants of high-ranked mothers play more often between themselves, while infants of low-ranked mothers do not exhibit any preference related to social rank in their playmate choice (Fady, 1969).

Only macaques have been studied long enough to have the detailed genealogies necessary to determine status inheritance.

7. *Status Gestures*. Tree shrews establish and maintain status through threat, chase and physical attack; wounding is common and sometimes fatal (Sorenson, 1970; Vandenberg, 1963). Lemurs also use overt threat, and physical attack, sometimes tearing out fur (Jolly, 1966:99).

Squirrel monkey status interactions are often characterized by chase, physical assault, and stylized penile display (Ploog and McLean, 1963; Plotnik et al., 1968). If food is placed between a pair of squirrel monkeys, both will go for it and one may steal it from the other. The food usually ends up with the dominant monkey (Ploog et al., 1963; Mazur and Robertson, 1972). This contrasts with the placement of food between a pair of baboons or macaques, where both animals usually behave as if the food belonged to the

dominant of the pair; he picks it up while the subordinate animal averts his eyes and does not try for it (Imanishi, 1963; Hall and DeVore, 1965). This sort of status is evidently based on a well-developed normative system, and we may think of it as a deference structure equally as well as a dominance structure (Rowell, 1966). In this sense, baboons and macaques are more similar to humans than to tree shrews or lemurs. Baboons and macaques differ markedly from humans in their great dependence on overt gestures of threat and submission. Status fights do occasionally cause injury (Sade, 1957; Kummer 1968).

Chimpanzees are similar to humans in the subtlety of their status gestures and their low level of agonistic interaction. One chimp will defer to another of higher status. For example, if two meet on a narrow branch, the subordinate will allow the dominant right-of-way. Chimps are quite tolerant of each other, rarely fighting over food and occasionally even sharing it (Nishida, 1970; Van Lawick-Goodall, 1968, 1971; Teleki, 1973).

Gorillas are similar to chimpanzees in deference and tolerance except that they have not been observed to share food. Gestures are subtle and nonagonistic;

Table 6. Status Behavior Progression along the Primate Series

Status Characteristic	Chickens	Primates	
		Tree Shrew	Lemur
1. Consistent ranking of power, influence and prerogatives.	yes	yes	yes
2. Stress symptoms in low-ranked members	yes	yes (also in high-ranked)	?
3. Tendency to interact with near-peers.	doubtful	doubtful	?
4. High-ranked members participate more.	no	perhaps	?
5. High-ranked members perform service and control functions.	no	doubtful	doubtful
6. Rank based on "external" attributes.	no	?	?
7. Status gestures.	overt threats; physical attacks	overt threats; physical attacks	overt threats; physical attacks

Table 6. Continued

Primates			
Squirrel Monkey	Raboon and Macaque	Chimp and Gorilla	Man
yes	yes	yes	yes
?	yes	?	yes
yes	baboon: ? macaque: yes	perhaps	yes
perhaps	yes	chimp: perhaps gorilla: yes	yes
perhaps	yes	yes	yes
?	baboon: ? macaque: yes	?	yes
overt gestures; physical attacks; little observable deference	overt gestures; occasional physical attacks; observable deference	subtle gestures; usually nonagon- istic; deference and high toler- ance	subtle gestures; usually nonagon- istic; deference and high toler- ance

dominance is asserted with a minimum of actions. The most frequently noted gesture involving bodily contact was a light tap with the back of the fingers or hand against the back or arm of the subordinate animal. Slightly more forceful means involved pushing with both hands, or walking into the animal and pushing it over with the chest. . . . The dominant animal frequently does not assert its rank. . . . (Schaller, 1963:242).

THEORETICAL IMPLICATIONS

The foregoing results are summarized in Table 6. Many of the spaces are yet to be filled with data, and even those marked "yes" are tentative. Still, there is a clear pattern. The originally mentioned status characteristics either appear throughout the series, or they emerge as we move along the series toward man. This certainly does not imply that all characteristics of human status may be found among nonhumans. I could have selected other characteristics which were uniquely human, such as any language-related status behavior. Not all, but many important aspects of human status emerge in the series, indicating that these at least are noncultural behavior patterns of the higher primates, including man.

Modern sociology ignores man's evolu-

tionary character, perhaps because of a distaste for Social Darwinism and Nazi racism (Mazur and Robertson, 1972:chapter 1). Social theorists often explain status by positing mechanisms which are only applicable to humans, and therefore are obviously insufficient to explain comparable status patterns among nonhumans. For example, Bales explains ranking and differential participation in small groups, as well as the emergence of a leader, in terms of learning theory. His explanation assumes that the leader "is internally rewarded by the 'knowledge that he is right.'" A secondary assumption is that "both the leader and the members identify with a symbol system and the leader is identified by the members as the 'true spokesman and interpreter' of the symbol system." (1953:139).

Homans, noting that girls prefer associating with near-peers, explains that near-peers "are apt to hold similar values, and so they are apt to reward and like one another. . . ." (1961:322). Several nonhuman primates also tend to interact with near-peers, but for them an explanation in terms of "similar values" does not have much meaning.

Berger et al. (1972), in explaining why external attributes (i.e. "diffuse status char-

acteristics") influence ranking, posit a several-step chain of reasoning that is performed by each member of the group. This mechanism may logically explain why the son of a wealthy Caucasian family usually dominates the son of a poor Negro family in face-to-face interaction, but it seems inappropriate to explain similar behavior in macaques where the son of a high-status mother dominates the son of a low-status mother.

Perhaps sociologists whose interests are limited to humans should accept cross-species behaviors as "given" and requiring no further explanation, as most of us accept hunger and the sex drive as given. There are always uniquely human variations on these givens, just as there are human variations on the sex theme; and these variations could, of course, be appropriately explained in uniquely human terms. Furthermore, uniquely human mechanisms may operate *in addition* to a cross-species mechanism. For example, a cross-species explanation is needed for the observed fact that chimps, gorillas and men usually handle status affairs without violence. But it is clear that in some human groups there is an additional factor since some cultures discourage violence more than others. Therefore, in this case, there may be a cross-species *and* a uniquely human (cultural) mechanism working together. In this sense, Bales, Homans and Berger may be correctly specifying additional mechanisms which apply only to humans (or to certain humans), but their theories are incomplete since they are missing the cross-species component of the explanation.

Blau (1964) and Homans (1961) consider high status to be a desired commodity which the group gives to certain individuals in exchange for valuable services. In Blau's words:

Men who make essential contributions to a group as a whole, or to its members individually, have an undeniable claim to superior status. . . . The obligations of group members to those who make such benefits possible are discharged by according them superior status. They command respect and compliance, which serve as rewards for having made contributions in the past and as incentives for continuing to make them in the future (1964:47).

This Blau-Homans "exchange" explanation is appealing for species in which high-ranked

members *do* contribute valuable services to the group, e.g. macaques. It is doubtful that dominant tree shrews or lemurs perform comparable services, however. Yet these species still rank themselves in terms of power, influence and prerogatives. Clearly, a rudimentary status system can operate without the exchange of deference for services.

I have already pointed out that as we move along the primate series from tree shrew toward man, status interactions become characterized less by overt "hands on" power behavior and more by subtle, normatively-based deference behavior. If we restrict the scope of the Blau-Homans explanation to status systems based on deference, excluding status systems based solely on overt threats and physical attack, then the theory is consistent with the cross-species data currently available. Marked deference behavior first appears along the series among macaques and baboons, and this is the same point in the series where we see clear signs of service and control functions performed by dominant animals (Table 6). For tree shrews, lemurs and squirrel monkeys we find neither deference behavior nor clear indications of service and control behaviors. The status systems of macaques, baboons, chimps, gorillas and man have both characteristics. The fact that these two characteristics either appear jointly or not at all, is consistent with the explanation that deference is given to high-ranked individuals in exchange for their contributions to the group. I emphasize again, however, that status ranking per se is not dependent on performing any particular services for the group, since ranking appears in species where dominant individuals do not make any special contribution.

CAUTIONS

Biological explanation has a bad name in sociology, so it seems appropriate to close with some cautionary notes.

1. Social Darwinism assumed that if one institution evolved rather than another, then that one must be "fittest" and therefore best. For example, since primate groups are differentiated by status, then this pattern must be evolutionarily adaptive and therefore proper and good. This sort of reasoning is *prima facie* absurd. The fact that one pat-

tern has evolved rather than another does not imply that it is morally better than another.

2. To claim that status behavior has a noncultural basis does not dismiss the importance of experience. To the contrary, the fact that chimps and humans have subtler status-maintenance gestures than baboons, probably reflects their increased ability to learn subtle cues (footnote 5). No serious primatologist today accepts the notion of pre-programmed primate instincts that emerge full blown. Biological tendencies mature and are modified through interaction with the social-cultural environment. For example, the "external" attributes that effect ranking must be learned since they may vary from one culture to another (Faunce and Smucker 1966).

Furthermore, the fact that certain behavior patterns are biologically evolved characteristics of a species does not necessarily imply that these patterns must occur in every society of that species. Appropriate manipulation of the social-cultural-physical environment might counter the normal tendency for these patterns to emerge. This is certainly true of evolved physical characteristics. The tragic malformation of thalidomide babies as a result of their prenatal environment is well known. Certain societies modify the physical form of woman by binding girls' feet or by inserting increasingly larger plugs into holes in lips or ears. Circumcision is widely practiced in our own culture.

3. The stratification system of a large society is in many ways analogous to the status order of a small group, and it is tempting to assume that if face-to-face status behavior is a noncultural characteristic, then large-scale social stratification must also be a noncultural characteristic (Tiger and Fox, 1971; Maclay and Knipe, 1972). But there is no biological justification for this sort of explanation by analogy. We have not observed macro social stratification in other species—only status behavior in face-to-face interactions. There is no basis for generalizing from one level to another.

REFERENCES

- Alexander, B. and J. Bowers
1969 "Social organization of a troop of Japanese

- monkeys in a two-acre enclosure." *Folia Primatologica* 10:230-42.
- Alexander, B. and E. Roth
1971 "The effects of acute crowding on aggressive behavior of Japanese monkeys." *Folia Primatologica* 39:73-90.
- Allee, W., N. Collias and C. Lutherman
1939 "Modification of the social order of flocks of hens by the injection of testosterone propionate." *Physiological Zoology* (October):412-40.
- Andy, O. and H. Stephan
1966 "Psychogeny of the primate *septelencephali*." Pp. 389-99 in R. Hassler and H. Stephan (eds.) *Evolution of the Forebrain*. New York: Plenum Press.
- Ardrey, R.
1966 *The Territorial Imperative*. New York: Atheneum.
- Baldwin, J.
1968 "The social behavior of adult male squirrel monkeys (*Saimiri sciureus*) in a seminatural environment." *Folia Primatologica* 9:281-314.
- 1971 "The social organization of a semifree ranging troop of squirrel monkeys (*Saimiri sciureus*)." *Folia Primatologica* 14:23-50.
- Bales, R.
1953 "The equilibrium problem in small groups. Chapter 4 in T. Parsons, R. Bales and F. Shils, *Working Papers in the Theory of Action*. New York: Free Press.
- 1970 *Personality and Interpersonal Behavior*. New York: Holt, Rinehart and Winston.
- Bartlett, D. and G. Meier
1971 "Dominance status and certain operants in a communal colony of rhesus macaques." *Primates* 12 (December):209-19.
- Berger, J., B. Cohen and M. Zelditch, Jr.
1972 "Status characteristics and social interaction." *American Sociological Review* 37 (June):241-55.
- Bernstein, I.
1964 "Role of the dominant male rhesus monkey in response to external challenges to the group." *Journal of Comparative and Physiological Psychology* 57 (June):404-6.
- 1968 "Social status of two hybrids in a wild troop of *Macaca irus*." *Folia Primatologica* 9:121-31.
- 1970 "Primate status hierarchies." Pp. 71-109 in L. Rosenblum (ed.), *Primate Behavior Volume 1*. New York: Academic Press.
- Bernstein, I. and L. Sharpe
1966 "Social roles in a rhesus monkey group." *Behavior* 26:91-104.
- Blau, P.
1955 *The Dynamics of Bureaucracy*. Chicago: University of Chicago Press.
- 1964 *Exchange and Power in Social Life*. New York: Wiley.
- Brandt, E. and G. Mitchell
1971 "Parturition in primates." Pp. 177-225 in L. Rosenblum (ed.), *Primate Behavior, Volume 2*. New York: Academic Press.

- Caudill, W.
1958 *The Psychiatric Hospital As a Small Society*. Cambridge: Harvard University Press.
- Davis, R., R. Leary, M. Smith and R. Thompson
1968 "Species differences in the gross behavior of nonhuman primates." *Behavior* 31: 326-38.
- Davis, R., R. Leary, D. Stevens and R. Thompson
1967 "Learning and perception of oddity problems by lemurs and seven species of monkey." *Primates* 8(December):311-22.
- Douglas, M.
1948 "Social factors influencing the hierarchies of small flocks of the domestic hen." *Physiological Zoology* 21(April):147-82.
- Eisenberg, J., N. Muckenhirn and R. Rudran
1972 "The relations between ecology and social structure in primates." *Science* 176(May 26):863-74.
- Fady, J.
1969 "Les jeux sociaux." *Folia Primatologica* 11:134-43.
- Faunce, W. and M. Smucker
1966 "Industrialization and community status structure." *American Sociological Review* 31(June):390-9.
- Freedman, D.
1967 "A biological view of man's social behavior." Pp. 152-88 in W. Etkin, *Social Behavior from Fish to Man*. Chicago: University of Chicago Press.
- Gartlan, J.
1968 "Structure and function in primate society." *Folia Primatologica* 8:89-120.
- Gibb, C.
1969 "Leadership." Pp. 205-82 in G. Lindzey and E. Aronson (eds.), *The Handbook of Social Psychology*, Volume 4. Reading, Mass.: Addison-Wesley.
- Goodall, J.
1965 "Chimpanzees of the Gombe Stream Reserve." Pp. 425-73 in I. DeVore (ed.), *Primate Behavior*. New York: Holt, Rinehart and Winston.
- Goodman, M. and G. Moore
1971 "Immunodiffusion systematics of the primates: I. The catarrhini." *Systematic Zoology* 20:19-62.
- Goodman, M., G. Moore, W. Farris and E. Poulik
1970 "The evidence from genetically informative macromolecules on the phylogenetic relationships of the chimpanzees." Pp. 318-60 in G. Bourne (ed.), *The Chimpanzee*, Volume 2. Baltimore: University Park Press.
- Guhl, A.
1956 "The social order of chickens." Pp. 112-16 in *Psychobiology: The Biological Bases of Behavior*. San Francisco: W. H. Freeman.
1962 "The behavior of chickens." Pp. 491-530 in E. Hafez (ed.), *The Behavior of Domestic Animals*. Baltimore: Williams and Wilkins.
- Hall, K. and I. DeVore
1965 "Baboon social behavior." Pp. 53-110 in I. DeVore (ed.), *Primate Behavior*. New York: Holt, Rinehart and Winston.
- Homans, G.
1950 *The Human Group*. New York: Harcourt, Brace & Co.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace and World.
- Imanishi, K.
1963 "Social behavior in Japanese monkeys, *Macaca fuscata*." Pp. 68-81 in C. Southwick (ed.), *Primate Social Behavior*. Princeton: Van Nostrand.
- Jay, P. (ed.)
1968 *Primates: Studies in Adaptation and Variability*. New York: Holt, Rinehart and Winston.
- Jolly, A.
1966 *Lemur Behavior*. Chicago: University of Chicago Press.
1972 *The Evolution of Primate Behavior*. New York: Macmillan.
- Kaufmann, J.
1967 "Social relations of adult males in a free-ranging band of rhesus monkeys." Pp. 73-98 in S. Altmann (ed.), *Social Communication Among Primates*. Chicago: University of Chicago Press.
- Koford, C.
1963 "Rank of mothers and sons in bands of rhesus monkeys." *Science* 141(July 26): 356-7.
- Kortlandt, A.
1965 "How do chimpanzees use weapons when fighting leopards?" *Yearbook of the American Philosophical Society*:327-32.
- Koyama, N.
1967 "On dominance rank and kinship of a wild Japanese monkey troop in Arashiyama." *Primates* 8(September):189-216.
- Kummer, H.
1968 *Social Organization of Hamadryas Baboons*. Chicago: University of Chicago Press.
- Le Gros Clark, W.
1961 *History of the Primates*. Chicago: University of Chicago Press.
- Lorenz, K.
1966 *On Aggression*. New York: Harcourt, Brace and Jovanovich.
- Loy, J.
1971 "Estrous behavior of free-ranging rhesus monkeys (*Macaca mulatta*)." *Primates* 12 (March):1-31.
- Maclay, G. and H. Knipe
1972 *The Dominant Man: The Pecking Order in Human Society*. New York: Delacorte Press.
- Marsden, H.
1968 "Agonistic behavior of young rhesus monkeys after changes induced in social rank of their mothers." *Animal Behavior* 16(February):38-44.

- Mazur, A. and L. Robertson
1972 *Biology and Social Behavior*. New York: Free Press.
- Moore, J. and R. Moore
1971 "A comparative study of the superior olivary complex in the primate brain." *Folia Primatologica* 16:35-51.
- Murdock, G.
1968 "The common denominator of cultures." Pp. 230-45 in S. Washburn and P. Jay (eds.) *Perspectives on Human Evolution*: 1. New York: Holt, Rinehart and Winston.
- Napier, J. and P. Napier
1967 *A Handbook on Living Primates*. London: Academic Press.
- Nishida, T.
1968 "The social group of wild chimpanzees in the Mahali Mountains." *Primates* 9(September):167-224.
1970 "Social behavior and relationship among wild chimpanzees of the Mahali Mountains." *Primates* 11(March):47-87.
- Peretti, P. and B. Lewis
1969 "Effects of alcoholic consumption on the activity patterns of individual rhesus monkeys and their behavior in a social group." *Primates* 10(June):181-8.
- Persky, H., K. Smith and G. Basu
1971 "Relation of psychologic measures of aggression and hostility to testosterone production in man." *Psychosomatic Medicine* 33(May-June):265-77.
- Ploog, D., J. Blitz and F. Ploog
1963 "Studies on social and sexual behavior of the squirrel monkey (*Saimiri sciureus*)." *Folia Primatologica* 1:29-66.
- Ploog, D. and P. MacLean
1963 "Display of penile erection in squirrel monkey (*Saimiri sciureus*)." *Animal Behavior* 11:32-9.
- Plotnik, R., F. King and L. Roberts
1968 "Effects of competition on the aggressive behavior of squirrel and cebus monkeys." *Behavior* 32:313-32.
- Potter, S.
1970 *The Complete Upmanship*. New York: Holt, Rinehart and Winston.
- Price, J.
1967 "The dominance hierarchy and the evolution of mental illness." *Lancet* (July 29): 243-6.
1969 "The ritualization of agonistic behavior as a determinant of variation along the neuroticism/stability dimension of personality." *Proceedings of the Royal Society of Medicine* 62:1107-10.
- Romer, A.
1967 "Major steps in vertebrate evolution." *Science* 158(December 29):1629-37.
- Rosenberg, M.
1965 *Society and the Adolescent Self Image*. Princeton: Princeton University Press.
- Rosvold, H., A. Mirsky and K. Pribram
1954 "Influence of amygdectomy on social behavior in monkeys." *Journal of Comparative and Physiological Psychology* 47: 173-8.
- Rowell, T.
1966 "Hierarchy in the organization of a captive baboon group." *Animal Behavior* 14, (October):430-43.
- Rumbaugh, D.
1968 "The learning and sensory capacities of the squirrel monkey in phylogenetic perspective." Pp. 256-317 in L. Rosenblum and R. Cooper (eds.), *The Squirrel Monkey*. New York: Academic Press.
1970 "Learning skills of anthropoids." Pp. 2-70 in L. Rosenblum (ed.), *Primate Behavior*, Volume 1. New York: Academic Press.
- Saayman, G.
1971 "Behavior of an adult Malesina troop of free-ranging chacma baboons (*Papio ursinus*)." *Folia Primatologica* 15:36-57.
- Sade, D.
1967 "Determinants of dominance in a group of free-ranging rhesus monkeys." Pp. 99-114 in S. Altmann (ed.), *Social Communication Among Primates*. Chicago: University of Chicago Press.
- Sarich, V.
1968 "The origins of the hominids: An immunological approach." Pp. 94-121 in S. Washburn and P. Jay (eds.), *Perspectives on Human Evolution*: 1. New York: Holt, Rinehart and Winston.
- Schaller, G.
1963 *The Mountain Gorilla*. Chicago: University of Chicago Press.
- Schjelderup-Ebbe, T.
1935 "Social behavior of birds." Pp. 947-72 in C. Murchison (ed.), *A Handbook of Social Psychology*, Volume 2. New York: Russell and Russell.
- Simonds, P.
1965 "The bonnet macaque in South India." Pp. 175-96 in I. DeVore (ed.), *Primate Behavior*, New York: Holt, Rinehart and Winston.
- Sorenson, M.
1970 "Behavior of tree shrews." Pp. 141-93 in L. Rosenblum (ed.), *Primate Behavior*, Volume 1. New York: Academic Press.
- Sorenson, M. and C. Conaway
1966 "Observations on the social behavior of tree shrews in captivity." *Folia Primatologica* 4:124-45.
- Southwick, C.
1967 "An experimental study of intragroup agonistic behavior in rhesus monkeys (*Macaca mulatta*)." *Behavior* 28:182-209.
- Southwick, C., M. Beg and M. Siddiqi
1965 "Rhesus monkeys in North India." Pp. 111-59 in I. DeVore (ed.), *Primate Behavior*. New York: Holt, Rinehart and Winston.
- Stephan, H.
1967 "Zur Entwicklungshöhe der Primaten nach Merkmalen des Gehirns." Pp. 108-20 in D. Starck, R. Schneider and H. Kuhn (eds.),

- Progress in Primatology. Stuttgart: Fischer-Verlag.
- Strodtbeck, F.
1951 "Husband-wife interaction over revealed differences." *American Sociological Review* 16(August):141-5.
- Sugiyama, Y.
1971 "Characteristics of the social life of bonnet macaques (*macaca radiata*)." *Primates* 12 (December):247-66.
- Teleki, G.
1973 "The omnivorous chimpanzee." *Scientific American* 228(January):32-42.
- Tiger, L. and R. Fox
1971 *The Imperial Animal*. New York: Holt, Rinehart and Winston.
- Tokuda, K. and G. Jensen
1968 "The leader's role in controlling aggressive behavior in a monkey group." *Primates* 9 (December):319-22.
- Torrance, E.
1955 "Some consequences of power differences on decision making in permanent and temporary three-man groups." Pp. 482-91 in A. Hare, F. Borgatta and R. Bales (eds.), *Small Groups*. New York: Knopf.
- Vandenbergh, J.
1963 "Feeding, activity and social behavior of the tree shrew, *Tupaia glis*, in a large outdoor enclosure." *Folia Primatologica* 1: 199-207.
- VanLawick-Goodall, J.
1968 "A preliminary report on expressive movements and communication in the Gombe Stream chimpanzees." Pp. 313-374 in P. Jay (ed.), *Primates: Studies in Adaption and Variability*. New York: Holt, Rinehart and Winston.
- 1971 *In the Shadow of Man*. New York: Dell.
- Whyte, W.
1955 *Street Corner Society*, 2nd ed. Chicago: University of Chicago Press.
- Yerkes, R. and A. Yerkes
1935 "Social behavior in infrahuman primates." Pp. 973-1033 in C. Murchison (ed.), *A Handbook of Social Psychology*, Volume 2. New York: Russell and Russell.

A BEHAVIORAL MODEL OF MAN: PROPOSITIONS AND IMPLICATIONS *

JOHN H. KUNKEL

University of Western Ontario

RICHARD H. NAGASAWA

Arizona State University

American Sociological Review 1973, Vol. 38 (October):530-543

The requirements which a model of man must meet to be sociologically significant are outlined. To overcome the limitations of presently popular behavioral models, a model of man based on recent studies of human learning, especially those of Bandura, is presented. A description of complexities introduced by recent empirical studies is followed by a summary set of formal axioms, and several illustrative theorems are then derived.

HOMANS' behavioral propositions and their role in explaining sociological phenomena have intrigued sociologists for more than a decade. While there has been quite a debate over the adequacy of these propositions and their implications for sociological theory, little has been done to assess and update them in the light of current work in psychology. This article examines the empirical evidence which has accumulated since *Social Behavior* was published and attempts to improve the propositions so that their implications for sociology can be specified and evaluated.

MODELS OF MAN

Most sociologists, even those whose work does not center on individuals, must make assumptions about man. These may be implicit and rather vague, as in Parsons' description of the actor with a set of expectations, need-dispositions, cognitive orientations, and goal direction (1951:5-23). Or they may be explicit and specific, as in Lenski's propositions that men are motivated by self-interest and tend to be creatures of habit (1966:30-2). The limited set of assumptions about those selected aspects of man which a sociologist may require in his later theoretical and empirical work is usually referred to as a "model of man" (e.g. Simon, 1957). Such models are derived from one or another theory of personality

* We wish to thank Professor Philip von Bretzel for assistance with the calculus used in this paper, and Professor Peter Killeen for helpful comments on behavioral principles. We also express our appreciation to the anonymous ASR referees.

and vary greatly in content, theoretical closure, and empirical support (e.g. Hall and Lindzey, 1970).

While psychologists spend much time developing and validating the components of such models, sociologists usually consider models as given elements and proceed to their primary concern, the analysis of social structures and processes. Models of man in sociology suggest hypotheses and ideas about the structure and operation of social systems (e.g. Parsons, 1951) or small groups (e.g. Homans, 1961; Maris, 1970). In addition, models may be combined with various assumptions about society (e.g. Lenski, 1966) and organizations (e.g. Udy, 1965) to yield explanatory schemes about such topics as stratification and bureaucracies. Hence it is only the sociologist so enamored with his model of man that he forgets the study of men who can be justly accused of "bartering [his] sociological heritage for a mess of psychological pottage" (Schnore, 1959:634). Generally speaking, a model of man will be useful to the extent to which it contains simple, testable, and refutable propositions in the following areas of sociological concern:

1. the establishment of behavior;
2. the maintenance of behavior;
3. the extinction of behavior;
4. the modification of behavior (usually a combination of [1] and [3]).

Such a model can then be used to describe large-scale processes (e.g. Kunkel, 1970) and small-group phenomena (e.g. Emerson, 1972). Contrary to what is implied in much recent literature, behavioral models of man cannot be equated with exchange theory; the former treat the activities of individuals, while the latter involve dyads (e.g. Homans, 1961) and larger groups (e.g. Blau, 1965). Models of man provide sociologists with propositions about human behavior in general which can be applied to a variety of phenomena, including interactions and social exchange. The fact that exchange theory is only one application of behavioral principles and provides only one approach to social psychology is seen by comparing Homans' (1961) and McGinnies' (1970) approaches to the field. In this paper we will concentrate on models of man as such and not on the specific theories in which they might be used.

HOMANS' MODEL

The behavioral model of man best known in sociology is George C. Homans' (1961), developed during the late 1950's according to concepts and data available at that time. It consists of four axioms based largely on elementary economics and Skinner's paradigm of animal studies. Homans derived five theorems from these axioms and used them to explain successfully numerous findings described in the social-psychological literature (e.g. Maris, 1970).¹ The theorems form the core of "exchange theory" (e.g. Emerson, 1969) and have become so identified with modern-day social behaviorism that learning theory and exchange theory are almost regarded as one.

During the last ten years, these nine propositions, and especially the theorems, have been criticized on several grounds. First, the empirical foundation—mainly the results

¹ The most important axioms concerning individual organisms are (Homans, 1961:28-9):

1. "The more fully an animal has been deprived of a positive reinforcer, the more often it emits an activity so reinforced; the more fully it has been satiated, the less often it does so."
2. "The more often an activity is reinforced, the more often the animal emits it; the more often an activity is punished, the less often the animal emits it."
3. "The withdrawal of a positive reinforcer releases the emotional behavior we call aggression, the presentation of a positive reinforcer may release, besides the reinforced activity, some degree of positive emotional behavior."
4. "Any increase in the frequency of a particular activity entails by that very fact a decrease in the frequency of an alternative activity."

The five theorems concerning human (social) behavior are (Homans, 1961:53-5, 75):

- I. "If in the past the occurrence of a particular stimulus situation has been the occasion on which a man's activity has been rewarded, then the more similar the present stimulus situation is to the past one, the more likely he is to emit the activity, or some similar activity, now."
- II. "The more often within a given period of time a man's activity rewards the activity of another, the more often the other will emit the activity."
- III. "The more valuable to a man a unit of the activity another gives him, the more often he will emit activity rewarded by the activity of the other."
- IV. "The more often a man has in the recent past received a rewarding activity from another, the less valuable any further unit of that activity becomes to him."
- V. "The more to a man's disadvantage the rule of distributive justice fails of realization, the more likely he is to display the emotional behavior we call anger."

of laboratory studies with animals, especially pigeons—has bothered both sociologists and philosophers. Those who reject the assumption of phylogenetic continuity, or who in other ways distinguish between pigeons and people, either demand human data or hold that studies with humans would result in different or at least modified propositions. Second, the logical status of the propositions, as well as the claim that they are among the most general of any sociological explanation, have been questioned and denied. Third, some suggest that the propositions in their present form are too simple, too few, and too generally phrased. Factors in human behavior are thought to be more complex, more numerous, and much more specific than is apparent in Homans' five propositions and the "other things being equal" caveat.

However, the assessment of any model must distinguish among the content of its constituent propositions, their empirical foundation, the specific descriptive phrases used, and the claims made for its applicability. Each may vary independently and usually reflects an author's time, place, and purpose. Homans' behavioral model of man relies heavily on animal studies, but this reliance is an artifact of the history of psychology; in the 1950's there simply were not many human studies, and very few theoretical treatments of human learning existed (cf. Hilgard, 1953; Hilgard and Bower, 1966). Current human evidence does empirically support behavioral models based on learning (e.g. Bandura, 1969), and we suspect that it is not so much the behavioral model of man, but rather the claims made for it, as well as its use in sociological theory (e.g. Homans, 1964) and everyday affairs (e.g. Skinner, 1971) which has made sociologists uneasy. Finally, Homans noted only a few of the "other things" which must be equal for the propositions to hold (e.g. 1961: 21, 28-9, 51-5). Research of the last fifteen years has greatly increased information about what these "other things" are and how they affect the validity and applicability of each proposition.

A BEHAVIORAL MODEL OF MAN

Homans' theorems and axioms concern behavior which presently exists, has already

been learned and for which specific discriminative stimuli and contingency probabilities have been learned through an individual's previous experiences. Such a concern with maintenance and extinction (and by implication behavior modification), however, does not exhaust the questions of interest to sociologists. A more complete model should also describe the establishment of new activities, and would treat behavior modification explicitly.

Homans' theorems provide only one explanation of how activities are learned: if a new activity should occur and be reinforced, it is more likely to recur, and when it is emitted at a relatively high rate and in appropriate circumstances, we can say it has been "learned." Note, however, that behavior must occur before it can be reinforced; hence a new behavior must "exist" before it can be "learned." While this situation holds in some cases, much evidence argues (e.g. Bandura, 1969) that most human learning proceeds through modeling rather than direct experience, as Homans' model implies. In fact, life would be quite dangerous if people did not rely on instructions, observations, and imitation. Complex activities and drawn-out schedules of reinforcement, especially, are likely to be learned only through instructions. An adequate model of man, then, should contain propositions about the maintenance, extinction, and establishment of behavior; and for the latter there should be propositions about direct and observational learning. Furthermore, one should be able to derive the propositions of a behavioral model of man from human studies. By relying exclusively on human studies, one should be able to take into account whatever characteristics are uniquely human; similarities of men and animals might become apparent but would be irrelevant, and a major critique of the model as well as an important source of polemics would thereby disappear.

The Building Blocks

Our major sources of data are such current studies of human learning as are found in Bandura (1969), Baron and Liebert (1971), Krasner and Ullmann (1965), Ullmann and Krasner (1965) and the *Journal of Applied Behavior Analysis*. The evidence described

and analyzed in these and other recent works will be summarized in generalizations which become the basic components of our model. We will treat them as higher-level propositions from which lower-level hypotheses will be derived. Together they should enable us to describe, explain, and predict a wide range of activities which constitute social structures and processes.

The basic idea of the model, and the major conclusion of literally hundreds of studies, is that most behavior patterns of interest to sociologists are learned, maintained, extinguished, and modified by means of differential reinforcement and punishment. The consequences of a person's previous activities, which greatly affect the probability of future repetition, may be directly or vicariously experienced. Present circumstances provide information about probable future events based on past experiences, and thus serve as signals for present behavior.

While empirical evidence for this thumbnail sketch of general learning models is impressive, the whys and wherefores are less well understood. The relationship between behavior, past and present environmental events, and internal processes has been treated variously by such investigators as Hull (1951), Rotter (1954), Skinner (1953), and Bandura (1969), to mention only a few. Since the sociologist's interest in overt behavior and its relationship to present and past social events can be reasonably well satisfied however the internal structure and operation of the human "black box" are conceived, it will be enough to abstract principles from learning studies rather than to subscribe to a particular learning theory.

Such a procedure is especially advisable today because there are at least two major behavioral schools. One has a long history of experimental work, emphasizes direct experiences, and is associated mainly with Skinner. Since the work of this school underlies Homans' model of man, the characteristics of its experimental paradigm have entered the study of social behavior and exchange. Another and more recent school, associated mainly with the work of Bandura but foreshadowed by Miller and Dollard (1941), allows for observations and vicarious experiences and includes a host of specific

propositions about human learning and behavior. These propositions and their experimental and naturalistic support contribute significantly to the humanization of the behavioral approach.

AXIOMS OF A BEHAVIORAL MODEL OF MAN

Introduction

We now turn to the model—first, to its empirical foundations and various logical considerations, and then to its axioms. Since a sociological treatise is not the place to describe psychological experiments and other studies in detail, we will limit ourselves to listing the significant sources of empirical evidence underlying an axiom. A major problem we encounter is the complexity of elements and relationships involved in acquiring, maintaining, and modifying behavior. As the results of experimental and naturalistic studies accumulate—Bandura (1969), for example, requires more than six hundred pages to analyze hundreds of human studies—it becomes increasingly difficult to generalize without doing violence to qualifying data. A sociologically relevant general model of man, however, need not include all the processes important to psychologists. Our discussion of reinforcement, for example, assumes intermittent schedules but is not concerned with the differing behavioral effects of variable-interval and variable-ratio schedules. Since one article cannot solve the problems of complexity and generality, we have been content with verbal descriptions of some of the intricacies along with the formal summary statements.

The Reinforcement Principle

The idea that an activity's past consequences affect the probability of its repetition has been expressed and elaborated by many psychologists, whose list of diverse experiments and studies is truly impressive (e.g. Hilgard and Bower, 1966). From Thorndike's "law of effect" through Hull's systematic formulations and Skinner's "operant conditioning" to Bandura's studies of behavior modification, the reactions of an individual's social and physical context to his present behavior have been considered the major determinants of his future activities (e.g. Herrnstein, 1970).

The most general statement of this relationship—a person rewarded for an activity is likely to repeat it—has often been thought circular; and the definition of reward (or reinforcer) has been viewed as a tautology. However, this problem arises only when a reinforcer is defined exclusively in terms of a particular preceding activity (for a solution, see Burgess and Akers, 1966). Human studies generally rely on either the trans-situational character of complex reinforcers and their relations to a variety of deprivations, or on preference hierarchies. That is, if something is known to reinforce a particular activity at a specific time and place, one may assume it is likely to reinforce other behavior patterns at other times and places as well (Hilgard and Bower, 1966:481 ff.). And when activities are ordered in a hierarchy of relative preference (based, for example, on time spent on them in a free environment), a “higher” behavior will reinforce any “lower” activity (Premack, 1965).

Many sociologists have viewed reinforcers in terms of deprivations, such as the lack of money or status, and have produced several careful analyses of cultural or learned reinforcers and their workings in social systems. Weber's (1947) analysis of prestige, Warner's (1960) discussion of occupation, reputation, and source of income as class indicators, as well as Lenski's (1966) and Merton's (1957) treatments of monetary and other values or goals, are excellent illustrations of diverse and significant reinforcers and deprivations in social life.

The term “reinforcer” can be applied to material objects, symbols, or the behavior of another person; while “reinforcement” refers to both the process of presentation and the individual's experience. The latter term is quite complex, for it includes not only the external presentation of reinforcers and removal of aversive stimuli (such as turning off a loud radio) but also the self administering of rewards (thus Bandura, 1969:32–8 attaches great importance to self-reinforcement). All three types of events increase the probability that the preceding activity will be repeated.

The literally hundreds of experiments, studies, and observations concerning the relationship between behavior, its consequences, and its replication, which have

filled psychological journals over the years, have been analyzed by Bandura (1969: 217–92); our first axiom summarizes them:

1. If an activity is followed by a reinforcer, it is likely to recur in similar circumstances.

The Modeling Principle

When a new activity becomes part of an individual's behavioral repertoire, we usually speak of acquisition or establishment. The processes involved, generally subsumed under “learning,” fall into two categories. The first, direct learning, is usually associated with the writings of Skinner (e.g. 1953, 1961) and his followers. Briefly, the relevant studies indicate that an individual learns through his own experiences which amount to trial and error: the “correct” activities are reinforced by some environmental reaction, while the “errors” are either punished or ignored. Obviously, a behavior cannot be reinforced unless it has occurred, hence learning is partly a matter of luck and, in the case of complex human activities, a long and arduous process. The procedure of “successive approximation” in which an approximation of the eventual behavior is reinforced, is commonly used; but here as well as in studies of imitation (e.g. Baer, et al., 1967), the new behavior must occur before it is reinforced.

The second type of learning, usually called “modeling,” has been studied especially by Bandura (e.g. 1969) and his associates. A rapidly increasing number of studies show that men learn by observing both the activities of others (the models) and the consequences of these actions. Later on, in similar circumstances, an individual is likely to “try out” the behavior he has observed *if* it was reinforced for the model (hence Bandura, 1969:30–2, speaks of “vicarious reinforcement”). However, whether the individual will continue to behave thus depends on whether or not he is rewarded. Modeling, in short, informs a person of a new activity and its likely consequences, but whether it becomes part of his repertoire depends on his *own* future experiences. If the new activity is not reinforced or if it is punished when he performs it, he is not likely to repeat it. In addition, modeling provides information of how *not* to behave (through

the observation of punishment), definitions of the "right" circumstances for action, and the rewards one might expect on the basis of what has happened to other people (Bandura, 1969:118-216).² The latter provides a basis for relative deprivation and is an important ingredient of the sociological problem of "distributive justice." According to Bandura, the observation of other people's outcomes helps one define the nature and effectiveness of directly experienced reinforcers by providing a standard for evaluating the consequences of one's behavior as beneficial, equitable, or unfair. Hence the same consequence may be experienced as a reward or a punishment (and affect behavior accordingly), depending on whether comparable actions by others are treated more or less favorably.

Evidence provided by studies of this second type of learning may be summarized in the form of our second axiom:

2. If an activity is learned through modeling, then the observer is likely to repeat it in similar circumstances (provided the model's behavior was reinforced).

The Deprivation Principle

When physiological and cultural requirements are not met, a person is said to be deprived, and when these requirements have been met, a person is satiated. Both state variables are matters of degree, change over time, and vary even among individuals of the same culture.

There are two major types of deprivation, not always easily distinguished. The first type, physiological or primary, derives from human physiological requirements like food. There are relatively few of these deprivations, they can change quite rapidly and be manipulated with relative ease. The second type, cultural or secondary, is learned, varies from one group and age to another, and

would include such phenomena as the lack of fashionable clothing, prestige, or popularity. The variety of these deprivations is wide indeed, they may change rapidly or slowly, and they are often difficult to define, measure, and reduce. Most animal studies involve primary deprivations like hunger; whereas, human beings are affected by a great variety of amorphous secondary deprivations. Important complexities are thereby introduced into any analysis of human social behavior (e.g. Bandura, 1969: 225-9, 261-82).

For example, physiological deprivations are limited, specific, and easily reduced. Thus, when reinforcers are viewed as means of reducing deprivations, it follows that the types, scope, and opportunity of reinforcement in this area will be relatively circumscribed and temporary, for every presentation of a reinforcer reduces the deprivation and makes the next reinforcer "less valuable," to follow Homans. Cultural deprivations, however, are less limited and specific, and therefore much more difficult to reduce: monetary and status deprivations, for example, seem almost impossible to eliminate. Such associated reinforcers as money and deferential behavior from others appear to have no lasting effect on the degree of deprivation, and the next reinforcer is *not* likely to decrease in value. To the extent that social life involves cultural deprivations and reinforcers, therefore, we should expect considerable stability in the "value" of most rewards.

Since reinforcers are able to affect behavior through their capacity to reduce deprivations, the absence of deprivations in effect eliminates reinforcers as well. Hence it would be difficult to establish new behaviors and to maintain existing activities; furthermore, an individual would be quite independent of his social environment and its reactions to him. At the very least, then, the schedules and types of reinforcement effective at one degree of deprivation cannot be expected to be effective at another magnitude.

Since deprivations take two major forms, and since the cultural form has great variety and relative stability, the casual and indiscriminate use of this concept—and therefore of "reinforcer" as well—should be avoided

² While it is generally correct to say that the effect of modeling is identical to that of learning by direct experience, one must recognize additional variables at work. An observer's respect for the model, age differences, whether the model is a stranger or a friend, and perceived similarities between model and observer, all affect the probability that an individual will repeat what he observes (Bandura, 1969:136-8). Overall, however, the consequences of the observed behavior and one's own later experiences appear to be the most significant determinants of learning through modeling.

in the analysis of complex social behavior. Hence, while it is useful to generalize, any generalization should be considered to hold only for the specific type deprivation, its rate of reduction, and its malleability in a particular situation. We summarize existing empirical evidence in the following axiom:

3. If a person is deprived of some items, these items tend to be reinforcers; if a person is not deprived of some items, they do not tend to be reinforcers.

The Extinction Principle

Not all consequences of behavior are reinforcers. Another large class of consequences, called "aversive stimuli," refers to things, events, symbols, or activities of others which increase a person's deprivation, are in some way painful or unpleasant or reduce the probability that the preceding activity will be repeated. As with reinforcers, aversive stimuli should be viewed transituationally; what a person considers aversive at one time and place, he is likely to consider aversive at other times and places as well. However, the circumstances must be similar; a friend's frown is usually more powerful than a stranger's. Finally, some behavior patterns may be intrinsically aversive, but these are as subject to question as intrinsically reinforcing activities. Again, we should distinguish between a relatively small number of physiologically based aversive stimuli, subject to relatively easy manipulation and rapid alteration, and a wide range of cultural (i.e. learned) aversive stimuli (Bandura, 1969:293-354).

The procedure of "punishment" refers to the presentation of an aversive stimulus, the withholding or withdrawal of a reinforcer, or the presentation of a reinforcer much smaller than expected. The extreme form of the latter is, of course, not being rewarded when one expects to be. In general, punishment reduces the probability of behavior repetition, and if repeated, eventually results in the activity's extinction. Punishment is often associated with emotional reactions and other behaviors which inhibit the learning of alternatives, hence it is not as effective a method as reinforcement for establishing or maintaining behavior. The existence of spontaneous recovery, moreover, often restricts effectiveness to the short term.

While the characteristics, operations, and limitations of reinforcers have been widely studied, those of punishment have not. As a result, present knowledge is incomplete and tentative. Axiom 4 summarizes the available empirical evidence (Bandura, 1969: 355-423):

4. If an activity is not followed by a reinforcer, it is not likely to recur in similar circumstances.

We now turn to a more formal statement of the model's axioms and theorems.

THE FORMAL MODEL

Axioms of a deductive model are higher-level propositions which explain lower-level theorems, in the sense that the latter are derived from axioms by rules of inference (Hempel, 1965). Since deriving theorems from axioms requires that the latter be stated in formal terms, we will use a simple calculus flexible enough to incorporate most features of quantification theory.³

The Calculus

The calculus of formal systems consists of four parts: (a) vocabulary, (b) formation rules, (c) rules of inference, and (d) axioms (Woodger, 1939). We will present a brief discussion of these parts, for the logic of calculi is not our major concern (for descriptions, see Quine, 1959; Suppes, 1957; Copi, 1967).

³ Our analysis could be performed in the logic of quantification. However, to go from the calculus of this paper to quantification logic, we would need to introduce new predicate constants and variables as well as rules dealing with quantifiers. Hence it seems that quantification logic, though useful in revealing the structure of statements, is inefficient and cumbersome for our analysis. The inefficiency of quantification logic can be illustrated by taking the simple subject-predicate sentence: "The activity is reinforced." In quantification theory, the statement yields: $(\exists x) (Ax \supset Rx)$; or in words "For every x , if x is an activity, then x is reinforced." In our calculus, the same sentence yields: Ra , where the lower case "a" refers to any activity, and the upper case "R" refers to the predicate "is reinforced." Our model is relatively simple and permits intuitive reasoning. Generally, the letters used as predicates in our calculus are self-evident; hence understanding the material discussed in the previous section is sufficient for understanding the structure of the calculus.

Vocabulary. The vocabulary of a formal system consists of the symbols used in writing the statements of the system. The symbols and their meanings for our formal system are as follows:

Logical Constants: "-" not, "&" and, "→"
if . . . , then, "↔" logically equivalent
Punctuation: "<" ">" and "<" ">"

Vocabulary for the Formal Model:

Symbols	Interpretation
S	-discrete, identifiable stimulus field
D	-state of deprivation
L	-observational learning
a	-any activity (R)*
b	-any new activity
O	-observation
AD	-activity to alleviate deprivation
R	-reinforcing stimulus (S')
t	-temporal location

Examples and intuitive meanings of these terms—as heuristic devices—are given in Table 1.

Formation Rules. The formation rules of a formal system indicate how to construct meaningful expressions of the object language or the language being described. While the rules are generally stated in terms of "well formed formulas," we will depart from

* (The symbols in parentheses refer to the common notation used in the psychological literature)

Table 1. Examples of the Foregoing Symbols and Their Intuitive Meanings

Example*	Intuitive Meaning
S_1	Person in a given stimulus context
a_1	Person's activity
Ra	Activity is reinforced
Rb	Novel activity is reinforced
D_1	Person is deprived
AD_a	Activity to reduce specific deprivation
Lb_1	New behavior learned through nonreinforced observation
Rb_2 O_{a_1}	Person observes model's behavior which is reinforced
$t+1$	The next temporal location

*The subscripts assigned to the symbols refer to persons.

this format and use, instead, a paradigm which consists of a sequence of five elements.

Let: $\langle e_i \rangle$ = a set of elements

where $i = e_1 \dots e_k$, and $k = 5$

$\langle So \rangle$ = any sequence of five elements

Then: $\langle So \rangle = \langle e_1, e_2, e_3, e_4, e_5 \rangle$

Each element is assigned a specific location, as shown. The elements of the paradigm in turn refer to the dimensions incorporated by the behavioral model, as follows:

Elements	Dimension
e_1	External state (i.e., discrete context of a stimulus field)
e_2	Internal state (e.g., processes such as thinking or feeling, and conditions of being thirsty or tired)
e_3	Overt behavior (i.e., activity which can be measured)
e_4	Function element which maps an activity (i.e., reinforcement)
e_5	Temporal location

The activity symbol Ada , for example, should be understood as being in location e_3 , or in the "activity" dimension. Great care must be taken to ensure that the elements are placed in the correct dimensions; where the sequence is not filled with all five elements, the \emptyset notation will be used to preclude any ambiguity which might arise. As a general rule, a paradigm must contain a sequence of one or more elements to be considered meaningful in our calculus. The symbolic expression for this rule is: $\langle So \rangle$ with i elements, where i may range from 1 to 5. For example, the statement: "If a person's behavior is reinforced, then the behavior is likely to occur again," in our calculus yields:

$$\langle S_1, \emptyset, a_1, Ra, t \rangle \longrightarrow \langle a_1, t+1 \rangle$$

Where the first element S_1 is the situation in which a person's activity is reinforced, the second element or an internal state is absent as indicated by \emptyset , the third element is the activity of a person, a_1 , the fourth element is the mapping of the activity or Ra , and the fifth is the expression of temporal location. In the object language (i.e., the language being described), it is understood that the behavior occurs in a given situation; this is made explicit in the metalanguage (i.e., the language of the description itself). The $t+1$

in the "then" clause denotes the temporal sequence of the activity as implied by the original statement. In the example, the \emptyset notation is not used in the "then" clause since the symbols' references are quite clear.

Axioms. The general principles adduced above are the content of our model and may therefore be taken as its axioms.

- I. **Reinforcement Principle.** If an activity is followed by a reinforcer, it is likely to recur in similar circumstances.
 $A1: \langle s_1, \emptyset, a_1, Ra, t \rangle \rightarrow \langle s_1, t+1 \rangle$
- II. **Modeling Principle.** If an activity is learned through modeling, the observer is likely to repeat it in similar circumstances (provided the model's behavior was reinforced).
 $A2: \langle s_1, Lb_1, \emptyset, \emptyset, t+1 \rangle \rightarrow \langle b_1, t+1 \rangle$
- III. **Deprivation-Satiation Principle.** If a person is deprived of some items, these items tend to be reinforcers; if a person is not deprived of some items, they do not tend to be reinforcers.
 $A3.1: \langle s_1, D_1, Ade, \emptyset, t \rangle \rightarrow \langle Ra, t \rangle$
 $A3.2: \langle s_1, \bar{D}_1, Ade, \emptyset, t \rangle \rightarrow \langle \bar{Ra}, t \rangle$
- IV. **Extinction Principle.** If an activity is not followed by a reinforcer, it is not likely to occur in similar circumstances.
 $A4: \langle s_1, \emptyset, a_1, \bar{Ra}, t \rangle \rightarrow \langle \bar{s}_1, t+1 \rangle$

Definitions*

- Def. 1: $\langle s_1, \emptyset, Oa_1, \emptyset, t \rangle \leftrightarrow \langle s_1, Lb, \emptyset, \emptyset, t \rangle$
 Def. 2: $\langle s_1, Lb_1, \emptyset, \emptyset, t \rangle \leftrightarrow \langle \emptyset, Lb_1, \emptyset, \emptyset, t+1 \rangle$

Rules of Inference. For the most part, standard rules of inference will be used in the proofs.⁴ However, due to the nature of

* Vicarious learning = the acquisition of behavior through observing a model's activity and its consequences.

⁴ In general, we shall conform to the conventional format used in deriving theorems from axioms in the form: $P_1, P_2, \dots, P_n/C$ (Copi, 1967). In this format, the formulas P_1, P_2, \dots, P_n serve as premises and the formula C as the conclusion. Its validity is determined by the *form* of the argument: If P_1, P_2, \dots, P_n are true, then so is C ; i.e., the conclusion follows logically from the premises. The demonstration constitutes a line

S_1
 S_2
 \vdots
 S_k

where $\langle S_1 \rangle$ is either a premise, or follows from previous $\langle S_i \rangle$'s by a rule of inference, and $\langle S_k \rangle$ is C . Usually, the proof entails taking one or more $\langle S_i \rangle$'s as premises and obtaining $\langle S_k \rangle$ as the last entry of the proof. Theorems are the $\langle S_k \rangle$'s in any proof. The reason for each step in the derivation is generally written with reference to previous steps and to a rule of inference. This makes explicit

our formation rules additional rules are introduced to permit inferences required by the calculus.⁵

Theorems. Given the calculus of the formal system, it is a simple task to determine precisely which axioms are required for deriving a given theorem; in essence, the axioms contain all the model's theorems. Since we wish our model to express the knowledge generated by recent human learning studies, we shall consider it "complete" when significant behavioral propositions can be derived from its axioms. For this reason only a limited number of theorems will be derived to demonstrate its utility.

Continuing Reinforcement Theorem. A theorem derived from Axioms 1 and 3.1 sets the condition under which some reinforcers are likely to remain effective regardless of how often they occur. From these axioms we derive an important, if not basic, theorem for the maintenance of behavior (the continuing reinforcement theorem): If a person is deprived of physical or cultural items, he is likely to engage in those activities which will reduce these deprivations.

the deductive steps which are often left implicit in less rigorous derivations.

⁵ R1: Absorption: Given the conditional $\langle So \rangle \rightarrow \langle S_1 \rangle$, if one or more elements are absent in $\langle So \rangle$, then any of the elementless dimensions may be filled with an appropriate $\langle e_1 \rangle$.

Example:

1. $\langle S_1, \emptyset, a_1, Ra, t \rangle \rightarrow \langle a_1, t+1 \rangle$
- \therefore 2. $\langle S_1, I, a_1, Ra, t \rangle \rightarrow \langle a_1, t+1 \rangle$ Absorp.

Where "I" stands for any internal state.

Line 2 follows by *absorption*.

R2: Assimilation: Two sequences $\langle So \rangle$ and $\langle S_1 \rangle$ which are lines in the proof may be rewritten as one sequence as long as none of the elements conflict. Two elements conflict if and only if:

- (a) they are appropriate to the same dimension;
- (b) they are different.

Example: 1. $\langle S_1, \emptyset, a_1, \emptyset, t \rangle$
 2. $\langle S_1, \emptyset, a_1, Ra, t \rangle$
 \therefore 3. $\langle S_1, \emptyset, a_1, Ra, t \rangle$ Assim.

Line 3 follows by *assimilation*.

R3: Disintegration: A sequence may be rewritten as the sum of two or more sequences, if and only if, they can be assimilated to form the original sequence.

R4: Substitution: $\langle So \leftrightarrow S_1 \rangle$
 $\langle So \rightarrow S_2 \rangle$
 $\therefore \langle S_1 \rightarrow S_2 \rangle$

THEOREM 1: $\langle s_1, D_1, A_n, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$

The proof of this theorem can be shown as follows:

PROOF:

- | | | | |
|-----|-----|---|-------------|
| 1. | 1. | $\langle s_1, S, A_1, M, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | A1 |
| 2. | 2. | $\langle s_1, D_1, A_n, S, t \rangle \longrightarrow \langle s_1, t \rangle$ | A3.1 |
| 3. | 3. | $\langle s_1, D_1, A_n, S, t \rangle \in \langle s_1, t+1 \rangle$ | assum |
| 4. | 4. | $\langle s_1, D_1, A_n, S, t \rangle$ | 3, Simp. |
| 5. | 5. | $\langle s_1, t \rangle$ | 2,4, H.P. |
| 6. | 6. | $\langle s_1, D_1, A_n, S, t \rangle$ | 4,5, Assim. |
| 7. | 7. | $\langle s_1, t+1 \rangle$ | 3, Simp. |
| 8. | 8. | $\langle s_1, D_1, A_n, S, t \rangle \in \langle s_1, t+1 \rangle$ | 6,7, Conj. |
| 9. | 9. | $\langle s_1, t+1 \rangle$ | 1,8, H.P. |
| 10. | 10. | $\langle s_1, D_1, A_n, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | 3-9, C.P. |

Note: The bent arrow notation defines the scope of the assumption under the rule of conditional proof (See Copi, 1967:86).

Satiation Theorem. Axiom 3.2 asserts that in the absence of deprivations the activities are not likely to be reinforced (i.e., the items will not become reinforcers). Axiom 4 tells us that if this is the case, then it will be quite difficult to maintain old activities or establish new ones. The theorem which follows from Axioms 3.2 and 4 can be called the "Satiation theorem," for it asserts that if a person is not deprived of physical or cultural items, he is not likely to engage in activities to obtain the items. In this context, the "items" may be defined as "less valuable," and the person is said to be in "state of satiation."

11. THEOREM 2: $\langle s_1, \bar{D}_1, A_n, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$

The proof of this theorem follows the same pattern sketched above. No proof will be given for this theorem or subsequent theorems whose proofs call for steps similar to those already given.

Modeling and Vicarious Reinforcement Theorem. According to modeling research, a person's readiness to perform an activity depends on whether or not the model he has observed engaging in the activity is re-

warded. We restate the central idea of Bandura's theory formally:

- | | | | |
|-----|-------------|---|-----------|
| 13. | 1. | $\langle s_1, S, O_{12}, S, t \rangle \longleftrightarrow \langle s_1, I_{12}, S, S, t \rangle$ | Def. 1 |
| 13. | 2. | $\langle s_1, I_{12}, S, S, t \rangle \longleftrightarrow \langle s_1, I_{12}, S, S, t+1 \rangle$ | Def. 2 |
| 14. | (Def. 3) 3. | $\langle s_1, S, O_{12}, S, t \rangle \longleftrightarrow \langle s_1, I_{12}, S, S, t+1 \rangle$ | 1,3, M.P. |

Bandura shows that one must distinguish between learning and performance, and that a response must be learned before it can be imitated (1969:118-43). Axiom 2 tells us that if an activity is learned (is part of the observer's repertoire), then the observer is likely to "try out" the novel response. From the definition of learning (Def. 2) and Axiom 2, the "learning theorem" can be derived: If a person has learned an activity in the past, he is likely to engage in similar activity in the next similar circumstances.

THEOREM 3: $\langle s_1, I_{12}, S, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$

PROOF:

- | | | | |
|-----|------|--|------------|
| 15. | 1. | $\langle s_1, I_{12}, S, S, t \rangle \longleftrightarrow \langle s_1, I_{12}, S, S, t+1 \rangle$ | Def. 2 |
| 16. | 2. | $\langle s_1, I_{12}, S, S, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | A2 |
| 17. | 3. | $\langle s_1, I_{12}, S, S, t+1 \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | 2, Disint. |
| 18. | 3:4. | $\langle s_1, I_{12}, S, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | 1,3, Sub. |

If our model is to contribute to theoretical developments in the social sciences, it should include the "vicarious reinforcement principle" because this is one of the major characteristics of human behavior and represents a significant difference between Bandura's and Skinner's approaches to learning. We can derive Bandura's principle as a theorem of our model by combining the definition of learning (Def. 3) and Axiom 2. From these it follows that if a person observes that a model's behavior is reinforced, then he is likely to engage in similar activity in the next similar circumstances.

THEOREM 4: $\langle s_1, S, O_{12}, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$

PROOF:

- | | | | |
|-----|------|--|------------|
| 19. | 1. | $\langle s_1, I_{12}, S, S, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | A2 |
| 20. | 2. | $\langle s_1, S, O_{12}, S, t \rangle \longleftrightarrow \langle s_1, I_{12}, S, S, t+1 \rangle$ | Def. 3 |
| 21. | 3. | $\langle s_1, I_{12}, S, S, t+1 \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | 1, Disint. |
| 22. | 3:4. | $\langle s_1, S, O_{12}, S, t \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+1 \rangle$ | 2,3, Sub. |

Maintenance and Extinction Theorems. Unlike Miller and Dollard's theory of social imitation (1941) which views reinforcement as the major vehicle for learning, Bandura views reinforcement as a facilitator of performance and a means of sustaining behaviors learned under conditions of nonreinforced observation. From Axiom 1 and

* Abbreviations used in the proofs:

- | | |
|---------|--------------------------|
| A | = Axiom |
| T | = Theorem |
| Simp. | = Simplification |
| Conj. | = Conjunction |
| M.P. | = Modus Ponens |
| C.P. | = Conditional Proof |
| H.S. | = Hypothetical Syllogism |
| Def. | = Definition |
| Sub. | = Substitution |
| Assim. | = Assimilation |
| Disint. | = Disintegration |
| Export. | = Exportation |
| Absorp. | = Absorption |

Theorem 3, it follows that if the behavior an observer adopts from a model is reinforced when he himself performs it, then the activity is likely to occur again in the next similar circumstances.

$$\text{THEOREM 3: } \langle s_1, Lb_1, b_1, Rb, v \rangle \longrightarrow [\langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle]$$

PROOF:

22. 1. $\langle s_1, R, b_1, Rb, v \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle$ A1
 24. 2. $\langle s_1, Lb_1, R, b_1, v \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle$ T2
 25. 3. $\langle s_1, Lb_1, b_1, Rb, v \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle$ 1, 2, Assump.
 26. 25, 4. $\langle s_1, Lb_1, b_1, Rb, v \rangle \longrightarrow [\langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle]$ 3, Export.

Finally, extinction can occur if the behavior an observer adopts from a model is not reinforced when he himself exhibits it. From Axiom 4, we derive our sixth theorem:

$$\text{THEOREM 6: } \langle s_1, Lb_1, b_1, Rb, v \rangle \longrightarrow [\langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle]$$

PROOF:

27. 1. $\langle s_1, R, b_1, Rb, v \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle$ A4
 28. 2. $\langle s_1, Lb_1, b_1, Rb, v \rangle \in \langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle$ 1, Absorp.
 29. 28, 3. $\langle s_1, Lb_1, b_1, Rb, v \rangle \longrightarrow [\langle s_1, t+1 \rangle \longrightarrow \langle s_1, t+2 \rangle]$ 2, Export.

Summary. In this section we have described the behavioral model in terms of a calculus and have shown (1) how general principles of learning might be rendered into a strict formal system, and (2) how various theorems can be derived from these axioms. In particular, we have shown that theorems concerning the establishment, maintenance, and extinction of behavior can be derived from the five axioms of the model. The general principles adduced in this paper do not exhaust the principles of social learning and behavior; and the model is too simple to represent the complexities involved in all social behavior.⁷ Yet the model contains

⁷ It is, of course, possible to begin with more complex axioms than we have in this paper. As a case in point, Herrnstein (1970) has recently demonstrated that behavior in choice situations can be described by a "matching law," which provides an excellent basis for accurate prediction in both "single behavior" and "multiple schedule" situations. In a "two activity" situation the formula is: $B1/B2 = V1/V2$; it indicates that the ratio of two rates of activities is equal to the ratio of the values of events which follow them, where "value" may be taken to be rate, amount, or immediacy of reinforcement. The validity of this law for behavior in small groups is currently being tested (Conger, forthcoming). In a paper now in preparation we will demonstrate how the present axiom system may be quantified and used to apply such a principle as the matching law to social behavior (for additional discussions of the matching law, see Rachlin, 1971 and Killeen, 1972).

axioms that specifically relate to the four areas any sociologically relevant model of man must consider: The establishment of behavior is treated by Axioms 1, 2 and 3.1., the maintenance of behavior by Axioms 1 and 3.1, and the extinction of behavior by Axioms 3.2 and 4. Behavior modification involves all five axioms, for altering activities involves little more than the extinction of some old behaviors and the establishment of new ones. Axioms 1, 3 and 4 may therefore be viewed as the major components of a behavioral model of man since they are required in the analysis of all four areas. Figure 1 graphically portrays the structure of our behavioral model of man.

SOME SOCIOLOGICAL IMPLICATIONS

The behavioral model we have described is not new—the basic ideas of reward and punishment are common in sociology and indeed are part of the conventional wisdom. But as long as one knows only that men learn or that the consequences of behavior affect future repetitions, one will have difficulty incorporating these ideas into methodological and theoretical efforts. Our behavioral model enables us to analyze the elements of social phenomena and processes, to specify the relations among elements, and to indicate the factors responsible for changes in these elements. It is only upon the formulation of a precise model that behavioral principles can be applied to sociology in a rigorous, consistent, and fruitful manner.

This model of man adds two elements to Homans'. First, we recognize that behavior is often learned and modified by observation and vicarious reinforcement. Second, in a serial presentation, later reinforcers are not always less valuable than earlier ones. Changes in value depend on modeling and the nature of deprivations, and these vary in stability. We have also found it unnecessary to establish an axiom concerning anger. It is true that sudden schedule changes often arouse anger, but the sociological significance of this fact is not clear.

The postulate that most activities (R) are preceded by one or more discriminative stimuli (S^D) and eventually are followed by one or more contingent stimuli (S^r and S^a),

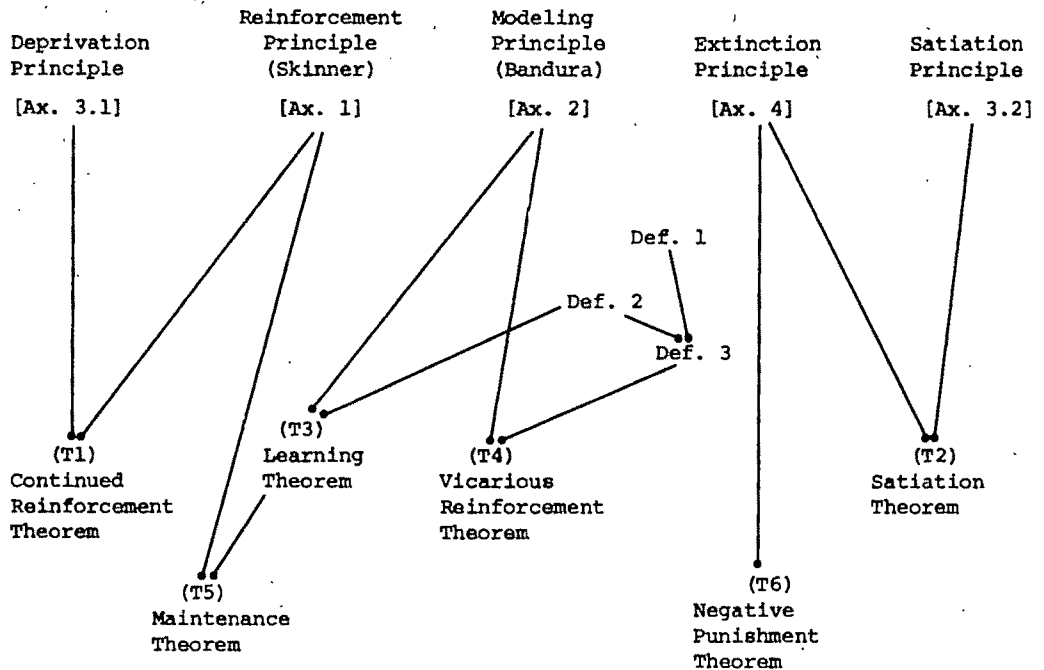


FIGURE 1. THE RELATION OF AXIOMS AND THEOREMS IN THE BEHAVIORAL MODEL *

*A line from one number to another ending with a dot indicates that the former entered into the derivation of the latter.

leads us to look for the S^D and S^r of those persistent and ephemeral activities which comprise the sociological subject matter. We can focus on group activities and categories of individuals, like women, minority members, or graduate students, to discover and analyze the operating S^D and S^r and their behavioral implications. While the structures and operations of the social contexts of such individuals vary, it will be fruitful to generalize about both the common discriminative and contingent stimuli as well as the behavior patterns which are thereby shaped and maintained.

In addition, our model of man sensitizes us to look for the social factors involved in the absence or low frequency of relevant S^D and S^r , the presence of aversive consequences, and the absence of learning opportunities. In this area the *individual's* perspective is especially important, i.e., what he thinks the state variables and behavioral consequences are. Our model treats a person's perceived context, even if he has difficulty describing it, and not an observer's view of it. Furthermore, we must not forget that there is a *reciprocal* relationship between individual and context. While we have emphasized the environment's influence on the individual, this is due only to our

focus on models of man; a person's behavior also affects his social environment, but the specifics of the reactions here depend on social and cultural factors, rather than on psychological principles. Suffice it to say that both experimental studies and careful naturalistic observations "demonstrate that persons, far from being ruled by an imposing environment, play an active role in constructing their own reinforcement contingencies through their characteristic modes of response . . . [thus] each person partly creates his own environment" (Bandura, 1969:46-7).

Finally, we can conceptualize complex social structures as intricate arrangements of large numbers of $S^D \rightarrow R \rightarrow S^r$ triads. Such configurations of activities—or "patterns"—have been assumed and described by many sociologists, but heretofore they have been little more than figures of speech. Our model of man makes it possible to indicate how several triads combine to produce "patterns," and how the resulting structure of discriminative and contingent stimuli affects behavior. Hamblin's (1971) recent study of the inconsistent and aversive operation of many schools and Kunkel's (1970) work on social change provide good examples.

In summary, our model enables us to

analyze social behavior by asking: "What are the S^D for this activity?" and "what are the S^r (or S^a) for that behavior?" The answers, together with our model's propositions, allow us to specify the probabilities of replication and modification, and to indicate the likely repercussions on other activities. We do not mean to suggest that all of sociology will benefit from using a formal behavioral model, or that psychological principles are the only ones sociologists will find useful. The old question: "What is the role of a model of man, or of psychological principles, in sociology?" has not yet been answered; we believe it cannot be without a set of precise and formal propositions whose role in the analysis of specific sociological topics can be described in considerable detail.

REFERENCES

- Baer, Donald M., Robert F. Peterson and James A. Sherman
1967 "The development of imitation by reinforcing behavioral similarity to a model." *Journal of the Experimental Analysis of Behavior* 10(July):405-16.
- Bandura, Albert
1969 *Principles of Behavior Modification*. New York: Holt, Rinehart and Winston.
- Baron, Robert A. and Robert M. Liebert (eds.)
1971 *Human Social Behavior: A Contemporary View of Experimental Research*. Homewood: Dorsey.
- Blau, Peter M.
1965 *Exchange and Power in Social Life*. New York: Wiley.
- Burgess, Robert L. and Ronald L. Akers
1967 "Are operant principles tautological?" *The Psychological Record* 16(Summer):305-12.
- Conger, Rand
Forth- "The effects of positive feedback on directing tion and amount of verbalization in a social setting." *Pacific Sociological Review*.
- Copi, Irving M.
1967 *Symbolic Logic*. Third ed. New York: The Macmillan Co.
- Emerson, Richard M.
1969 "Operant psychology and exchange theory." Pp. 379-405 in Robert L. Burgess and Don Bushell, Jr. (eds.), *Behavioral Sociology*. New York: Columbia University Press.
- 1972 "Exchange theory." Pp. 38-87 in Joseph Berger, Morris Zelditch, Jr., and Bo Anderson (eds.), *Sociological Theories in Progress*, Vol. II. New York: Houghton Mifflin.
- Hall, Calvin S. and Gardner Lindzey
1970 *Theories of Personality*. Rev. ed. New York: Wiley.
- Hamblin, Robert L., David Buckholdt, Daniel Ferritor, Martin Kozloff and Lois Blackwell
1971 *The Humanization Processes: A Social, Behavioral Analysis of Children's Problems*. New York: John Wiley & Sons.
- Hempel, Carl G.
1965 *Aspects of Scientific Explanation*. New York: The Free Press.
- Herrnstein, Richard J.
1970 "On the law of effect." *Journal of the Experimental Analysis of Behavior* 13 (March):243-66.
- Hilgard, Ernest R.
1953 *Theories of Learning*. New York: Appleton-Century-Crofts.
- Hilgard, Ernest R. and Gordon H. Bower
1966 *Theories of Learning*. Third Edition. New York: Appleton-Century-Crofts.
- Homans, George C.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace.
- 1964 "Contemporary theory in sociology." Pp. 951-77 in Robert E. L. Faris (ed.), *Handbook of Modern Sociology*. Chicago: Rand McNally.
- Hull, Clark L.
1951 *Essentials of Behavior*. New Haven: Yale University Press.
- Killeen, Peter
1972 "The matching law." *Journal of the Experimental Analysis of Behavior* 17 (November):489-95.
- Krasner, Leonard and Leonard P. Ullmann (eds.)
1965 *Research in Behavior Modification*. New York: Holt, Rinehart & Winston.
- Kunkel, John H.
1970 *Society and Economic Growth: A Behavioral Perspective of Social Change*. New York: Oxford University Press.
- Lenski, Gerhard D.
1966 *Power and Privilege: A Theory of Stratification*. New York: McGraw-Hill.
- Maris, Ronald
1970 "The logical adequacy of Homans' social theory." *American Sociological Review* 35 (December):1069-81.
- McGinnies, Elliott
1970 *Social Behavior: A Functional Analysis*. Boston: Houghton Mifflin.
- Merton, Robert K.
1957 *Social Theory and Social Structure*. Glencoe: Free Press.
- Miller, Neil E. and John Dollard
1941 *Social Learning and Imitation*. New Haven: Yale University Press.
- Parsons, Talcott
1951 *The Social System*. Glencoe: Free Press.
- Premack, David
1965 "Reinforcement theory." Pp. 123-80 in David Levine (ed.), *Nebraska Symposium on Motivation: 1965*. Lincoln: University of Nebraska Press.

- Quine, W. V. O.
1959 *Methods of Logic*. New York: Holt, Rinehart and Winston.
- Rachlin, H. C.
1971 "On the tautology of the matching law." *Journal of the Experimental Analysis of Behavior* 15(March):249-51.
- Rotter, Julian B.
1954 *Social Learning and Clinical Psychology*. Englewood Cliffs: Prentice-Hall.
- Schnore, Leo F.
1958 "Social morphology and human ecology." *American Journal of Sociology* 63(May): 620-34.
- Simon, Herbert A.
1957 *Models of Man*. New York: Wiley.
- Skinner, B. F.
1953 *Science and Human Behavior*. New York: Macmillan.
1961 *Cumulative Record*. New York: Appleton-Century-Crofts.
1971 *Beyond Freedom and Dignity*. New York: Knopf.
- Suppes, Patrick C.
1957 *Introduction to Logic*. Princeton: D. Van Nostrand.
- Udy, Stanley H.
1965 "The comparative analysis of organization." Pp. 678-709 in James G. March (ed.), *Handbook of Organizations*. Chicago: Rand McNally.
- Ullman, Leonard P. and Leonard Krasner (eds.)
1965 *Case Studies in Behavior Modification*. New York: Holt, Rinehart & Winston.
- Warner, W. Lloyd, Marchia Meeker and Kenneth Eells
1960 *Social Class in America*. New York: Harper Row.
- Weber, Max
1947 *The Theory of Social and Economic Organization*. New York: Oxford University Press.
- Woodger, Joseph
1939 *The Technique of Theory Construction*. *International Encyclopedia of Unified Science*, Vol. 2, No. 5.

SOME DEVELOPMENTAL INTERPERSONAL DYNAMICS THROUGH CHILDHOOD *

H. W. SMITH

University of Missouri, St. Louis

American Sociological Review 1973, Vol. 38 (October):543-552

Methods for studying small groups are applied to theoretical questions derived from social psychology. Groups of five persons, of like sex and age, ranging from age five to twenty were studied over four separate sessions. The focus was on age-related developments in interpersonal dynamics, such as number of interaction participants, clique formation, interaction forms, interaction sequence profiles, and within-session dynamics. Piagetian, social learning and symbolic interaction paradigms are used to interpret the findings. The evidence argues that negative and positive interpersonal actions differ in their effect on social adaptation, status and power differentiation increases with age, and that developmental stages are more complex than the Meadian and Piagetian traditions have suggested. Furthermore, the data suggest strong pressures towards homeostasis within interpersonal relationships, regardless of age.

LITTLE empirical work has been done on the development of interpersonal dynamics through the life cycle. Indeed, a search of the literature turned up only one article of any scope (Smith, 1960). The descriptively enlightening but theoretically insufficient propositions laid down by Mead

(Reck, 1964), Piaget (1967) and their followers (cf. Flavell, 1968; McCall and Simons, 1966) on interpersonal dynamics through the life cycle invite clarification. For example, if developments within groups are observed across age groups, will the same intra-age dynamics be observed, whatever the age of the group members? There is theoretical reason to believe that intra-age dynamics differ for adults as compared to child groups. Bales (1950:33) says that "effective decision-making is basically a continuous process

* This paper utilized a portion of the data reported in the author's dissertation. See Herman W. Smith, "Some Developmental Antecedents of Adult Interpersonal Behavior," unpublished Ph.D. dissertation, Northwestern University, 1970.

of building and maintaining a structure of cultural objects. . . ." Certainly we assume that children's groups have less cultural base than other groups; and, therefore, we could assume that interaction processes in them will be more random. Thus, the age of the group members could be related to intra-age dynamics, since the socialized adult, unlike the child, will have learned normative strictures on possible interaction. These strictures, in turn, would grant a stability to the adult interpersonal structure not found among children. The problem of the relationship between inter-age and intra-age development then becomes to determine the age at which adult intra-age dynamics is reached, assuming that interpersonal situations require establishing "rules of the game."

A possible objection to comparing adult with child interpersonal dynamics concerns the relativity of time for developments at each age level. Psychoanalytic and behavioral literature commonly notes that development is more rapid at the earliest ages. Hence, a group of four-year-olds observed over a month's period may not yield results comparable to a group of adults so observed.

Other interaction differences have been noted as possible sources of error in adult-child comparison studies. Piaget (1954; 1957) stressed that child interaction differs qualitatively and quantitatively from adult interaction. The qualitative difference he attributed to the egocentric nature of the child. The child, in contrast to the adult, allegedly bases his communication on his own viewpoint rather than his listener's. Each of these problems is dealt with in the next section.

METHODS

The Sample. With no precedent for ascertaining the age level cutting point for adult-child interaction patterns, a simple adult-child dichotomy cannot clearly specify the development of interpersonal responses. Since not until the age of four does a child normally participate in groups larger than two (Parten, 1933; Mussen, 1960), age four sets a reasonable minimum for the subjects. The upper age limit was twenty-one, since the literature (Hare, 1962:107-8) indicates that adult interpersonal dynamics seem stable from eighteen on. Inter-age differences

are larger at younger ages. The younger the subject, the more impressionable and less stable (Mussen, 1960). Further, changes are more rapid and pronounced earlier in life.

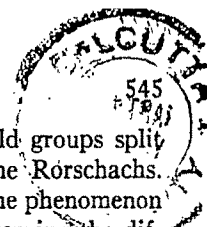
Subjects were drawn from a Northside Chicago kindergarten-elementary-high school parochial school system and Catholic college. These were stratified by median class age—five, six, eight, ten, thirteen, sixteen and twenty. Students within these classes were pooled by sex and names were then randomly drawn from each list.

Groups were comparable with respect to personal and social characteristics (white Catholic, working and lower middle class, college preparatory), degree of transiency (artificially constructed groups), group size (five), group sex composition (like-sexed),¹ and number of sessions (four one-half hour sessions). This comparability corrected for such potential internal validity problems as history, maturation, statistical regression, differential respondent selection, and experimental mortality (Campbell & Stanley, 1963).

Data Collection. For comparative purposes it is not feasible to use one category system for children and another for adults, as past studies have done. A category system is needed adequate for handling data from children and adults. Borgatta's (1962) revision of Bales' I.P.A. was used because it emphasizes *intensity* of interaction in the Bales' system.

A major objection to using adult interaction category schema with children's groups has been that since children's interaction is not completely socialized, some category representing "egocentric" speech is required (Piaget, 1967:32). In other words, socialized interaction occurs only when (a) one person's action is responded to by another; (b) each person is aware of the other and of the action in question; and (c) the action responded to is directed to or is about the person responding (Dyck, 1963:78). Interaction sequence analysis circumvents the

¹ In the data reported in this article, there were no statistically significant sex differences; hence, all male and female group data have been combined for each age level. Two of the four groups at each age level were composed of members of each sex.



need for egocentric categories because it shows that *ego's* actions modify the distribution of responses from *alter's* repertory in interaction (Raush, 1965). Thus, if sequence changes in interaction are not as apparent or don't exist for children, (i.e., are more random), then one might have independent proof of the child's egocentric speech. For egocentric, unlike socialized speech, lacks interpersonal modifiability since ironically in it, the person believes he is sharing the viewpoint of alter while in truth he remains isolated within his own (Piaget, 1967). Sequence analysis of alter's reactions to ego's actions may show that interpersonal modifiability is necessary (i.e., attempted answers to questions) to socialized verbal interaction.

The Task. Task requirements called for standardization across groups at all age levels to insure that the same basic stimulus was received. Given the wide range of ages, a general and ambiguous stimulus was required; hence, a different Rorschach card was chosen for each of the first three sessions, and two Rorschachs were used in the fourth. Groups were told to come to common agreement on what they saw in the Rorschach, and then to make a group story from that perception.

FINDINGS

In this section, the findings for group size, coalition formation, status, power, interaction profiles, interaction sequences, intra-age, and inter-age dynamics will be considered.

Physical Aspects. Physical features are the most elemental level of interpersonal behavior. Size, for example, refers to the number of participants. One must also consider complicating facets such as coalitions of participants. A comparison of children (Parten, 1933) with adults (James, 1951, 1953) suggests that children, given the choice, gravitate towards groups of two more often than do adults. Indeed, Parten (1933) shows that before age four children cannot interact in groups of five or more. Even after age four children's groups tend to break down into twosomes more often than adults'. For example, in the fourth session of this experi-

ment, the five and six year old groups split into sub-groups to analyze the Rorschachs.

Table 1 may help explain the phenomenon of cleavage formation. By observing the differences in amounts of interaction addressed to the group (Sum to 0) across age, we see that the amount increases with age. Indeed, using two-tailed tests for independent samples, from ages 6-8, 8-10, and 10-13, these increases are significant beyond the .001 level, and beyond the .05 level for other ages. This result suggests that younger children may not have a clear conception of a group beyond simple two-person relations. Thus, one would expect these groups to have a higher number of cleavages than adult groups. As further support of this line, group percentages were compared within ages by sessions (variance not shown). Variance was greater at younger ages, suggesting again that from session to session younger subjects' interaction patterns are less stable and, hence, less predictable than older subjects'. Within-session variance decreased significantly between ages 6-8 ($p < .001$) and ages 8-10 ($p < .02$) using two-tailed F-tests for independent samples.

The data also reveal status and power development across ages. Table 1 shows a gradual increase with age in differences in total initiated interaction. Thus, the two persons with highest initiated interaction initiate approximately the same amounts; the third and fourth persons initiate approximately half the amount of the first and second persons; and a fifth person initiates approximately one-fourth as much as the third and fourth persons. It seems reasonable to conclude that the inability of younger group members to deal with group size and complexity creates a much less differentiated power structure. Furthermore, evidence shows that the highest participator seems, with increasing probability, to receive more acts than he initiates, with increasing age. This indicates, following Bales arguments (1970:87-8), that there may be more behavioral acknowledgement of status and power with age.

Interaction Forms. In studying the development of types of interaction, it is useful to have diagnostic norms for judging the uniqueness of interaction rates. Although

Table 1. Who-to-Whom Interaction Matrices by Age (in percentages)*

		1	2	To: 3	4	5	Sum to I	Sum to O	Total Init.
Age 5									
From:	1	--	20.1	6.3	4.0	1.8	32.2	1.8	34.0
	2	24.4	--	2.9	2.4	1.4	31.1	1.9	33.0
	3	10.1	3.3	--	.6	.5	14.5	.8	15.3
	4	6.7	3.0	2.3	--	.8	12.8	.6	13.4
	5	3.0	.7	.2	.2	--	4.1	.2	4.3
Sum Rec.		44.2	27.1	11.7	7.2	4.5	94.7	5.3	100.0
Age 6									
From:	1	--	20.2	6.2	3.9	1.8	32.1	2.1	34.2
	2	24.3	--	3.6	2.7	1.5	32.1	1.9	34.0
	3	9.4	3.4	--	.7	.6	14.1	1.1	15.2
	4	6.5	2.9	2.4	--	.1	11.9	.9	12.8
	5	2.1	.7	.3	.1	--	3.2	.6	3.8
Sum Rec.		42.3	27.2	12.5	7.4	4.0	93.4	6.6	100.0
Age 8									
From:	1	--	16.6	6.3	3.4	1.4	27.7	6.1	33.8
	2	19.2	--	5.0	3.4	.6	28.2	4.8	33.0
	3	9.3	3.4	--	.8	.6	14.1	2.6	16.7
	4	6.3	2.9	1.2	--	.4	10.8	2.3	13.1
	5	2.2	.6	.2	.2	--	3.2	.2	3.4
Sum Rec.		37.0	23.5	12.7	7.8	3.0	84.0	16.0	100.0
Age 10									
From:	1	--	9.1	4.9	3.9	2.2	20.1	16.6	36.7
	2	16.4	--	3.5	1.7	.6	22.2	8.6	30.8
	3	8.8	2.7	--	.9	.5	12.9	3.4	16.3
	4	5.1	1.1	.7	--	.2	7.1	2.8	9.9
	5	2.9	.5	.3	.2	--	3.9	2.4	6.3
Sum Rec.		33.2	13.4	9.4	6.7	3.5	66.2	33.8	100.0
Age 13									
From:	1	--	6.2	4.3	3.8	2.0	16.3	21.2	37.5
	2	12.7	--	2.9	1.8	.6	18.0	8.3	26.3
	3	8.9	2.2	--	1.0	.7	12.8	5.9	18.7
	4	5.6	1.0	.3	--	.2	7.1	3.8	10.9
	5	3.1	.6	.6	.4	--	4.7	1.9	6.6
Sum Rec.		30.3	10.0	8.1	7.0	3.5	58.9	41.1	100.0
Age 16									
From:	1	--	4.9	3.8	3.6	1.9	14.2	32.0	46.2
	2	11.7	--	2.3	2.0	.6	16.6	6.2	22.8
	3	8.4	1.9	--	.6	.5	11.4	4.1	15.5
	4	5.6	1.4	.4	--	.1	7.5	2.4	9.9
	5	2.9	.7	.5	.3	--	4.4	1.2	5.6
Sum Rec.		28.6	8.9	7.0	6.5	3.1	54.1	45.9	100.0
Age 20									
From:	1	--	4.9	4.1	3.8	1.9	14.7	32.1	46.8
	2	11.8	--	2.3	2.0	.5	16.6	5.5	22.1
	3	8.5	1.9	--	.6	.6	11.6	3.9	15.5
	4	5.7	1.3	.4	--	.2	7.6	2.5	10.1
	5	2.9	.7	.4	.3	--	4.3	1.2	5.5
Sum Rec.		28.9	8.8	7.2	6.7	3.2	54.8	45.2	100.0

* "Sum to I" stands for sum to individuals, "Sum to O" for sum to the group, and "Sum Rec." for sum received.

Borgatta's IPS has no such device, Bales and Hare (1965) provide statistical standards for adults using Bales' IPA. These standards may be used with the IPS collapsed into the original IPA distinctions.²

Table 2 shows the interaction profiles for each age group. The use of the combined categories 01 and 02, equivalent to Bales' "shows solidarity," is close to 1.96 standard deviations below the suggested Bales-Hare diagnostic mean at age five. Category 02 seems generally to increase with age, though statistically this trend is not significant.

One can easily see the increasing use of acknowledgments (04) and signs of understanding (05) with age. Showing agreement (04 and 05 combined) is slightly less than 1.00 standard deviations below the mean for both five- and six-year olds. This may mean that showing agreement is somewhat more primitive than is showing solidarity. This seems logical since showing solidarity would seem somewhat more interactively complex and abstract.

Bales' "gives suggestion" (06 and 07 combined) also is little used at age five where the percentage is 1.96 standard deviations below the mean, while for age six it is less than 1.00 standard deviations below the mean. This is also the case for "gives opinion," which can be obtained by summing categories 08, 09, and 10.

There seems a clear-cut increase ($p < .001$ at every age gap except ages five and six) in the use of "asking for opinion" with age. Also, the percentages at ages five and six are well below 1.00 standard deviations from the norm.³ Questioning behavior approaches adult standards around age thirteen.

² Bales (1970:473) claims the distribution of percentage rates are heavily skewed to the right, and thus he has in later publications used the median rate instead of the mean. The difference is that the median rate is only *slightly lower* than the mean. This difference should not pose a problem for the present analysis.

Since Bales' diagnostic profile is based on twenty-one different adult studies, it is reasonable to assume that when children's interaction consistently deviates greatly from the mean (i.e., more than one standard deviation), we can use this as a test of significant differences between child and adult interaction. Unless otherwise specified, T-tests for independent samples under two-tailed assumptions are used as reference statistics.

³ The reader familiar with Bales' schema will note

The IPA category "shows tension" (summation of 15 and 16) percentages for age five are well over 1.96 standard deviations above the mean, while for ages six through ten they are over 1.00 standard deviations above the mean. A similar, though more striking, trend is witnessed in the IPA "shows antagonism" (summation of 17 and 18) where the percentages for all ages between five and ten are over 1.96 standard deviations above the mean, after which a sharp reduction seems to occur in the amount of antagonism shown. Both trends are significant beyond the .001 level for all age levels.

The remaining categories, "shows tension release," "gives information," and "disagreement," do not seem to develop from ages five to twenty.

Two other types of content in interaction are important in relation to development trends: positive-negative and task-expressive content. The more negative the content (the further to the right of category 13), the sharper the drop in negative content with age. Conversely, the more positive the content (the further to the left of category 4), the sharper the rise in positive content with age ($p < .001$ for every age level).

Further, by combining negative and positive reaction percentages we can compare expressive behavior to task behavior. Task behavior increases from approximately 47 percent to 68 percent of interaction between the ages of five and thirteen and then drops off slightly and stabilizes at about 58 percent ($p < .001$ for ages 5-13, $p < .01$ for age 13-16). Apparently, there is less need for expressive behavior with increased age, thus more behavior becomes task relevant.

Interaction Sequences. Several trends are evident in interaction sequences shown in Table 3. The data support our earlier contention that one learns to use positive content with age. Indeed, there is an age-related inclination for positive proactions to be followed by positive reactions ($p < .001$ for every age gap), attempted answers ($p < .001$ in every case) and questions ($p < .001$ for

that the Borgatta system lacks the two other types of questions. This is due to the fact that little theoretical use has been made of these two categories (Borgatta, 1962).

Table 2. Interaction Profiles in Percentages by Age of Groups

Age	Interaction Process Score Categories								
	01 Common Social Acknow- ledge- ment	02 Shows Soli- darity	03 Shows Tension Release, Laughs	04 Acknow- ledges, Under- stands	05 Shows Agree- ment, Concur- rence	06 Gives Procedural Suggestion	07 Sug- gests Solu- tion	08 Gives Opinion, Expresses Wish	09 Self- analysis or Self- ques- tioning
05	0.9	0.1	8.8	2.4	1.3	0.0	0.0	6.5	0.0
06	2.8	0.1	9.1	3.8	0.8	0.4	*	8.7	0.0
08	3.6	0.3	10.0	5.6	1.0	1.5	0.5	14.5	0.0
10	2.2	0.4	8.6	7.0	0.7	1.1	0.4	13.7	*
13	1.4	0.3	7.4	10.9	1.3	0.6	0.4	25.7	*
16	1.8	0.5	12.3	10.9	2.4	0.4	0.2	18.0	*
20	2.9	1.3	10.2	12.9	2.1	0.8	0.3	18.0	*

Table 2.--Continued

Ref. to Ext. Sit.-- Redir- ected Agg.	Interaction Process Score Categories								
	10 Gives Orien- tation, Infor- mation	11 Draws Atten- tion, Repeats	12 Asks for Opinion, Evalua- tion	13 Dis- agrees	14 Shows Tension, Asks for Help	15 Shows Tension Increase	16 Shows Antag- onism, Is De- manding	17 Ego Defen- siveness	18 Total**
0.8	12.9	27.4	0.5	2.8	15.0	4.0	8.4	9.0	99.8 (9384)
0.4	13.0	28.9	0.7	4.3	10.8	1.8	6.0	8.3	99.9 (11446)
0.4	4.6	30.5	1.6	3.1	8.5	1.3	6.1	7.0	100.1 (11958)
1.0	4.7	32.0	1.9	3.3	6.3	2.0	5.7	8.9	99.9 (14054)
0.2	4.0	33.8	3.5	3.4	3.9	0.9	2.1	0.1	99.9 (14862)
0.2	6.5	30.1	5.2	3.5	3.7	1.2	2.9	0.4	100.2 (15026)
0.3	6.6	25.9	5.9	2.3	6.5	1.5	1.9	0.4	99.8 (16702)

* Less than one-half of 0.1.

** Not all percentages add up to 100.0 due to rounding errors; in parenthesis below each total percentage is the total raw number of interaction scores for each age grouping.

age 13-16, 16-20). No trends appear for negative proactions followed by positive reactions.

Attempted answers are more likely to follow positive proactions, attempted answers ($p < .001$ for all cases) and questions ($p < .001$ for ages 6-13; 16-20) with age. Negative reactions to attempted answers falls off with age ($p < .001$ for ages 6-13;

16-20). All four proaction categories are more likely to be followed by a question with age, although the increases are only significant ($p < .001$) for ages 10-16 for attempted answers and for ages 13-20 for questions.

Positive proactions and attempted answers elicit no discernible differences in negative reaction with age. But questions tend to be followed increasingly by negative reactions

Table 3. Inter-age Interaction Sequence Development Expressed in Percentages

Reaction*	Age													
	5				6				8				10	
	Proaction													
	1	2	3	4	1	2	3	4	1	2	3	4	1	2
1.	3.20	7.88	**	4.22	4.10	8.33	.06	4.19	6.11	9.48	.29	4.55	4.78	9.86
2.	6.71	29.15	.02	13.13	8.29	31.53	.52	10.64	9.31	32.26	1.00	8.95	9.61	32.61
3.	**	.17	.00	.07	.09	.42	.02	.15	.42	.89	.03	.28	.45	1.08
4.	3.13	9.66	**	21.82	4.20	10.64	.11	16.70	5.02	8.72	.30	12.40	4.33	8.15

Table 3.--Continued

Age													
10		13				16				20			
Proaction													
3	4	1	2	3	4	1	2	3	4	1	2	3	4
.29	4.08	5.02	12.84	.71	2.57	8.27	14.76	1.36	3.23	10.99	13.59	1.55	3.25
1.23	8.22	12.67	44.23	2.33	5.42	15.49	31.95	2.91	5.45	13.06	29.15	3.50	5.81
.04	.28	.97	2.32	.06	.31	1.58	2.78	.21	.49	1.65	3.50	.19	.65
.33	14.65	2.38	5.37	.37	2.24	3.48	4.94	.69	2.41	3.33	5.38	.84	3.56

* 1. Pos. react.; 2. Attp. ans.; 3. Quest.; 4. Neg. react.

** Less than one-half of .01 percentage.

($p < .001$ for age 16-20); while negative proactions decrease dramatically in subsequent negative reactions with age, particularly between the tenth and thirteenth years ($p < .001$). Thus, thirteen of the possible sixteen interaction sequences vary distinctly with age.

Intra-Age Dynamics. Heinicke and Bales (1953) and others since have noted that when adult problem-solving discussion sessions are divided into three equal time periods, the predominant activity shifts from one phase to another in a manner which reflects the stages of the group's progress toward a decision. Table 4 may shed some light on the question of whether these intra-session dynamics are specific to adults or also found in children.

Positive reactions tend to increase over each of the three periods regardless of age ($p < .001$ in every case). Giving suggestions ($p = n.s$) and reacting negatively ($p < .001$ in every case except age thirteen, between

periods 2 and 3) also tend to increase slightly over periods, regardless of age. Opinions, again regardless of age, tend to increase from period 1 to 2 and decrease from period 2 to 3 ($p < .001$ in each case). Finally, giving information tends to decrease over each period for all ages ($p < .001$ except for age six, between session 2 and 3).

It may seem surprising to find phase movements within sessions at all ages approximating the adult model. The literature, in particular Bales (1950), emphasizes that children lack sufficient common cultural understandings, or developed "rules of the game," for effective group decision-making. However, although phase movement slopes are similar across ages, y-axis intercepts are *relative across* ages (i.e., while positive reactions increase from period 1 to 2 and then decrease through period 3 for all ages, the intercept base increases from 12.9 to 25.7 percent over the fifteen year span.)

Table 4. Phase Movements in Group Progress toward a Decision by Age*

Age	Positive Reactions			Suggestions			Negative Reactions			Opinions			Information		
	Period			Period			Period			Period			Period		
	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
5	12.9	13.4	14.0	0.0	0.0	0.0	37.2	39.0	39.9	6.9	7.7	7.0	43.1	39.9	39.0
6	16.5	16.7	16.6	0.0	0.3	0.6	30.0	32.0	31.7	8.9	9.3	9.0	44.6	41.8	41.8
8	18.4	20.0	22.3	1.9	2.1	2.2	25.3	26.3	26.5	13.6	15.5	15.6	40.8	36.0	33.4
10	18.6	18.7	19.5	1.3	1.5	1.7	25.9	26.3	26.9	14.0	15.2	14.9	40.1	38.3	37.1
13	20.0	19.9	22.7	1.0	1.0	1.1	8.2	11.1	11.1	25.3	26.7	25.6	45.4	41.4	39.5
16	24.9	27.4	30.2	0.5	0.6	0.7	10.5	12.9	12.8	17.2	19.8	17.5	46.9	39.3	38.8
20	25.7	26.1	31.2	1.0	1.0	1.3	11.1	13.7	13.8	17.4	19.7	17.5	44.8	39.5	36.3

* Table is expressed in percentages. Percentages do not add up exactly to 100.0% in all cases due to rounding errors. Percentages are based on each of the three phases, and not on the total sessions.

DISCUSSION

An inherent limitation of the methodological tradition followed is its difficulty in showing causal, or even correlational ties between variables. Nevertheless, the data suggest an orderly development from child into adult patterns of group dynamics. Several explanations may be given for the findings.

Interaction profile analysis of Table 2 showed that task behavior and questioning behavior stabilized between the thirteenth and sixteenth years. Negative verbal interaction appeared before the fifth year but was not used in adult fashion until between the sixth and eighth years, and positive verbal interaction was used in adult fashion around age six. Hence, socio-emotional behavior may be less complex, and more primitive in origin, than task-relevant behavior.⁴ Perhaps, the younger the child, the less neutrally he regards objects, and the more he perceives identity-oriented (Turner, 1968) objects. Because the child cannot easily differentiate between himself and his environment, he is more likely to perceive things he dislikes as a threat to his identity. As the person comes to know his identity better, he may come to feel more integrated

personally. Hence, it is conceivable that he learns to discriminate better between the massive flow of stimuli from without and thus to respond more selectively towards others.

While this symbolic interactionist perspective can be used to interpret the findings, social learning theory may be applied. For example, positive and negative actions appear behaviorally independent, as suggested by Bradburn (1969) and Bradburn and Caplowitz (1965). This view contradicts several models (Foa, 1964; Bales, 1950, 1970) which assume negative and positive actions to be behavioral opposites. In other words, their functional bases may differ. For instance, negative actions may be based more genetically than positive actions (Etkin, 1967:6); and thus, the individual may need to learn to use negative actions more discriminately. On the other hand, positive reactions may be learned more through social situations, perhaps because the individual learns that rewards cause less stress than punishments (Homans, 1961).

Several indicators support the Piagetian proposition that egocentrism should diminish with increasing age. First, the increasing use of question tallies with the Piagetian view that the child learns consciously to adapt his viewpoint to others rather than assumes (falsely) that they share the same world view.

⁴ M. O. Gordon has suggested to me that a decrease in negative socioemotional behavior may simply reflect the child's learning of positive and task oriented behaviors.

Second, four developments in interaction sequences reinforce the above interpretation: questioning proactions tend increasingly to be followed by attempted answers and negative reactions, attempted answers tend to be followed by attempted answers, and negative proaction-negative reaction tends to fall away with age. Questions followed by attempted answers shows obvious awareness of alter and action directed towards alter. Positive proactions followed by questions may indicate that ego likewise is consciously trying to modify his world view toward alter through clarifying whatever the positive proaction was itself a reaction to. Finally, the age-related decrease in negative reactions to negative proactions also suggests that the individual is becoming less egocentric.

Furthermore, we observed more behavioral acknowledgment of status and power in interpersonal relationships with increasing age. Following the symbolic interactionist argument, as the person comes to know more clearly who he is, he comes to know those around him more clearly. This clearer perspective would naturally include his conception of his *relationship to others*.

An adequate conception of others is probably not the final step in the development of adult behavior, as symbolic interactionist and Piagetian literature suggests (Mead, 1934; Flavell, 1968). Instead, an individual normally advances from an egocentric viewpoint, to perceiving other individual viewpoints, to incorporating other viewpoints (the generalized other), to perceiving the group as group. This ability to perceive the group as group has been inferred from the growing number of relationships larger than two which the individual participates in as well as his increasing tendency to address the whole group rather than specific members.

We have yet to relate intra-age dynamics to inter-age developments; that is, the total capacity for particular types of interaction content at younger ages. The same phase movements were observed for all ages, and younger children used different proportions of each type of interaction form than older individuals. The only difference across age appeared to lie in the amount of such behavior which varied within sessions. This points to another hypothesis: intra-session

phase dynamics seem invariant regardless of variation in age related proportions of interaction. This hypothesis implies that strong pressures towards homeostasis must exist in interpersonal relations, regardless of age.

By contrast, inter-age developments appear to become more rational and efficient by the same reasoning. That is, the more the individual relates to others, the more he encounters situations which call for his understanding the other's point of view. Thus, each new relationship reaccentuates the need for adjusting his position; and this adjustment, to succeed, requires an efficiency and rationality not necessary in sustained relationships.

BIBLIOGRAPHY

- Bales, R. F.
 1950 *Interaction Process Analysis*. Cambridge, Massachusetts: Addison-Wesley.
 1970 *Personality and Interpersonal Behavior*. New York: Holt, Rinehart & Winston.
- Bales, R. F. and A. P. Hare
 1965 "Diagnostic use of the interaction profile." *Journal of Social Psychology* 67(December):239-58.
- Borgatta, E. F.
 1962 "A systematic study of interaction process scores, peer and self assessments, personality and other variables." *Genetic Psychology Monographs* 65(May):219-92.
- Bradburn, N. M.
 1969 *The Structure of Psychological Well-Being*. Chicago: Aldine.
- Bradburn, N. M. and D. Caplovitz
 1965 *Reports on Happiness: A Pilot Study of Behavior Related to Mental Health*. Chicago: Aldine.
- Campbell, D. T. and J. C. Stanley
 1963 *Experimental and Quasi-Experimental Design for Research*. Chicago: Rand McNally.
- Dyke, A. J.
 1963 "The social contacts of some midwest children with their parents and teachers." Pp. 78-98 in R. G. Barker (ed.) *The Stream of Behavior*. New York: Appleton-Century.
- Etkin, W.
 1967 *Social Behavior from Fish to Man*. Chicago: University of Chicago Press.
- Flavell, J. H., P. Botkin, C. Fry, J. Wright, and P. Jarvis
 1968 *The Development of Role-Taking and Communication Skills in Children*. New York: John Wiley.
- Foa, V.G.
 1964 "Cross-cultural similarity and difference in interpersonal behavior." *Journal of Abnormal and Social Psychology* 68(May):517-22.

- Hare, A. P.
1962 *Handbook of Small Group Research*. New York: Free Press.
- Heinicke, C. and R. F. Bales
1953 "Developmental trends in the structure of small groups." *Sociometry* 16(March):10:19.
- Homans, G. C.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace and World.
- James, J.
1951 "A preliminary study in size determinant in small group interaction." *American Sociological Review* 16(August):474-7.
1953 "The distribution of free-forming small group size." *American Sociological Review* 18(October):569-70.
- McCall, G. J. and J. L. Simmons
1966 *Identities and Interactions*. New York: Free Press.
- Morris, C. W. (ed.)
1934 *George Herbert Mead: Mind Self and Society from the Standpoint of a Social Behaviorist*. Chicago: University of Chicago Press.
- Mussen, R. (ed.)
1960 *Handbook of Research Methods in Child Development*. New York: John Wiley.
- Parten, M. B.
1933 "Social play among pre-school children." *Journal of Abnormal and Social Psychology* 28(July-September): 136-47.
- Piaget, J.
1954 *The Moral Judgment of the Child*. Cleveland: Meridian.
1957 *The Language and Thought of the Child*. Cleveland: Meridian.
- Rausch, H. L.
1965 "Interaction sequences." *Journal of Personality and Social Psychology* 2(October): 487-99.
- Reck, A. J. (ed.)
1964 *George Herbert Mead: Selected Writings*. Indianapolis: Bobbs-Merrill.
- Smith, A. J.
1960 "A developmental study of group processes." *Journal of Genetic Psychology* 97(September):29-39.
- Turner, R. H.
1968 "The self-conception in social interaction." Pp. 93-106 in Chad Gordon and K. J. Gergen, (eds.), *The Self in Social Interaction*, Volume 1. Evanston, Illinois: John Wiley.

DISTURBANCE IN THE SELF-IMAGE AT ADOLESCENCE

ROBERTA G. SIMMONS *

University of Minnesota

FLORENCE ROSENBERG

The American University

National Institute of Mental Health

MORRIS ROSENBERG

American Sociological Review 1973, Vol. 38 (October):553-568

The purpose of this cross-sectional study was to investigate whether adolescence is a period of disturbance for the child's self-image and, if so, at what age of adolescence and under what social conditions the disturbance is greatest. Several dimensions of the self-image were measured among 1,917 urban school children in grades three through twelve. Compared to children in the eight to eleven age group, the early adolescents, particularly those between twelve and thirteen, were shown to exhibit heightened self-consciousness, greater instability of the self-image, slightly lower self-esteem, and a less favorable view of the opinions held of them by significant others. Evidence is presented suggesting that the child's environment may have a stronger effect than his age in producing such changes. Children who had entered junior high school appeared more disturbed along these lines than their age-peers still in elementary school.

AMONG the most widely accepted ideas in the behavioral sciences is the theory that adolescence is a period of disturbance for the child's self-image. Hall (1904) originally characterized the age as one of "storm and stress." Erikson (1959) views it as a time of identity-crisis, in which the child struggles for a stable sense of self. Psychoanalytic theory postulates that the burgeoning sexual desires of puberty spark a resurgence of oedipal conflicts for the boy and pre-oedipal pressures for the girl (Blos, 1962, 1971; A. Freud, 1958). To establish mature cross-sexual relationships in adulthood, the child must resolve these conflicts during adolescence. In the interim, the physiological changes of puberty and the increase in sexual desire challenge the child's view of himself in fundamental ways. Both his body-image and his self-image radically change.

Sociologists (Davis, 1944) traditionally characterize adolescence as a period of physical maturity and social immaturity.¹ Be-

cause of the complexity of the present social system, the child reaches physical adulthood before he is capable of functioning well in adult social roles. Adolescence becomes extremely difficult because the new physical capabilities and new social pressures to become independent coincide with many impediments to actual independence, power, and sexual freedom.

The resulting status-ambiguities, that is, the unclear social definitions and expectations, have been seen as engendering a corresponding ambiguity of self-definition. In addition, the need to make major decisions about future adult roles on the basis of what he is like at present further heightens the adolescent's self-awareness and self-uncertainty (Erikson, 1959).

From society's viewpoint, these external and internal pressures to plan for a future career, to become more independent, and to establish relationships with the opposite sex, all direct the individual away from his family of origin toward the creation of a new family. In the course of adolescence he changes from a dependent being whose prime emotional attachments are to his family of origin into a person capable of embarking on an independent existence, ready to establish his most important emotional allegiances outside of his present family. With all these physical, emotional and social changes, it is

* The work of the first author is currently supported by a Research Development Award from the National Institute of Mental Health, #5-K1-MH-41, 688-03. The work was also partly supported by USPHS Grants 1-F3-MH-41, 688-01 and MH-197541-01.

¹ See Gordon (1971), Bakan (1971) and Kohlberg and Gilligan (1971) for discussions of adolescence as a social phenomenon.

small wonder that social theorists assume that this period is difficult for the child's self-image.

Yet Offer (1969), on the basis of his longitudinal study of adolescent boys from ages fourteen to eighteen, suggests that for most boys these years are not characterized by stress or turmoil. Other investigators (e.g., Grinker, 1962; Elkin and Westley, 1955; Douvan and Adelson, 1966; Weiner, 1970) also question the assumption of adolescent crisis. These studies, however, often do not deal with early adolescence; nor do they systematically measure differences in the self-image over age.

Aside from psychiatric case-histories, in fact, there is little evidence to refute or support the argument that the child's self-image changes from childhood to adolescence. (See Engel, 1959; Piers and Harris, 1964; Jorgenson and Howell, 1969). Since most work on adolescent disturbance has been clinical in nature, several fundamental questions on the self-image remain to be answered. First, do data support the belief that the adolescent's self-image differs from that of younger children? If so, could one term this difference a "disturbance," that is, a change which would cause the child some discomfort or unhappiness? In this paper we use the word "disturbance" as a milder term than "turmoil," "storm or stress," or "crisis," so that we can encompass less severe changes. It is not meant to imply psychopathology.

Second, if there is an adolescent self-image disturbance, when does it begin? This question is crucial to the evaluation of certain theoretical notions. Erikson (1959) tells us that the adolescent must deal with the issues of a career decision and the establishment of his own family. While these concerns may be salient to the eighteen or nineteen year old, they do not concern the twelve year old. Conversely, it is the younger adolescent who is confronted with the body-image changes of puberty. This study tries to specify the onset of adolescent self-image disturbance.

Third, if there is an adolescent self-image disturbance, what is the course of its development? Do the problems appearing at the time it is precipitated continue to grow? Do they level off at a higher plane? Or do they decline as the adolescent learns to cope with them?

Finally, if it does exist, what triggers the adolescent disturbance? Typically, the onset of puberty is viewed as the trigger. But perhaps aspects of the social environment are at work.

Self-Image Dimensions

In this paper we adopt Gardner Murphy's (1947) view of the self as "the individual as known to the individual." So conceived, the self-image can be viewed as an attitude toward an object; and, like all attitudes, it has several dimensions (Rosenberg, 1965). We shall deal with four of these. In each case, there is reason to think that changes in these dimensions would be disturbing or uncomfortable for the individual.

The first dimension is self-consciousness; it refers to the salience of the self to the individual. As Mead (1934) posited, in an interaction the ordinary individual must take account of others' reactions to himself and his behavior. But people vary in the degree to which the self is an object of attention. Some people are more "task-oriented," i.e., more involved in the situation and less concerned with how they are doing or what others are thinking of them. For others, the self becomes so prominent that the interaction is uncomfortable. Do adolescents show more of this type of uncomfortable self-consciousness than younger children?

The second dimension of the self-image is stability. If an individual must take account of himself as an important part of a situation and if he is unsure of what he is like, then he is deprived of a basis for action and decision. Indeed, Lecky (1945) described the self-concept as "the basic axiom of one's life theory," and Brownfain (1952) showed instability to be associated with disturbance. The question is whether this stability is especially shaken during adolescence.

The third dimension is self-esteem, i.e., the individual's global positive or negative attitude toward himself. The importance of this feeling has been widely recognized. (William James, 1950; McDougall, 1908). Probably more research has been devoted to this aspect of the self-concept than to all others combined (Wylie, 1961). In part, this interest is probably attributable to the great relevance of self-esteem for emotional dis-

turbance (Kaplan and Pokorny, 1969; Rogers, 1951; Rosenberg, 1965; Turner and Vanderlippe, 1958; Wylie, 1961, Ch. IV). Is there evidence of self-esteem disturbance during adolescence?

The final dimension deals with the "perceived self."² While technically not an integral part of the phenomenal self, there is both theoretical and empirical reason to believe that the perceived self has an extremely important bearing on the self-image, particularly the self-esteem. Mead's (1934) and Cooley's (1912) classic theories emphasized the importance to the individual of his perceptions of how others see him. (For empirical support, see Miyamoto and Dornbusch, 1956; Reeder, Donohue, and Biblarz, 1960; Sherwood, 1965; Manis, 1955; Helper, 1955; Rosenberg and Simmons, 1972). Our question is whether adolescents are more likely than younger children to see others as viewing them unfavorably?

METHOD

Sample

The data for this analysis were collected from public school children in grades three through twelve in Baltimore City in 1968. A random sample of 2,625 pupils distributed among twenty-five schools was drawn from the population of third to twelfth grade pupils. Each school in Baltimore City was initially stratified by two variables: (1) proportion of non-white students, and (2) median income of its census tract. Twenty-five schools falling into the appropriate intervals were randomly selected. From each school, 105 children were selected by random procedures from the central records.

Some children had withdrawn from school after the central records were compiled and were no longer available. However, we were able to interview 1,917 children, that is, 79.2 percent of the sample children still registered in the school, or 73.0 percent of all children

originally drawn from the central records.³ Closely reflecting the population, the present sample is 63 percent Negro and more heavily working class than the national average. None of the findings presented here were found to be spurious when controlled for race or class.

Comparisons across ages, however, must take account of the fact that school dropouts are absent from our population of older adolescents. The fact that this study is a cross-sectional rather than panel design generally limits the conclusiveness of the findings. We cannot be certain that age differences represent actual changes, particularly in the higher school grades.

Each subject was interviewed directly after school in his school. For the elementary school children, objective background information was collected from the parents. Parents were reached either by a five to ten minute telephone interview or, when there was no telephone, by home interview. Almost all parents were extremely cooperative and in only sixty cases were we unable to locate the parent or conduct the interview.

Measures

Indexes were developed to measure the four aspects of the self-image discussed above. (The indicators of each measure are presented in the Appendix along with their Guttman scale coefficients.) "Self-consciousness" is based on a seven-item Guttman Scale. (Example: "If a teacher asked you to get up in front of the class and talk a little bit about your summer, would you be very nervous, a little nervous, or not at all nervous?") "Stability of self" is indexed by a five-item Guttman Scale. (Example: "A kid told me: 'Some days I like the way I am. Some days I do not like the way I am.' Do your feelings *change* like this?")

Since the self-esteem dimension is central

² There is no standard terminology to communicate the idea of the individual's perception of how others see and evaluate him. Different terms have been used, for example, by Cooley (1912), Miyamoto and Dornbusch (1956), Reeder, et al. (1960), and Backman, et al. (1963). For want of a better term we shall use "perceived self."

³ One school—a combined elementary and junior high school—was entered twice in the total population of schools and, by chance, was selected in both categories. It was not practicable to double the sample size of this school; hence, the responses of these thirty-five elementary school children and those of the thirty-six junior high school children were doubled in weight to better represent the total population. In our analysis, we have thus treated our sample as 1,988 children.

this concept was measured in two ways. First, we ascertained the individual's general, overarching feeling toward himself through a series of general questions; we call this the global measure of self-esteem. For this purpose, a six-item Guttman Scale was used. (Example: "Everybody has some things about him which are good and some things about him which are bad. Are most of the things about you good, bad, or are both about the same?")

As Murphy (1949) has observed, however, the individual's self-attitude is both general and specific. He not only has attitudes toward himself as a totality but also attitudes toward his specific qualities, such as his looks or his intelligence. A different approach to measurement, then, is to infer the individual's general self-assessment from his specific self-evaluations; we call this the specific approach. Both the global and specific indices are designed to measure general self-esteem; they are simply founded on competing rationales. In this study, the specific approach to self-esteem measurement is based on the individual's average self-assessment on the following eight characteristics: being smart, good-looking, truthful or honest, good at sports, well-behaved, hard-working in school, helpful, and good at making jokes.

While certain investigators (Miyamoto and Dornbusch, 1956) speak of "the perceived self," clearly, the individual has many perceived selves since he interacts with many types of people who evaluate him. Some of these perceived selves were investigated by asking these children what they believed the following people thought of them: their parents, their teachers, children of the same sex, and children of the opposite sex.

RESULTS

The Disturbance

Does adolescence produce a disturbance in the child's self-picture? Table 1 clearly suggests that the emergence of self-image problems in adolescence is no myth, and that these problems occur early in adolescence. In general, self-image disturbance appears much greater in the twelve to fourteen age group than in the eight to eleven age group. In contrast to younger children, the early

adolescents (twelve to fourteen year olds) show a higher level of self-consciousness, greater instability of self-image, slightly lower global self-esteem, lower specific self-esteem, and a more negative "perceived self" (that is, they are less likely to think that parents, teachers, and peers of the same sex view them favorably). The assumption that such changes are likely to be disturbing is consistent with the fact that early adolescents also show a higher level of depressive affect than do the younger children (Table 1).⁴ The only area showing improvement in early adolescence involves the opposite sex: children see themselves as better liked by the opposite sex as they grow older.

While the early adolescents are more self-conscious and have a more unstable self-image, this self-consciousness appears to decline somewhat in later adolescence and the self-image becomes somewhat more stable. However, even in late adolescence, the subjects manifest greater self-consciousness and instability than do the eight to eleven year old children.

Only for global self-esteem is there an improvement in later adolescence marked enough for the youngsters from age fifteen up to score more favorably than the eight to eleven year olds. The older adolescents show higher global self-esteem than both the young children and the early adolescents. Earlier studies (Engel, 1959; Piers and Harris, 1964) have also shown an increase in self-esteem among senior high school students; furthermore, one of these (Piers and Harris, 1964) also demonstrated a decline in self-esteem in early adolescence as compared to childhood.

Although global self-esteem feelings decline only slightly in early adolescence, and rise conspicuously in later adolescence, this pattern is not true of self-esteem based on those specific qualities we have considered, such as intelligence, honesty, diligence and good behavior. If one simply averages the self-ratings on these qualities, he will find a relatively sharp decline between childhood and early adolescence; and this lowered self-evaluation continues into later adolescence.

While one may reasonably assume that the

⁴ The scale of "depressive affect" appears in the Appendix.

Table 1. Children's Self-Ratings by Age

	Age			Total: Median χ^2 Test ^b
	Median Scores			
	8-11 (N = 819) ^a	12-14 (N = 649)	15+ (N = 516)	
Self-consciousness (Low score = high self-consciousness)	3.8 ***	3.0	3.2	p < .001
Stability of the self-image (Low score = high instability)	2.6 ***	2.1	2.3	p < .001
Self-esteem (global) (Low score = low self-esteem)	4.0	3.8 ***	4.4	p < .001
Self-esteem (specific) (High score = unfavorable rating)	3.6 ***	5.0	5.0	p < .001
Perceived self (High score = unfavorable rating)				
Perceived opinion of parents	4.8 **	5.1	5.1	p < .01
Perceived opinions of teachers	3.2 ***	3.4	3.4	p < .001
Perceived opinions of peers of the same sex	1.6 ***	1.8 **	2.0	p < .001
Perceived opinions of peers of opposite sex	2.4 ***	2.1 **	2.0	p < .001
Depressive affect (High score = high depression)	2.3 ***	2.9	3.0	p < .001

* = p < .05 for adjacent age groups according to median χ^2 test;

** = p < .01;

*** = p < .001.

Tests between adjacent age groups are not entirely appropriate, in part, because of the nonindependence of comparisons (i.e., the 12-14 age category is compared with each of the other age groups). Since the test affords some indication of how seriously to take the observed differences, however, it is included for convenience. (See Blalock, pp. 328-34 for a discussion of this problem in the case of analysis of variance).

^aFor missing data, total cases are reduced accordingly.

^bSiegel (1956:111-116, 179-184).

lowered self-evaluations of these specific qualities indicate some degree of self-image disturbance, this conclusion is not certain. For, as William James (1890) long ago noted, it is not simply a question of how favorably the individual judges himself, but also how much he has staked himself on a particular quality. For example, an adolescent may agree that he is poor at sports or is

plain; but if he cares little about these qualities, he will not be disturbed by their lack. Thus only a low self-rating on a quality that is valued highly is likely to be experienced as disturbing.

To take account of self-values, we asked our respondents how much they cared about each of these qualities, i.e., how important they were to them. Table 2 deals solely with

Table 2. Proportion Rating Selves Very Favorably on Each Characteristic among Those Who Care "Very Much" about that Characteristic, by Age

Respondent Rates Self Very Favorably on Following Qualities	Age			Total: χ^2 Test
	8-11	12-14	15 or Older	
Smart	26% *** (547)	9% (366)	5% (244)	p < .001
Good-looking	20% * (258)	13% * (197)	6% (121)	P < .001
Truthful or honest	54% *** (527)	38% (424)	38% (320)	p < .001
Good at sports	50% (339)	46% (266)	42% (163)	p < .001
Well-behaved	46% *** (474)	31% (332)	39% (239)	p < .001
Work hard in school	71% *** (494)	50% ** (373)	39% (231)	p < .001
Helpful	60% *** (506)	46% (329)	46% (225)	p < .001
Good at making jokes	49% (151)	40% (53)	46% (28)	p < .05

* = p < .05 for adjacent age groups according to χ^2 test;

** = p < .01;

*** = p < .001.

Tests between adjacent age groups are not entirely appropriate, partly because of nonindependence of comparisons. Since the test affords some indication of how seriously to take the observed difference, however, it is included for convenience.

children who care "very much" about whether they are smart, good-looking, helpful, etc. It is among these children that one would expect an unfavorable self-rating on a quality to be psychologically upsetting (Rosenberg, 1965: Ch. 13).

With respect to this criterion, Table 2 indicates that the early adolescents (twelve to fourteen) have a consistently lower self-image than the younger children (eight to eleven); i.e., they are less likely to rate themselves very favorably on the qualities they consider important. In some cases, such as being "good at making jokes," the differences are minor; for others they are large. On the other hand, there is little consistent difference between early and later adolescents in this regard. The consistent and clear age difference appears between childhood and early adolescence, with the early adolescents less likely to say they are performing well with respect to their self-values.

It may be contended that the lower self-

ratings on these qualities simply reflect the fact that adolescents are more "realistic" while the younger children tend to "inflate" their self-qualities. Other analyses of these data have shown that compared to older children, elementary school children do tend to inflate the prestige of their racial and ethnic group status and their father's occupational status (Simmons and Rosenberg, 1971; Rosenberg and Simmons, 1972). Perhaps the adolescent does become more realistic about what he is like, but this does not mean that the adjustment to reality is not distressing for him. As Blos (1962:192) has noted: "The difficulty of relinquishing the inflated self-image of childhood is usually underestimated." From the viewpoint of one's emotional state, "reality" is not the issue.

To summarize, the results show a general pattern of self-image disturbance in early adolescence. The data suggest that, compared to younger children, the early adoles-

cent has become distinctly more self-conscious; his picture of himself has become more shaky and unstable; his global self-esteem has declined slightly; his attitude toward several specific characteristics which he values highly has become less positive; and he has increasingly come to believe that parents, teachers, and peers of the same sex view him less favorably. In view of these changes, it is not surprising that our data show early adolescents to be significantly more likely to be psychologically depressed.

The course of self-image development after twelve to fourteen is also interesting. In general, the differences between early and late adolescence are not large. There is improvement in self-consciousness, stability, and especially global self-esteem, but no improvement in assessment of specific qualities or in the perceived self. The main change occurs almost always between the eight to eleven year old children and the twelve to fourteen year old children.

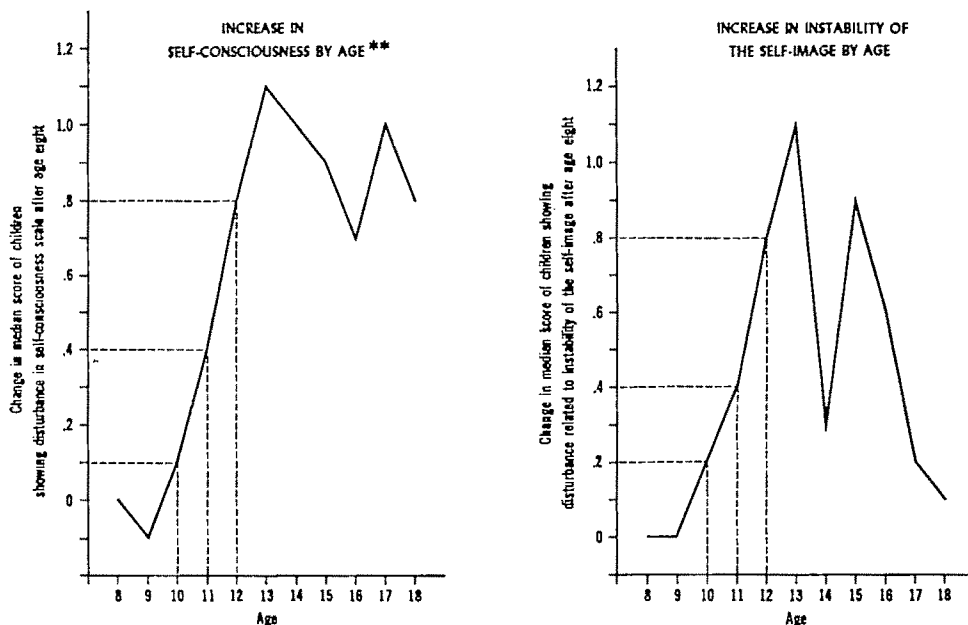
Onset of the Disturbance

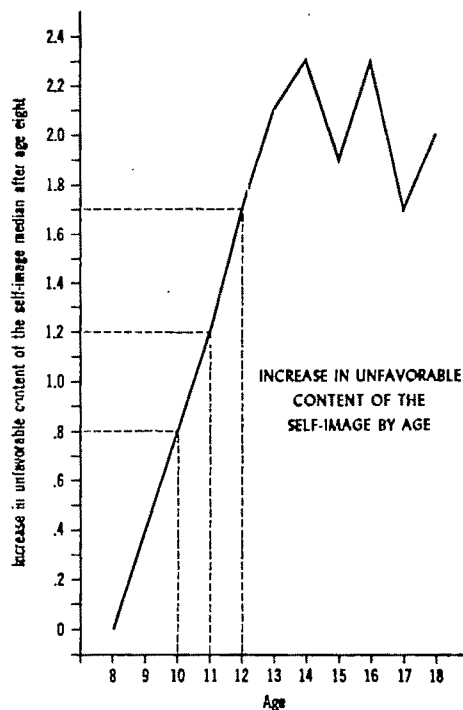
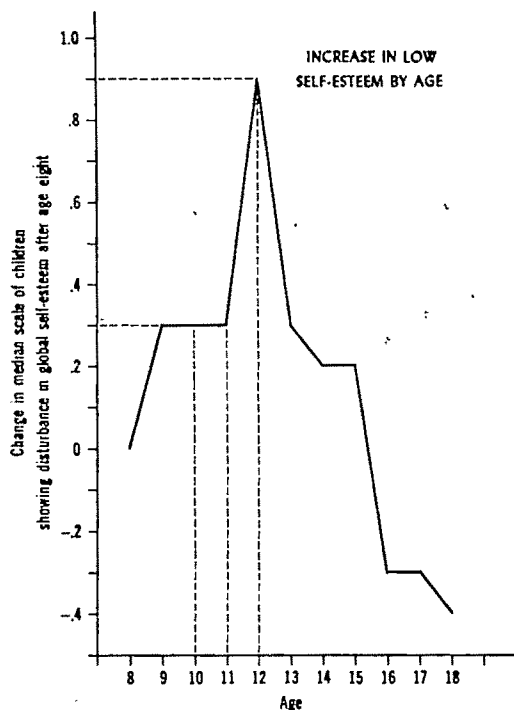
Can we be more specific about this change in the self-image from childhood to early adolescence? Does it occur gradually or suddenly? As Figure 1 shows, a noticeable dif-

ference appears between eleven year olds and twelve year olds. (Note that the eleven year old group includes children from eleven years, no months, to eleven years, eleven months; while the twelve year old group covers those from twelve years, no months to twelve years, eleven months.) Self-consciousness, instability of the self-image, low global self-esteem, and low specific self-esteem all rise relatively sharply among the twelve year olds compared to the eleven year olds, although in most cases some rise has begun earlier, particularly the year before. This movement from the eleven year old group to the twelve year old group is the only one-year period in which the children show an increase of disturbance on all these measures. In fact, on all four measures it is the largest yearly increase in disturbance up to that age. For three out of four measures, it is the largest increase between any two ages.

For almost all the dimensions considered here, disturbance continues to increase after twelve, but in most cases the high point of disturbance occurs either at age twelve, thirteen, or fourteen. In fact, stability of self-image and global self-esteem seem to improve after this point, particularly in late adolescence; while disturbances in self-consciousness and in specific self-esteem seem to

FIGURE 1*





* For each scale, the median score of the eight-year old group is subtracted from the median score of each subsequent age group. If the graph line rises, then disturbance along the dimension is said to increase. The points above "0" indicate a higher level of disturbance after age 8, while the points below "0" indicate a lower level.

** The values at age 10, 11 and 12 are indicated by dotted lines. In the sample there are 98 eight-year olds, 225 nine-year olds, 263 ten-year olds, 233 eleven-year olds, 237 twelve-year olds, 213 thirteen-year olds, 199 fourteen-year olds, 150 fifteen-year olds, 162 sixteen-year olds, 130 seventeen-year olds and 56 eighteen-year olds.

level off and remain at early adolescent levels. The sole area of increasing disturbance in later adolescence involves the children's perceptions of the opinions of significant others.

The course of global self-esteem development deserves special mention. We noted earlier that the self-esteem level of the twelve to fourteen year group was only slightly lower than that of the eight to eleven group. But this finding conceals an important change: the sudden dramatic decline in self-esteem among the twelve year olds (Figure 1: compare eleven and twelve year olds). But during the following year, when the children reach thirteen, global self-esteem rapidly returns to its earlier level and continues to rise in later adolescence. Later we shall see some possible reasons for this dramatic but temporary shift.

In sum, the data suggest that when they are twelve years old (that is, between their twelfth and thirteenth birthdays), children

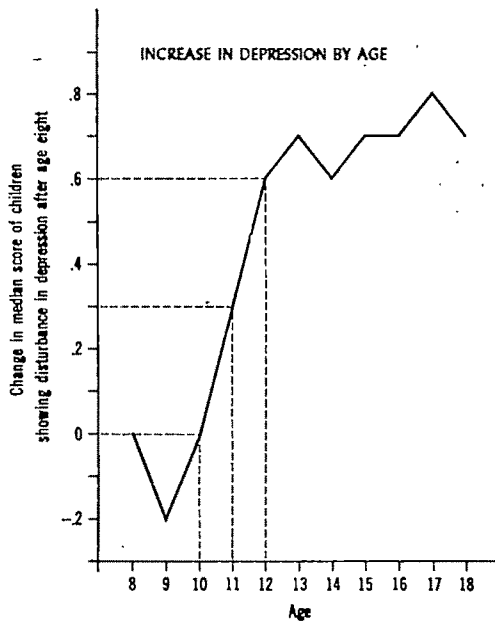
tend to experience a more marked increase in self-image disturbance. For some dimensions, this relatively sharp increase continues among those who are age thirteen. It is relevant to note that early adolescence is also characterized by a corresponding increase in feelings of depression or unhappiness, though this rise has clearly begun earlier (Figure 2). After age thirteen, there is again a general leveling off.

Environmental Context

Placing the rise in self-image disturbance at some time after the twelfth birthday would seem to agree with the assumption that puberty is the chief determinant of this disturbance. But are there factors in the social environment which may also be responsible for these changes?

One important environmental change occurs for most children at this time. They generally begin their last year of elementary

FIGURE 2.



school (the sixth grade) when they are eleven and the first year of junior high school (the seventh grade) when they are twelve. Does the movement into junior high school itself contribute to the increase in self-image disturbance?

Obviously, one cannot examine the effects of change in environment by comparing sixth and seventh graders since one does not know whether such differences are due to the fact that the seventh graders are in junior high school or simply that they are older. It is, however, possible to disentangle the effects of age maturation and school contexts by comparing children of the same ages.

By the spring of the school year, when our data were collected, both the sixth and seventh grades held an appreciable number of twelve year olds. If the junior high school experience were particularly stressful for the child, then the twelve year olds in junior high should show greater disturbance of their self-images than the twelve year olds in elementary school.

Table 3 dramatically supports this hypothesis. The twelve year olds in junior high school have lower global self-esteem, lower specific self-esteem, higher self-consciousness, and greater instability of self-image than their age-peers in elementary school. For example, 41 percent of the twelve year olds in junior high school indicate low global self-esteem in contrast to only 22 percent of those in elementary school; 43 percent of the former manifest high self-consciousness compared to only 27 percent of the latter. All but one of the these differences are statistically significant beyond the .05 level.⁵

If these findings are valid, they certainly afford a vivid illustration of the way a social context can affect individual personality. Yet it is possible that these differences are spurious. Perhaps the sixth grade twelve year olds differ in other ways from the seventh grade twelve year olds. The sixth grade twelve year olds are more likely to have poorer grades, to be black, and to be from the lower social classes; but these factors do not appear likely to improve their self-images. In

⁵ One's view of the opinions of significant others, however, does not appear to be affected by movement into junior high school.

Table 3. Disturbance of the Self-Image by School Context, among Twelve-year-old Children

Self-Image Disturbance	Twelve-year-old Children		According to χ^2 Analysis
	In Elementary School	In Junior High School	
Percent low self-esteem (global)	22% (167)	41% (59)	$p < .01$
Percent low self-esteem (specific)	28% (151)	46% (57)	$.10 > p > .05$
Percent high self-consciousness	27% (172)	43% (61)	$p < .05$
Percent high instability of self-image	30% (158)	53% (60)	$p < .01$

any case, controlling for these factors by means of test factor standardization (Rosenberg, 1962), we find that none of the original differences involving global self-esteem, specific self-esteem, self-consciousness, or stability of self-concept, can be explained by any of these variables. Even when standardized on race, class, or marks in school, all differences between elementary school and junior high school twelve year olds remain essentially unchanged. Furthermore, Table 4 shows that in general these findings hold for blacks as well as whites, for middle class as well as working class respondents, and for students with high as well as low grades.

Does this mean that the only remaining difference between these two types of twelve year olds is the school which they attend? One other possibility involves the relative ages of these two groups in their classes. The sixth grade twelve year olds are among the oldest and biggest children in their class, while the seventh grade twelve year olds are among the youngest and least physically mature. The self-pictures of the sixth grade twelve year olds could benefit from their relative advantage, while the self-images of their seventh-grade age peers could suffer from their age-rank in their group. If so, the sixth grade twelve year olds should have more positive self-images than the younger children in their classes; while the seventh grade twelve year olds should show more

disturbed self-images than the older children in their grade.

Yet Table 5 shows there is virtually no difference between the self-image ratings of eleven and twelve year olds in the sixth grade, nor is there a difference between the self-pictures of the twelve and thirteen year olds in the seventh grade.

Thus, the transition into junior high school seems to represent a significant stress along several dimensions of the child's self-image; while aging from eleven to twelve and twelve to thirteen does not in itself appear stressful. Within the same school class, age makes little difference; but within the same age group, school class makes a great difference.

One further question is whether the self-image disturbance associated with the transition from sixth to seventh grade results from the general disturbance associated with transferring to any new school or whether it is specifically associated with entry into a junior high school. One way to examine this question is to look at those twelve year old sixth graders who have moved to their current schools this year. These children are identical with other twelve year old sixth graders in grade level but are different in being new students; conversely, they are similar to all twelve year old seventh graders in being new students, but are different in grade level.

Table 4. Disturbance of the Self-Image among Twelve-year-olds in the Sixth or Seventh Grade, by Race, Social Class, and Marks in School

Self-Image Disturbance	Race				Social Class				Marks in School			
	Blacks		Whites		Middle Class		Working Class		A's and B's		C's and Below	
	6th Gr.	7th Gr.	6th Gr.	7th Gr.	6th Gr.	7th Gr.	6th Gr.	7th Gr.	6th Gr.	7th Gr.	6th Gr.	7th Gr.
Percent low self-esteem (global)	18% (106)	33% (27)	30% (61)	47% (32)	14% (21)	44% (16)	21% (119)	36% (39)	20% (49)	37% (30)	23% (104)	42% (24)
Percent low self-esteem (specific)	33% (92)	46% (26)	19% (58)	45% (31)	48% (21)	41% (17)	26% (108)	44% (36)	17% (47)	39% (28)	29% (91)	50% (24)
Percent high self-consciousness	28% (109)	32% (28)	27% (62)	52% (33)	24% (21)	53% (17)	29% (122)	38% (40)	22% (50)	37% (30)	29% (106)	58% (26)
Percent high instability of the self-image	31% (104)	50% (28)	26% (53)	56% (32)	35% (20)	71% (17)	27% (112)	44% (39)	30% (47)	52% (29)	28% (96)	54% (26)

Table 5. Disturbance of the Self-Image by Age, by Grade in School

	Grade in School			
	Sixth Grade		Seventh Grade	
	Age 11	Age 12	Age 12	Age 13
Percent low self-esteem (global)	29% (73)	24% (106)	40% (58)	36% (101)
Percent low self-esteem (specific)	20% (69)	25% (93)	46% (56)	41% (90)
Percent high self-consciousness	22% (76)	27% (106)	42% (60)	46% (101)
Percent high instability of self-image	30% (76)	33% (102)	53% (59)	46% (92)

Unfortunately, the number of twelve year olds in the sixth grade new to their schools is small; the results are thus no more than suggestive. Nevertheless, the data in Table 6 are particularly interesting. Though some differences exist between sixth graders new in the school and other sixth

graders, the differences are inconsistent and would not suggest less disturbed self-images in either group. But the findings involving junior high students are clear and consistent. The twelve year olds in junior high are considerably more likely than either sixth grade group to show disturbances of the

Table 6. Self-Variables by Grade and Geographical Mobility, among Twelve-year-olds Only

Self-Image Disturbance	Twelve-year-old Children		
	6th Grader; Not New to School	6th Grader; Moved into School this Academic Year	7th Grader
Self-consciousness			
High	28%	25%	42%
Medium	50	40	48
Low	22	35	10
N = 100%	(67)	(20)	(60)
Instability of self-image			
High	36%	18%	53%
Medium	49	76	41
Low	15	6	7
N = 100%	(66)	(17)	(59)
Self-esteem (global)			
Low	23%	15%	40%
Medium	27	55	36
High	50	30	24
N = 100%	(68)	(20)	(58)
Self-esteem (specific)			
Low	23%	44%	46%
Medium	51	44	30
High	26	12	23
N = 100%	(61)	(16)	(56)

self-image, by scoring higher on self-consciousness, higher on instability of self-image and lower on global self-esteem. With regard to specific self-esteem, they are slightly higher than the sixth grade newcomer but much lower than the other sixth graders. The results as a whole suggest that a twelve year old child who moves from one elementary school to another may not find the experience as stressful as does the twelve year old who has entered a junior high school in the past year.

Note, incidentally, that the transition from junior to senior high school does not show a parallel effect on the self-image; fifteen year olds in senior high school do not show more disturbed self-images than fifteen year olds in junior high school. Why this should be so is not clear. Perhaps the difference between junior high school and elementary school is experienced as much greater than the difference between junior and senior high school.

In sum, the data indicate increased self-image disturbance associated with the transition from elementary to junior high school. The reason does not appear to be solely the age change (with its associated biological changes); for at ages roughly equivalent, the seventh graders still show greater disturbance. Nor does it simply appear to be the shock of transferring to a new school; although the number of cases is small, the newly arrived sixth graders generally show less disturbance than seventh graders and are not consistently worse off than other sixth graders (all at the same age). Furthermore, the transition from junior to senior high shows no such effect.

Perhaps puberty does not in itself disturb the self-image but heightens vulnerability to environmental circumstances which threaten the self-concept. Only further research can determine what it is about the junior high school experience that is stressful for the self-image.

SUMMARY AND DISCUSSION

This cross-sectional study has investigated several dimensions of self-image development in 1,917 urban school children in grades three through twelve. A definite disturbance of the self-image has been shown to occur in ado-

lescence, particularly early adolescence. In some respects this disturbance appears to decline in later adolescence, while along other dimensions it persists. In many areas, a particular rise in disturbance appears to occur when the child is twelve, that is, between the twelfth and thirteenth birthdays. The rise often begins a year before, and may continue for the next year or so. Often, however, it seems to increase little, if at all, after age thirteen or fourteen.

During early adolescence, compared to the years eight to eleven, the children exhibited heightened self-consciousness, greater instability of self-image, slightly lower global self-esteem, lower opinions of themselves with regard to the qualities they valued, and a reduced conviction that their parents, teachers and peers of the same sex held favorable opinions of them. They were also more likely to show a high depressive affect.

These data agree with the findings of Offer (1969, Ch. 11), who studied a somewhat older adolescent group (fourteen to eighteen), and who reports that both parents and adolescents agreed that the greatest amount of "turmoil" in their lives occurred between ages twelve to fourteen. The finding that instability of the self-picture increases during adolescence might appear to support Erikson's (1959) views on adolescent problems of ego-identity. However, Erikson seems to place the ego-identity crisis in late adolescence; whereas our data indicate a rise in instability during early adolescence.

Our data do not completely explain the dynamic processes at work in adolescence. For example, they cannot measure the effects of hormonal and other pubertal changes on the self-image in early adolescence (see Shock, 1946; Tanner, 1971). Yet they do show, dramatically, that the child's environment appears to have a stronger effect than age-maturation on certain aspects of the self-image. One of the major reasons twelve year olds are more likely than eleven year olds to show an increase in self-image disturbance appears to be that the twelve year olds have moved into junior high school. Twelve year olds in the seventh grade are more likely to indicate disturbance on these self-image measures than are twelve year olds in the sixth grade. There are no comparable differences between eleven and twelve year olds in the

SELF-IMAGE AND ADOLESCENCE

sixth grade, or between twelve and thirteen year olds in the seventh grade.

Thus, movement into junior high school at puberty is a significant event for the child. He moves from a protected elementary school, where he usually has one teacher and one set of classmates, to a much larger, more impersonal junior high where his teachers, classmates, and even his rooms are constantly shifting. He moves from a setting where the teacher is a parent-surrogate, to a more impersonal environment. Here he is expected to behave more independently and more responsibly, and he must make his first career decision—whether to take an academic, commercial, or vocational course.

That a disturbance of the self-image does not occur in the move from junior to senior high school raises a question. Is this finding due to the fact that the difference between senior and junior high is quantitative and not qualitative; i.e., that the school is bigger but of similar type? Or is it that the move into a very different type of school roughly coincides with the onset of puberty, which makes the self-image of the early adolescent more vulnerable to the assaults of the junior high school environment? Our data do not answer these questions. Many of them might be answered by a study of a middle school covering the fourth to eighth grades. In such a school, one might expect a more gradual transition to departmentalization, a more gradual buildup in others' expectations for independence and responsibility. The shift would not necessarily coincide with the onset of puberty.

Knowledge about self-concept development is still pretty much an unknown land in social psychology. Our sample tells us something about what differences appear between the ages of eight and eighteen, but there is little information about development before and after these years. Whether the level or type of self-image disturbance which develops in early adolescence persists in adult life or changes in a positive or negative direction is still unknown. Nor does our study reveal the more dynamic processes of self-image change. That would require a long-term panel study. Given the importance of the self-concept to the individual, we hope that the required research will be forthcoming.

REFERENCES

- Backman, Carl W., Paul F. Secord and Jerry R. Peirce
1963 "Resistance to change in the self-concept as a function of consensus among significant others." *Sociometry* 26(March): 102-11.
- Bakan, David
1971 "Adolescence in America: from idea to social fact." *Daedalus* 100(Fall):979-95.
- Blalock, Hubert M., Jr.
1972 *Social Statistics*. New York: McGraw-Hill Book Company.
- Blos, P.
1962 *On Adolescence: A Psychoanalytic Interpretation*. New York: Free Press.
- Brownfain, J. D.
1952 "Stability of the self-concept as a dimension of personality." *Journal of Abnormal and Social Psychology* 47(July):597-606.
- Cooley, Charles H.
1912 *Human Nature and the Social Order*. New York: Scribner's.
- Davis, Kingsley
1944 "Adolescence and the social structure." *Annals of the American Academy of Political and Social Science* 236:8-16.
- Douvan, E. and J. Adelson
1966 *The Adolescent Experience*. New York: John Wiley and Sons, Inc.
- Elkin, F. and W. A. Westley
1955 "The myth of adolescent culture." *American Sociological Review* 23:680-3.
- Engel, Mary
1959 "The stability of the self-concept in adolescence." *Journal of Abnormal and Social Psychology* 58:211-15.
- Erikson, E. H.
1959 "Identity and the life cycle." *Psychological Issues* 1:1-171.
- Freud, Anna
1958 "Adolescence." *Psychoanalytic Study of the Child* 13:255-78.
- Gordon, Chad
1971 "Social characteristics of early adolescence." *Daedalus* 100(Fall):931-60.
- Grinker, R. R., Sr., R. R. Grinker, Jr. and J. Timberlake
1962 "A study of 'mentally healthy' young males (homoclitcs)." *American Medical Association Archives of General Psychiatry* 6:405-53.
- Hall, G. S.
1904 *Adolescence: Its Psychology and Its relations to Physiology, Anthropology, Sociology, Sex, Crime, Religion and Education*. New York: D. Appleton and Company.
- Helper, M.
1955 "Learning theory and the self concept." *Journal of Abnormal and Social Psychology* 51(September):184-94.
- James, W.
1950 *The Principles of Psychology*. New York: Dover (copyright, 1890 by Henry Holt and Company).

- Jorgensen, E. Clay and Robert J. Howell
1969 "Changes in self, ideal-self correlations from ages 8 through 18." *Journal of Social Psychology* 79(June):63-7.
- Kaplan, Howard B. and Alex D. Pokorny
1969 "Self-derogation and psychosocial adjustment." *Journal of Nervous and Mental Disease* 149(November):421-34.
- Kohlberg, Lawrence and Carol Gilligan
1971 "The adolescent as a philosopher: the discovery of the self in a postconventional world." *Daedalus* 100(Fall):1051-86.
- Lecky, Prescott
1945 *Self-Consistency: A Theory of Personality*. New York: Island Press.
- McDougall, W.
1908 *Introduction to Social Psychology*. London: Methuen and Company.
- Manis, M.
1955 "Social interaction and the self-concept." *Journal of Abnormal and Social Psychology* 51(November):362-70.
- Mead, George Herbert
1934 *Mind, Self and Society*. Chicago: University of Chicago Press.
- Miyamoto, S. Frank and Sanford Dornbusch
1956 "A test of the symbolic interactionist hypothesis of self-conception." *American Journal of Sociology* 61(March):339-403.
- Murphy, Gardner
1947 *Personality*. New York: Harper.
- Offer, Daniel
1969 *The Psychological World of the Teen-ager*. New York: Basic Books, Incorporated.
- Piaget, Jean
1965 *The Moral Judgment of the Child*. New York: The Free Press.
- Piers, Ellen V. and Dale B. Harris
1964 "Age and other correlates of self-concept in children." *Journal of Educational Psychology* 55(No. 2):91-5.
- Reeder, Leo G., George A. Donohue and Arturo Biblarz
1960 "Conceptions of self and others." *American Journal of Sociology* 66(September):153-9.
- Rogers, Carl R.
1951 *Client-centered Therapy: Its Current Practice, Implications, and Theory*. Boston: Houghton-Mifflin.
- Rosenberg, Morris
1962 "Test factor standardization as a method of interpretation." *Social Forces* 41(October):53-61.
1965 *Society and the Adolescent Self-Image*. New Jersey: Princeton University Press.
- Rosenberg, Morris and Roberta G. Simmons
1972 *Black and White Self-Esteem: The Urban School Child*. Washington, D.C.: The American Sociological Association.
- Siegel, Sidney
1956 *Nonparametric Statistics for the Behavioral Sciences*. New York: McGraw-Hill Book Company, Incorporated.
- Sherwood, John J.
1965 "Self-identity and referent others." *Sociometry* 28(March):66-81.
- 1967 "Increased self-evaluation as a function of ambiguous evaluations by referent others." *Sociometry* 30(December):404-9.
- Shock, Nathan W.
1946 "Some physiological aspects of adolescence." *Texas Reports on Biology and Medicine* 4:289-310.
- Simmons, Roberta G. and Morris Rosenberg
1971 "Functions of children's perceptions of the stratification system." *American Sociological Review* 36(April):235-49.
- Stone, L. J. and J. Church
1968 *Childhood and Adolescence*. New York: Random House.
- Tanner, J. M.
1971 "Sequence, tempo, and individual variation in the growth and development of boys and girls aged twelve to sixteen." *Daedalus* 100(Fall):907-30.
- Turner, R. H. and R. H. Vanderlippe
1958 "Self-ideal congruence as an index of adjustment." *Journal of Abnormal and Social Psychology* 57:202-6.
- Weiner, Irving B.
1970 *Psychological Disturbance in Adolescence*. New York: John Wiley and Sons.
- Wylie, Ruth
1961 *The Self-Concept: A Critical Survey of Pertinent Research Literature*. Lincoln, Nebraska: University of Nebraska Press.

APPENDIX

MEASURES

Self-Consciousness Scale

Let's say some grownup or adult visitor came into class and the teacher wanted them to know who you were, so she asked you to stand up and tell them a little about yourself. Would you like that, * Would you not like it, or Wouldn't you care.

If the teacher asked you to get up in front of the class and talk a little bit about your summer, would you be . . . * Very nervous, a Little nervous, or Not at all nervous.

If you did get up in front of the class and tell them about your summer, . . . * Would you think a lot about how all the kids were looking at you, Would you think a little bit about how all the kids were looking at you, or Wouldn't you think at all about the kids looking at you.

If you were to wear the wrong kind of clothes to a party, would that bother you . . . * A lot, A little, or Not at all.

If you went to a party where you did not know most of the kids, would you wonder what they were thinking about you? . . .

* Yes, No.

Do you get nervous when someone watches you work? . . .

* Yes, No.

A young person told me: "When I'm with people I get nervous because I worry about how much they like me." Do you feel like this . . . * Often, Sometimes, or Never.

The alternatives marked with an asterisk represent high self-consciousness. This is a Guttman scale with 89.4 percent coefficient of reproducibility, 17.8 percent improvement and 62.5 percent coefficient of scalability. To validate the self-consciousness scale, we asked the interviewers to rate the child as "very nervous, somewhat nervous, or not nervous." Forty-three percent of those students categorized as "very nervous" scored high on the self-consciousness scale, in contrast to 24 percent of those rated as "not nervous." ($\chi^2 = 27.6769$, 4 df, $p < .01$). On the total scale, the cut-off point for "high self-consciousness" was obtained by trichotomizing the distribution as closely as possible and selecting the most self-conscious third. This principle was used in obtaining cut-off points for all the following scales.

Stability of Self Scale

How sure are you that you know what kind of person you really are? Are you . . . * Very sure, * Pretty sure, Not very sure, or Not at all sure.

How often do you feel mixed up about yourself, about what you are really like? . . . Often, Sometimes, or * Never.

Do you feel like this: "I know just what I'm like. I'm really sure about it." . . . * Yes, No.

A kid told me: "Some days I like the way I am. Some days I do not like the way I am." Do your feelings *change* like this? . . .

Yes, * No.

A kid told me: "Some days I am happy with the kind of person I am, other days I am not happy with the kind of person I am." Do your feelings change like this? . . .

Yes, * No.

The alternatives marked with an asterisk represent high stability. This is a Guttman scale with 89.1 percent coefficient of reproducibility, 20.1 percent improvement, 64.8 percent coefficient of scalability.

Self-Esteem Scale

Everybody has some things about him which are good and some things about him which are bad. Are more of the things about you . . . Good, * Bad, or * Both about the same.

Another kid said, "I am no good." Do you ever feel like this? (IF YES, ASK): Do you feel like this a * lot, or a * little? "I am no good."

A kid told me: "There's a lot wrong with me." Do you ever feel like this? (IF YES, ASK): Do you feel like this a * lot, or a * little? "There's a lot wrong with me."

Another kid said: "I'm not much good at anything." Do you ever feel like this? (IF YES, ASK): Do you feel like this a * a lot or * a little? "I'm not much good at anything."

Another kid said, "I think I am no good at all." Do you ever feel like this? (IF YES ASK): Do you feel like this a * a lot, or * a little. "I think I am no good at all."

How happy are you with the kind of person you are? Are you . . . Very happy with the kind of person you are, Pretty happy, * A little happy, or * Not at all happy.

The responses indicated by an asterisk indicate low self-esteem. This is a Guttman scale with 90.2 percent coefficient of reproducibility, 20.4 percent improvement, 67.6 percent coefficient of scalability. This scale has undergone extensive validation tests in Rosenberg and Simmons (1972, Ch. 2) for both whites and blacks. First of all, it has been validated against another measure of the same concept; that is, it appears to have trait validity. It is satisfactorily correlated with adolescents' scores on the Rosenberg measure of self-esteem which had been validated in previous research (Rosenberg, 1965). It was not possible to use the Rosenberg measure for the younger children in the sample because of its adult language; therefore, this present scale was constructed. Secondly, this scale seems to have construct validity—ten theoretical predictions were made concerning self-esteem and in all cases the predictions were confirmed using the scale. Self-esteem was shown to correlate positively with measures of depression and anxiety, with marks in school, with indicators of school leadership, and with the opinions of several significant others including parents, teachers and friends, for all age groups. Third, the scale appears to satisfy the interchangeability criterion; it "behaves" the same way as the Rosenberg measure of self-esteem in relation to other variables. Finally, it appears to have face validity as a measure of the individual's global feelings about his own self-worth.

Content of the Self-Image

The child was given a labeled chart with 10 bars of different sizes and asked to rank his

self on the following eight characteristics: being smart, good-looking, truthful, good at sports, well-behaved, hardworking at school, helpful, and good at making jokes. With the help of the chart he was asked: are you very (smart), pretty (smart), not very (smart), or not at all (smart). On each item, the individual was given one point for a category chosen more unfavorable than the overall median for that item. A total score was constructed by adding the points for all eight items.

Perceived Self-Image

Scales were constructed from the following items. Score values for each item are indicated in front of the appropriate category.

Parents

Would you say your mother thinks you are
1 A wonderful person, 2 A pretty nice person,
3 A little bit of a nice person, 4 or Not such
a nice person.

Would you say your father thinks you are
1 A wonderful person, 2 A pretty nice person,
3 A little bit of a nice person, 4 or Not such
a nice person.

Let's pretend your parents wanted to tell someone all about you. What type of person would they say you are? (The description was coded into the following three categories: 1 Only favorable and neutral remarks were made, 2 remarks were all neutral or were both favorable and unfavorable, 3 only unfavorable and neutral remarks were made.)

Teachers

Would you say your teacher thinks you are
1 A wonderful person, 2 A pretty nice person,
3 A little bit of a nice person, 4 or Not such a nice person.

What if your teachers wanted to tell someone all about you? What type of person would your teachers say you were? (The description was coded into the following three categories: 1 Only favorable and neutral remarks were made, 2 remarks were all neutral or were both favorable and unfavorable, 3 only unfavorable and neutral remarks were made.)

Peers

How much do boys like you? 1 Very much,
2 Pretty much, 3 Not very much, 4 Not at all.

How much do girls like you? 1 Very much,
2 Pretty much, 3 Not very much, 4 Not at all.

Depressive Affect

How happy would you say you are most of the time? Would you say you are . . . Very happy, Pretty happy, Not very happy, or * Not at all happy.

Would you say this: "I get a lot of fun out of life."

Yes, * No.

Would you say this: "Mostly, I think I am quite a happy person."

Yes, * No.

How happy are you today: Are you . . . Very happy, Pretty happy, Not very happy, or * Not at all happy.

A kid told me: "Other kids seem happier than I." Is this . . .

* True for you, or Not true for you.

Would you say that most of the time you are . . . Very cheerful, Pretty cheerful, Not very cheerful, or * Not cheerful at all.

In each case the starred alternative indicates high depression. This is a Guttman scale with 92.2 percent coefficient of reproducibility, 17.8 percent improvement, and 69.5 percent coefficient of scalability.

AN INTERPRETATION OF THE RELATION BETWEEN OBJECTIVE AND SUBJECTIVE SOCIAL STATUS *

MARY R. JACKMAN

University of Michigan

ROBERT W. JACKMAN

Michigan State University

American Sociological Review 1973, Vol. 38 (October):569-582

The pluralist and interest-group (or modified Marxian) views of society offer two competing sets of hypotheses concerning the relationship between objective and subjective social status and the role of other variables in this relationship. Using a 1964 national sample survey of the United States, this paper specifies and examines these hypotheses more fully. Starting with a series of simple formulations, and building up to a fuller multivariate recursive system, the paper concludes that the data are more consistent with the interest-group approach than they are with the pluralist approach.

MANY studies of subjective class identification in the United States have reported that the relation between objective and subjective status is far from perfect, and have sought to infer from this that the United States is a pluralist society, making Marxist predictions invalid (see, e.g., Jones, 1941: 348; Cantril, 1943; Rosenberg, 1953; Haer, 1957; Hodge and Treiman, 1968). However, the empirical reality of a less than perfect fit between objective and subjective social status does not in itself imply a pluralist society: a modified Marxian approach may be more in order. In this paper we use path analytic and analysis of covariance models to interpret and specify the relationship between objective social status and subjective class identification among the adult population of the United States. While such an assessment cannot provide evidence for or against a pure Marxist approach (because sample surveys invariably include few substantial capitalists), it does enable a direct evaluation of the rela-

tive utility of two competing bodies of research: on the one hand, the interest-group or modified Marxian approach (of which a good example is Centers' work [1949]), and on the other, the pluralist approach (of which good examples are Rosenberg [1953] and Hodge and Treiman [1968]).

I

The notion of subjective class identification, as we shall use it in this paper, refers to the individual's perception of his own position in the status hierarchy and is thus a "psychological phenomenon . . . , a feeling on [a person's] part of belongingness to something" (Centers, 1949:27). Rosenberg summarizes the characteristics of this feeling as follows:

The individual must identify himself with the class to which he belongs according to the objective definition; he must feel united with others in the same objective position; and he must feel separated from, or must disidentify with, people in a different objective class position. These cognitive factors represent elements of awareness. They are often viewed as overlaid with affect, leading to characteristics such as intra-class friendship and inter-class resentment and antagonism (Rosenberg, 1953:23).

Note the distinction between class identification on the one hand and class consciousness on the other. The former deals with perceived self-location in the socioeconomic structure; while the latter deals with a series of issues arising from this perceived self-location, such as the intensity of the percep-

* This paper is in every sense a joint effort and our names are listed alphabetically. We would like to thank Donald J. Treiman for giving us permission to use the data and for his helpful advice. We are also grateful to David L. Featherman, Robert M. Hauser, Douglas A. Hibbs, Russell Middleton and William H. Sewell for their valuable criticisms of an earlier draft. Any remaining shortcomings in the analysis or interpretation are the authors' sole responsibility. All data were made available through the Data and Program Library Service at the University of Wisconsin, Madison; and the research was supported in part by funds from the Research Committee of the Graduate School at Wisconsin and an All-University Research Grant from Michigan State University.

tion, and its effects on political orientations. This distinction is clear, for example, in the last sentence of the excerpt from Rosenberg, who suggests that class identification can be seen as "leading to" a number of other class-consciousness characteristics. In the following analysis, the focus is restricted to the relationship between objective socioeconomic status and class identification, thus defined.

Most analysts have suggested that feelings of class identification are neither developed nor crystallized, especially in the United States. A number of factors are seen as inhibiting such feelings, most of them stemming from the hypothesis that the United States is a pluralist society not polarized along any one dimension, especially an economic or social status dimension.

In the pluralist society individuals hold overlapping group memberships on the economic as well as other dimensions so that no one group can command a person's undivided loyalty. There is no single cleavage that divides society into distinctive subgroups; rather, a large number of cross-cutting cleavages prevents any particular group membership from assuming over-riding significance (see e.g., Coser, 1956:72-81; Lipset, 1960:88; Wilensky, 1970). Thus strong feelings of identification with others of the same socioeconomic status cannot develop because of other competing and equally salient affiliations that cut across the economic dimension.

This position was developed in response to the Marxist prediction that industrialized capitalist societies would become divided into distinct economic classes with a high degree of intra-class identification and inter-class hostility. A pure Marxist approach predicts that an individual's relationship to the means of production (owner vs. worker) defines his socioeconomic class. The interest-group approach (such as that proposed by Centers [1949]) derives from the Marxist position, but it involves a substantial modification of it: it retains the emphasis on the primary importance of the socioeconomic dimension, but it assigns weight to the differing interests of individuals within the non-owning class as well. Thus, the Marxist position sees the socioeconomic dimension as essentially broken into two categories on the basis of relationship to the means of production: in contrast, the interest-group approach

treats this dimension as more of a continuous variable, with variations in income and status being more salient than objective relationship to the means of production.

Evaluating the relative merits of the pluralist, Marxist, and interest-group positions essentially involves assessing the structure of the relationships among key independent variables as well as between each of these variables and subjective class identification. In the following analysis, we shall discuss the various expectations of these three positions concerning the status of the major explanatory variables. At times the last two lead to the same empirical predictions, but we shall note the discrepancies where they occur.

1. *Socioeconomic status.* The Marxian and interest-group positions assume that the various components of an individual's socioeconomic status—education, occupation, and income—are highly correlated; while the pluralist position assumes that they are poorly inter-related, making distinct class interests for each individual difficult to perceive (e.g., Hodge and Treiman, 1968). Furthermore, while the Marxian and interest-group theories hold that socioeconomic status is the prime determinant of class identification, with other variables (which we shall identify below) playing only a mediating role, the pluralist position maintains that these other variables are independent of socioeconomic status and act to weaken the relationship between objective and subjective social status.

2. *Social Contacts.* Kornhauser has outlined three aspects of class: (a) "the use of objective factors to define class in terms of economic position"; (b) "class in terms of social relationships and participation in formal and informal organizations"; and (c) "a subjective definition of class: a person belongs to a class if he feels himself a member of it" (1950:338). Perhaps the crucial difference between the Marxist and interest-group approaches on the one hand and the pluralist position on the other is in the role assigned to social relationships in the link between objective and subjective class. The former positions assume that in the urban industrial structure objective class is the salient dimension in dictating the pattern of individuals' social lives, and consequently

that people mix primarily with others of the same socioeconomic status. Such patterns of social contact in turn lead to psychological identification with the relevant (socioeconomic) group. Objective status is not sufficient in itself to bring about accurate subjective class identification; it is the sharing of a common experience with others of the same status that leads a "class in itself" to become a "class for itself." Marx's famous passage in *The Eighteenth Brumaire* underlines the importance he assigned to social relationships as a major intervening variable between objective and subjective class:

The small-holding peasants form a vast mass, the members of which live in similar conditions but without entering into manifold relations with one another. Their mode of production isolates them from one another instead of bringing them into mutual intercourse. . . . Each individual peasant family is almost self-sufficient; it itself directly produces the major part of its consumption and thus acquires its means of life more through exchange with nature than in intercourse with society. . . . In this way, the great mass of the French nation is formed by simple addition of homologous magnitudes, much as potatoes in a sack form a sack of potatoes. In so far as millions of families live under economic conditions of existence that separate their mode of life, their interests and their culture from those of the other classes, and put them in hostile opposition to the latter, they form a class. In so far as there is merely a local interconnection among these small-holding peasants, and the identity of their interests begets no community, no national bond and no political organization among them, they do not form a class (Marx, 1963: 123-4).

The pluralist position also maintains that social contacts are important in explaining subjective class identification; but in sharp contrast to the Marxian tradition, the pluralists assume that in capitalist American society other *overlapping* dimensions are just as salient as the class/status one in activating the patterns of social life (see e.g., Rosenberg, 1953; Case, 1955; Wilensky, 1970: 424). Thus an individual's friends and neighbors are likely to occupy an economic status different from his own, while they share other memberships and affiliations: when asked which social class he identifies with, an individual is as likely to use the objective status of his friends and neighbors as a framework

as he is to refer to his own status; and in many cases this will make his subjective choice appear "incorrect." For example, Hodge and Treiman (1968:547) conclude that

The major defect of the interest theory of classes . . . lies in its systematic neglect of the great range between-class contacts which are open to many citizens. . . . Such inter-class contact is one cornerstone of democracy, preventing the emergence of social issues which would pit group against group in a class struggle.

This argument rests heavily on the assumption that one's own status and the status of one's friends and social contacts are at best very weakly interrelated, an assumption that runs counter to results from some recent research (Laumann and Guttman, 1966:177).

3. *Capital Ownership.* As noted above, the interest-group position places little emphasis on relationship to the means of production as a factor in class identification; but both the Marxist and pluralist positions assume that capital ownership is an important factor in accounting for subjective class identification. However, the former posits a sharp cleavage between the owners of capital and the workers in industrial capitalist society with small businessmen being gradually driven out of business and into the ranks of the proletariat. By contrast, the pluralist approach maintains that many workers may own a small amount of capital in the form of rental property, stocks and bonds, savings bonds, or a small business and that this will confuse their subjective class identification. Illustrative of the pluralist position is Hartmann and Newcomb's (1939) hypothetical case of the high-school teacher living in

a strife-ridden community who hates labor unions and all they represent. Examining his total annual income, we find it derived from the following sources:

Yearly amount	Category	Origin
\$1,800	Wages (salary)	School budget, i.e. taxes
100	Rent	Payments by tenants on father's farm
100	Interest or Profit	Dividends, etc., on investments or savings

Economically, this teacher is properly classified as a laborer, for 90 percent of revenue comes from some definite service rendered under supervision. Nevertheless, he thinks as a member of the middle class hostile to the "encroachments" of other workers. Although no more than 10 percent of his purchasing power originates from the return upon his small inherited or accumulated capital, this amount is literally the tail that wags the dog. This phenomenon occurs with such frequency that we are compelled to acknowledge that the attitude here manifested must be traced to certain "levels of aspiration," i.e., ideals, standards and norms, which makes the values of the employer more impressive than those of the employee (Hartmann and Newcomb, 1939:196; see Hodge and Treiman, 1968:535 for a similar argument).

In other words, the pluralist argument anticipates (a) that capital ownership and self-employment are widely distributed across socioeconomic groups in the United States, and (b) that this is another circumstance preventing the crystallization of clearly perceptible class interests.

4. *Work Experience.* While work experience has not been an important variable in the pluralist position, the Marxian approach has assumed that the alienating work experience itself has a strong impact on the development of class identification among members of the working class. It holds that the relationship between objective and subjective class should be intensified for the family member who is the breadwinner, and even more for the trade union member. The family's main earner will have personally experienced the impact of the work situation, and will perceive his or her objective relationship to the means of production more clearly than will non-earners, such as unemployed housewives. Similarly, the union member will have had greater socialization into the interests of his work group and into the concept of collective action. The interest-group position has retained some emphasis on work experience and union membership as factors enhancing the development of class identification, but these factors appear to be less important than they would be in a pure Marxist argument. In addition, the interest-group sees the role of these factors more as socialization agencies rather than as involving any (alienating) dynamics imbedded in the objective structure of the work

situation itself. These agencies act to increase the individual's education into the interests of his socioeconomic status group rather than to increase his level of consciousness through personal experience as a non-owning worker.

5. *Ethnic Cleavages.* The major thrust of the Marxian tradition assigns prime importance to the class cleavage, and the recognition of cleavages along ethnic lines is generally attributed to Weber (1946). However, both Marx and Engels observed that ethnic subgroupings leading to distinctive subcultures could break up class groups, in the case of the Irish and English in England (Marx and Engels, 1971:292-5), the various ethnolinguistic groups in the United States, and blacks and whites in the United States (Marx and Engels, 1961:xiv, 280-1).

The pluralist position assumes that because all cleavages are cross-cutting, no single cleavage along ethnic, class, religious or any other dimension can become strong enough to create distinctive subcultures. With society cross-cut by a complex of overlapping group memberships, distinctive behavior by any particular group is not expected. Thus Rosenberg writes that "... one may perceive a member of one's own objective class as an enemy because one looks at him from the viewpoint of his religion, his race, his nationality, his residence, etc. Conversely, one may perceive a member of a different objective class as a friend because he has the same race, religion or nationality" (1953:25).

II

The primary data for this study come from a national area-probability sample of the non-institutionalized adult population of the United States (N=923) conducted by the National Opinion Research Center in the summer of 1964. Because the concept of class identification is usually discussed in terms of the non-agricultural occupational structure, all farmers have been omitted from the analysis.¹ The exclusion of farmers and of all observations with missing data leaves us with a sample of 754 respondents. Using the same data set, this analysis seeks to extend and revise some earlier work by

¹ The exclusion of farmers actually made little empirical difference to the results presented below.

Hodge and Treiman (1968), which is one of the few attempts in the literature to examine empirically the role of other variables in the relationship between objective socioeconomic status and subjective class identification. We are able to replicate two of the results with other data sets: the results of these replications are reported in the Appendix, along with a description of the data and measures that were used.

The dependent variable, subjective class identification, is based on replies to a structured question asking respondents to place themselves in one of five categories. Of the 918 persons responding to this question before farmers and missing data were removed, 2.2 percent identified with the upper class, 16.6 percent with the upper middle class, 44.0 percent with the middle class, 34.3 percent with the working class, and 2.3 percent with the lower class. Less than one percent denied the existence of classes when faced with this structured question (for fuller discussion on this item, along with comparisons with earlier research, see Hodge and Treiman, 1968: 535-6).

1. *Socioeconomic Status and Social Contacts.* The purpose of this part of the analysis is to assess the role of social contacts in the relationship between socioeconomic status and subjective class identification. To begin, we specify a simple recursive model which expresses class identification as a function of respondent's objective status characteristics, i.e., education, occupation, and income (see Figure 1). In line with recent research on socioeconomic attainment (e.g., Duncan, Featherman, and Duncan, 1972; Featherman, 1972), education is assumed to be temporally prior to occupation, and both of these variables are assumed prior to income. The equations for this model are:

$$X_2 = p_{21}X_1 + p_{2u}X_u \quad (1)$$

$$X_3 = p_{32}X_2 + p_{31}X_1 + p_{3v}X_v \quad (2)$$

$$X_{15} = p_{15,3}X_3 + p_{15,2}X_2 + p_{15,1}X_1 + p_{15,u}X_u \quad (3)$$

where X_1 is respondent's education, scored in eight categories ranging from no formal schooling to attendance at graduate or professional school; X_2 is main earner's occupation (Duncan 2-digit SEI scores); X_3 is family income, coded in units of \$1,000 and ranging from under \$1,000 to \$15,000 or

more; and X_{15} is class identification as defined above. Figure 2 presents an elaborated version of this model where the status of the respondent's social contacts is specified as an intervening variable. Thus it includes equations (1) and (2) as above, while equation (3) is replaced by the following two equations:

$$X_4 = p_{43}X_3 + p_{42}X_2 + p_{41}X_1 + p_{4w}X_w \quad (4)$$

$$X_{15} = p_{15,4}X_4 + p_{15,3}X_3 + p_{15,2}X_2 + p_{15,1}X_1 + p_{15,u}X_u \quad (5)$$

where all terms are defined as above, and X_4 is High Status Contacts, coded from 0 through 4. This variable is based on responses to the following two items: "Do you have any *friends* who are professionals (scored 1) or businessmen (scored 1)?" and "Do you have any *neighbors* who are professionals (scored 1) or businessmen (scored 1)?" The maximum score of 4 is achieved by those who responded positively to each of the four contingencies.²

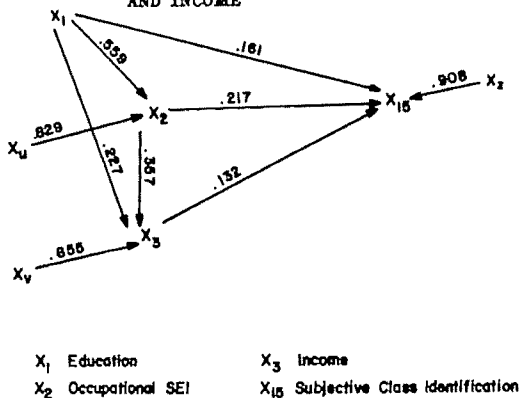
* Ideally, we would measure the social status of respondents' contacts by asking them to name the exact occupation of their friends and neighbors and then code these data according to some criterion such as the Duncan SEI (as is done in Laumann and Guttman, 1966). While our variable is not as precise as this, we feel it does index part of the variance that we are seeking to tap. We should also note that our measure of social contacts differs from Hodge and Treiman's (1968) in three respects:

(1) We have not included relatives in the measure on two grounds: first, we wanted to include contacts developed by the respondents themselves rather than ascriptive contacts with which they were born; and second, we assume that actual contact with friends and neighbors (measured, say, in hours per annum) is likely to be greater than contact with relatives, which may be quite minimal for many respondents. (However, use of a scale including relatives made no substantial difference to the results.)

(2) Hodge and Treiman scored their contacts variable by assigning a score of 1 both to those who could name friends who were professionals and friends who were businessmen, and to those who could name friends in only one of those categories, and the same scoring for relatives and neighbors to yield a scale ranging from 0 to 3; our scale tries to maximize variance by assigning one point each for professionals and businessmen.

(3) We have not included low status contacts in the model on the grounds that it is theoretically redundant (i.e., it is merely the obverse of the high status contacts variable) and empirically non-discriminating (as we scored it, its variance was

FIGURE 1: SUBJECTIVE CLASS IDENTIFICATION AS A FUNCTION OF EDUCATION, OCCUPATION, AND INCOME

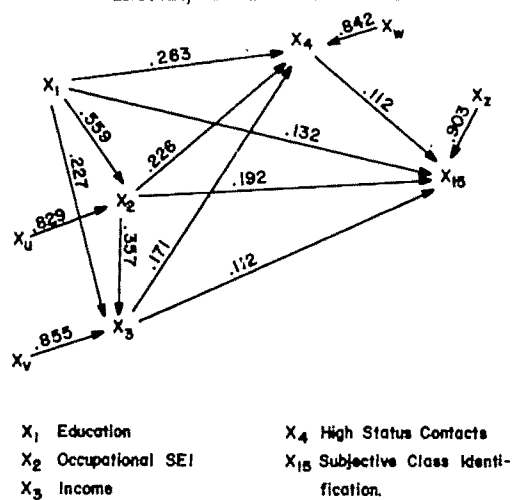


Source: NORC national prestige survey of the United States, 1964
 N=754 (farmers and missing data excluded)
 All coefficients are significant beyond the .05 level

According to the pluralist perspective, the addition of X₄ to the model should lead to a more accurate accounting of variance in subjective class identification, and thus equation (5) should have a higher R² than equation (3). The argument made from this viewpoint is that the far less than perfect correlation between objective and subjective status reflects (a) a weak relationship between the individual's objective status and that of his associates, and (b) his sensitivity to the status of his associates as well as to his own objective status in forming his class identification. In contrast, the interest-group approach would lead us to expect (a) that X₄ should be related substantially to X₁, X₂, and X₃, and (b) that X₄ should act to interpret at least part of the relationship between objective and subjective status rather than to add to the variance explained in the latter variable.

Comparing Figure 1 with Figure 2, we may observe that the addition of the status contacts variable does not substantially increase the amount of variance explained in subjective class identification (the R² increases from .176 to .185). Instead, the effect of elaborating the model to include the High Status Contacts variable is to inter-

FIGURE 2: SUBJECTIVE CLASS IDENTIFICATION AS A FUNCTION OF EDUCATION, OCCUPATION, INCOME, AND HIGH STATUS CONTACTS



Source: NORC national prestige survey of the United States, 1964
 N=754 (farmers and missing data excluded)
 All coefficients are significant beyond the .05 level

pret in part the basic relationship: that is to say, the contacts variable acts as an intervening factor. Our analysis indicates that one's likelihood of being able to name high status friends and neighbors increases fairly substantially with one's own objective social status, as the interest-group theory would lead us to expect (the multiple R for equation (4) is .54; R² = .29). Note that the amount of variance explained in X₄ by X₁, X₂, and X₃ is approximately the same as that explained in X₃ by X₁ and X₂, and in X₂ by X₁. Insofar as education, occupational status and income are generally discussed as various indicators of socioeconomic status, it would seem justifiable to regard our High Status Contacts variable as an indicator of the social aspect of socioeconomic status.³

³ Note that this particular measure of status of friends and neighbors is weak in a way that should work against the interest-group theory: the question is "do you have any friends who are . . ." rather than "what *proportion* of your friends are . . .". Even from an interest-group perspective it should be easier to think of *one* friend or neighbor who has a higher or lower occupational status than oneself, whereas, the second question would be far more sensitive to possible variations in the socioeconomic *composition* of one's friends and neighbors. Thus, the R² of .29 may be a conservative estimate.

about half that of the high status contacts variable and it was orthogonal to all the other variables in the model).

Figure 2 suggests further that part of the effect of one's objective socioeconomic status on subjective class identification is channeled through the status of one's social contacts, which is not inconsistent with the thrust of the Marxian argument that the social aspect of class plays a mediating role in the development of feelings of subjective class identification.

This interpretation of the data is at variance with that of Hodge and Treiman (1968). The basic difference stems from their emphasis on accounting for variance and attempting to calculate the relative importance of the independent variables: in contrast, the present analysis is concerned primarily with examining the causal structure of the relationships among the independent variables themselves as well as assessing their combined impact on the major dependent variable. Thus, Hodge and Treiman relied on an estimate of a multiple partial correlation of status contacts and class identification from a single equation including these variables and the respondent's own education, occupation, and income,⁴ from which they concluded that contacts have an independent impact on class identification that is just as strong as the impact of the objective status characteristics. However, as Fisher (1946:190, cited in Duncan, 1970: 42-3) points out, the problem with this kind of analysis is that "if . . . we choose a group of social phenomena with no antecedent knowledge [or theory] of the causation or absence of causation among them, then the calculation of correlation coefficients, total or partial, will not advance us a step towards evaluating the importance of the causes at work." As Duncan notes, "the contribution of path analysis . . . does not consist so much in rationalizing calculations of explained variance or in evaluating the 'relative importance' of variables as in making explicit the formulation of assumptions that must precede any such calculations if they are to yield intelligible results" (1970:46).

Indeed, our inference about the structure of the model is the most important one to be drawn from a comparison of Figures 1 and

2: i.e., that the status of one's social contacts is not independent of one's own objective status and that it acts to interpret partially the relationship between objective and subjective status. A replication of equations (1), (2) and (3) suggests that the absolute magnitudes of these coefficients are of lesser importance. Here, we used the 1968 Election Study of the Survey Research Center of the University of Michigan (which unfortunately does not include a social contacts variable comparable to the one described above, thus ruling out replication of equations (4) and (5)). Figure 6 in the Appendix presents the results: this replication provides a model quite similar to that displayed in Figure 1, except that with the SRC 1968 data, the R^2 for equation (3) is .30, where it is .18 with the main NORC data set.

2. *Capital Ownership*. There are six items in the data set asking about ownership of real estate, savings bonds, local/state/federal bonds, shares/stocks/bonds in a private company, patents/copyrights, and partnerships in private companies. Ideally, one would like to know how much money the respondent has invested in these various forms of capital ownership: in an attempt to compensate for this lack of information, we gave each of the items a weight of 1 and summed them to form a six-point scale (in fact, no one achieved a score greater than 5). We then added this capital ownership variable to the model as a function of education, occupation, and income, and as mediating between these variables and (a) social contacts, and (b) class identification. Thus the model retains equations (1) and (2); while equations (3), (4), and (5) are replaced by:

$$X_5 = p_{53}X_3 + p_{52}X_2 + p_{51}X_1 + p_{5t}X_t \quad (6)$$

$$X_4 = p_{45}X_5 + p_{43}X_3 + p_{42}X_2 + p_{41}X_1 + p_{4w}X_w \quad (7)$$

$$X_{15} = p_{15,4}X_4 + p_{15,5}X_5 + p_{15,3}X_3 + p_{15,2}X_2 + p_{15,1}X_1 + p_{15,w}X_w \quad (8)$$

where all terms are defined as before, and X_5 is capital ownership. It might be objected that capital ownership should precede X_1 , X_2 and X_3 —indeed, such an argument might be more justifiable for that small segment of the population that controls a disproportionate share of the national income via in-

⁴ Hodge and Treiman (1968:547) report a multiple partial correlation of .244 and reach their conclusion by comparing this figure with the multiple correlation (for the whole equation) of .494.

herited wealth. We defend our specification on the grounds that while national sample surveys may include one or two such individuals, our equations are more appropriate for the vast majority of respondents.

Figure 3 suggests that capital ownership is substantially dependent on an individual's education, occupation, and income: given the crudeness of the measure of capital ownership, the causal paths to this variable are reasonably strong (and the R^2 is .26), indicating that not everyone has equal access to capital. Note also that capital ownership mediates a small part of the effect of education, occupation, and income on the status of one's friends and neighbors. However, the direct path from capital ownership to class identification is small and non-significant ($p = .20$) although the zero-order correlation between these two variables is .251. This suggests that the bivariate relationship between capital ownership and class identification is partly spurious (due to the relationship of these two variables with the temporally prior education, occupation, and income) and partly mediated by the tem-

porally intermediate social contacts variable. Thus, as with the social contacts variable, the addition of capital ownership to the model does not increase the amount of variance explained in class identification, but it helps in interpreting the relationship between objective and subjective status.

An alternative but weaker index of capital ownership, self-employment, was also added to the model as a dummy variable. Contrary to strict Marxist expectations concerning advanced capitalist societies and in line with those of the pluralists, this variable is almost orthogonal to education, occupation, and income. However, it also has no effect on either the social contacts variable or class identification, contrary to both Marxian and pluralist predictions, which assume that the self-employed view themselves as belonging to the higher (if not upper) ends of the stratification system.

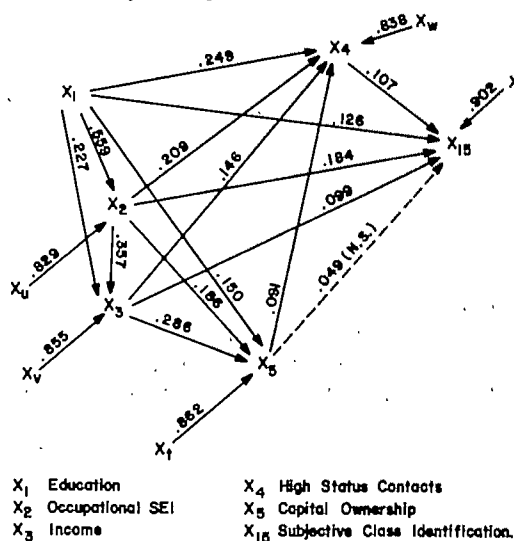
These results indicate that for mass samples capital ownership in the form of either ownership of stocks, bonds, etc., or self-employment does not have an independent impact on class identification. The capital ownership variable is quite strongly related to education, occupation, and income, and mediates a small part of the effect of these variables on social contacts; while the self-employment variable is virtually orthogonal to all the other variables in the model. This suggests that in accord with the interest-group position, relationship to the means of production is not nearly as important in channelling the dynamics of subjective class identification as are earned income and the status associated with occupation and education.

3. *Work Experience.* Since the interest-group tradition anticipates that the dynamics of class identification will be intensified by the work experience and the socialization into the group's interest by unions (which implies a statistical interaction), we constructed an analysis of covariance design to test whether either the experience of being the family's main earner or union membership increased the relationship between occupation and class identification.

We estimated the following equations:

$$X_{15} = a + b_1 X_2 + b_2 X_7 + b_3 (X_2 * X_7) + e_1 \quad (9)$$

FIGURE 3: SUBJECTIVE CLASS IDENTIFICATION AS A FUNCTION OF EDUCATION, OCCUPATION, INCOME, CAPITAL OWNERSHIP, AND HIGH STATUS CONTACTS



Source: NORC national prestige survey of the United States, 1964

N = 754 (farmers and missing data excluded)

All paths except for $P_{15,5}$ are significant beyond the .05 level

$$X_{15} = a + b_1 X_2 + b_2 X_8 + b_3 (X_2 * X_8) + e_2 \quad (10)$$

where all terms are defined as above, and X_7 is a dummy variable for main-earners and X_8 is a dummy for union membership. In equation (9) a gives the intercept for non-earners, while in (10) it gives the intercept for non-union members; b_1 gives the slope for non-earners and non-union members respectively; $(a + b_2)$ gives the intercept for main-earners and union main-earners respectively; and $(b_1 + b_3)$ gives the slope for main-earners and union members respectively (see Schuessler, 1969). The results of this analysis indicate no significant differences in either the intercept or the slope for either main-earners or union members.⁵ This suggests that the relationship between occupational status and subjective class identification is not intensified by work experience, as we have defined it here.⁶

4. *Ethnic Cleavages.* In this part of the analysis we employ an analysis of covariance design to ascertain whether ethnic group membership can imply any distinctive behavior in relation to the development of class identification. While there are clearly other ethnic groups in the United States, we have chosen to focus specifically on the black-white cleavage on the grounds that this represents the most salient ethnic cleavage in contemporary America. Noting the comments of Marx and Engels on the case of American blacks, we might expect that blacks and whites will not act as one

group in their personal reactions to objective socioeconomic status characteristics. Meanwhile, the pluralist position assumes that no cleavage can be strong enough to divide society into subgroups exhibiting distinctive behavior and that membership in an underprivileged ethnic group does not preclude participation in other groups: thus, for example, a black doctor may not hold such high class identification as a white doctor; but on the other hand he will not feel as low in class identification as a black laborer. This reasoning about the effect of cross-cutting cleavages implies that on the average, blacks will hold lower class identification than whites but that nevertheless blacks will react to the same stimuli as whites in developing their class identification. In terms of an analysis of covariance design, the pluralist approach assumes that there will be no significant interaction for, say, race, occupational status, and class identification, and that the slopes for blacks and whites will be parallel (with the black slope slightly below that for whites).

We estimated three equations as follows:

$$X_{15} = a + b_1 X_1 + b_2 X_9 + b_3 (X_1 * X_9) + e_3 \quad (11)$$

$$X_{15} = a + b_1 X_2 + b_2 X_9 + b_3 (X_2 * X_9) + e_4 \quad (12)$$

$$X_{15} = a + b_1 X_3 + b_2 X_9 + b_3 (X_3 * X_9) + e_5 \quad (13)$$

where all terms are defined as before and X_9 is a dummy variable scored 1 for blacks and zero otherwise (the interpretation of the coefficients is the same as in equations (9) and (10) above). Figure 4 presents the results of this analysis, which indicates a significant interaction effect for race, education and class identification, and race, occupation and class identification respectively, but not for race, income and class identification.⁷ While there is a moderately strong

⁵ Unstandardized regression coefficients for equation (9):

a	2.362	b_4	-0.019	($p = .85$)
b_1	0.011	b_5	0.002	($p = .26$)
$R^2 = .142$				

Unstandardized regression coefficients for equation (10):

a	2.300	b_2	0.032	($p = .78$)
b_1	0.012	b_3	-0.003	($p = .33$)
$R^2 = .140$				

Note that our results are consistent with Hodge and Treiman's (1968) conclusion that union membership has no independent impact on class identification: however, their conclusion was based on an empirical test for additive effects only.

⁶ Equations equivalent to (9) and (10) with high status contacts as the dependent variable yielded similar results (i.e., coefficients b_4 and b_5 were not significantly different from zero).

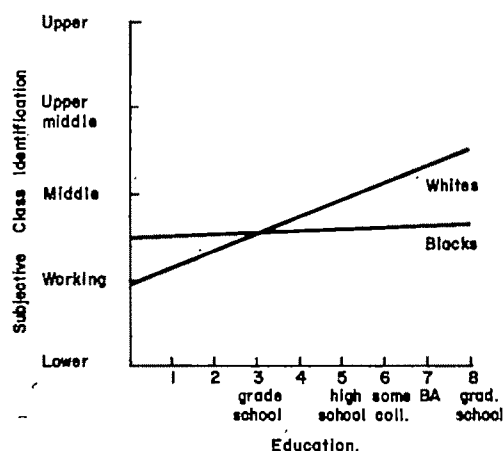
⁷ Unstandardized regression coefficients for equation (11):

a	1.947	b_2	0.644	($p < .01$)
b_1	0.195	b_3	-0.177	($p < .01$)
$R^2 = .135$				

Unstandardized regression coefficients for equation (12):

a	2.301	b_2	0.314	($p = .01$)
b_1	0.014	b_3	-0.012	($p < .01$)
$R^2 = .150$				

FIGURE 4(a): RELATIONSHIP BETWEEN EDUCATION AND SUBJECTIVE CLASS IDENTIFICATION FOR WHITES AND BLACKS



Source: NORC national prestige survey of the United States, 1964
N = 754 (farmers and missing data excluded)

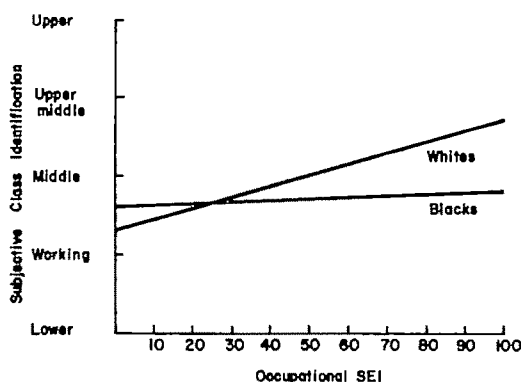
slope for education and occupation respectively and class identification for whites, the class identification of blacks is not at all sensitive to education and occupational status. However, the relationship between income and class identification appears to be of the same form for both whites and blacks. These results suggest that the low prestige accruing from ascribed ethnic status is of such overwhelming salience to blacks that additional prestige accruing from universalistic achieved criteria such as education and occupational status cannot make an impact on the ethnic group member's class identification. By contrast, a more readily visible stimulus, such as income (which can be visibly expressed in differing patterns of consumption) seems to affect the class identifica-

Unstandardized regression coefficients for equation (13):

a	2.405	b_2	0.159 (p = .28)
b_1	0.072	b_3	-0.049 (p = .08)
R^2	.098		

Note that the distribution of blacks on education, occupation, and income is approximately the same: there are 111 blacks in this analysis. tion of blacks in the same way as it does that of whites.⁸

FIGURE 4(b): RELATIONSHIP BETWEEN OCCUPATIONAL STATUS AND SUBJECTIVE CLASS IDENTIFICATION FOR WHITES AND BLACKS

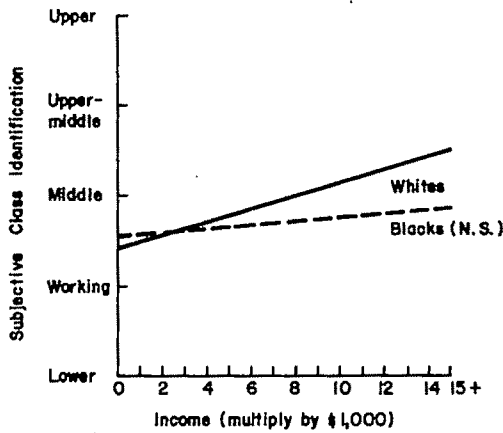


Source: NORC national prestige survey of the United States, 1964
N = 754 (farmers and missing data excluded)

However, before we accept this interpretation we proceed to a replication of the models presented in Figure 4 with two other data sets: the 1968 Survey Research Center Election Study data used above, and the Selznick-Steinberg (1969) study of anti-Semitism in the United States collected by NORC in 1964 (see the Appendix). The equations estimated in this replication are (11), (12) and (13), where all variables are defined as above. The results presented in the Appendix indicate that the parameter estimates for the three interaction terms are all statistically significant and of the predicted sign for the 1968 SRC data. For the Selznick-Steinberg data we observe the same pattern as we did with the main data set above: i.e., in both NORC samples there is a significant interaction effect for race, education and class identification, and race, occupation and class identification respectively, but not for race, income and class identification. The fact that the estimates for the adjustments to the intercepts (b_2 in equations (12) and (13) with the SRC data, and b_2

⁸ Analysis of covariance models equivalent to equations (11), (12), and (13) were constructed with high status contacts as the dependent variable: neither the intercept nor the slope were significantly different than those for whites.

FIGURE 4(c): RELATIONSHIP BETWEEN INCOME AND SUBJECTIVE CLASS IDENTIFICATION FOR WHITES AND BLACKS



Source: NORC national prestige survey of the United States, 1964
N=754 (farmers and missing data excluded)

in equation (11) with the Selznick-Steinberg data) are not significantly different from zero is of no real concern given our fundamental interest in the behavior of the estimates for the adjustments to the slopes (i.e., the interactions).

This replication provides further support for the interpretation presented above concerning the race-education and race-occupation interactions. At the same time, it leads us to modify our conclusion about the differences between these two interactions on the one hand, and the race-income interaction on the other. That is, on the basis of both the main analysis and the replications, it seems more appropriate to argue that the low prestige accruing from ascriptive ethnic status is of such overwhelming salience to blacks that any additional prestige they may gain from more universalistic, achieved criteria such as education, occupational attainment, and income has at best a negligible impact on their self-location in the socio-economic structure.

III

In this section we attempt to incorporate our results into a single, more complete model and to assess the implications of this model for the literature on class identification. The equations for the model are:

$$X_1 = p_{19}X_9 + p_{1q}X_q \quad (14)$$

$$X_2 = p_{29}X_9 + p_{21}X_1 + p_{2u}X_u \quad (15)$$

$$X_3 = p_{39}X_9 + p_{31}X_1 + p_{32}X_2 + p_{3v}X_v \quad (16)$$

$$X_5 = p_{59}X_9 + p_{51}X_1 + p_{52}X_2 + p_{58}X_8 + p_{5t}X_t \quad (17)$$

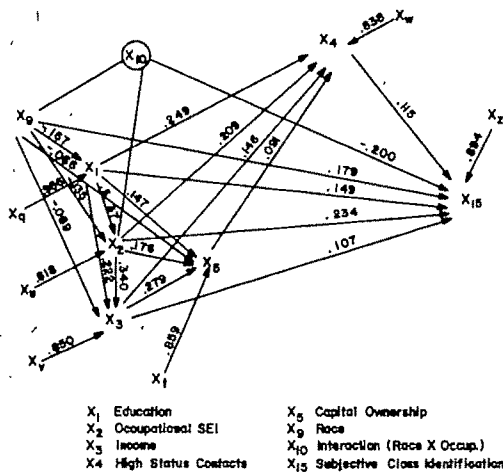
$$X_4 = p_{41}X_1 + p_{42}X_2 + p_{48}X_8 + p_{45}X_5 + p_{4w}X_w \quad (18)$$

$$X_{15} = p_{15,9}X_9 + p_{15,1}X_1 + p_{15,2}X_2 + p_{15,8}X_8 + p_{15,10}X_{10} + p_{15,s}X_s \quad (19)$$

where all terms are defined as before, and X_{10} represents an interaction term (Black \times Occupational SEI). In line with our earlier results, the dummies for self-employment, main-earner, union-membership, and the various interaction terms associated with each of these were omitted from the final model since their effects were non-significant. To simplify the model, the interaction term (Black \times Education) was omitted, since this and the Black \times Occupation interaction term are regarded as theoretically equivalent for our purposes. The interpretation of the paths from X_2 , X_9 and X_{10} to X_{15} is as follows: $p_{15,9}$ gives the standardized slope for whites of class identification on occupation; $p_{15,8}$ gives the adjustment to the intercept for blacks in class identification (in a standardized system this adjustment is, of course, a deviation from zero); and $p_{15,10}$ gives the adjustment to the slope for blacks of class identification on occupation (thus the slope for blacks is $[\.23 - .20] = .03$).

Race is assumed to be temporally prior to all the other variables in the model since it is an ascriptive characteristic. There is no direct path from race to high status contacts (in line with the results from our analysis of covariance models), implying that the effect of race on the status of social contacts is completely mediated by education, occupation, income, and capital ownership. In line with our earlier results, a direct path from capital ownership to class identification is also omitted. The addition of capital ownership to the equation estimating the status of social contacts increases the R^2 by only one percent (from .29 to .30), the addition of race and the race-occupation interaction term to the equation estimating class identification increases the R^2 by only two percent, and the inclusion of the social contacts variable in the model adds less than one per-

FIGURE 5: AN INTERPRETATION OF THE RELATION BETWEEN OBJECTIVE AND SUBJECTIVE SOCIAL STATUS



Source: NORC national prestige survey of the United States, 1964
 N=754 (farmers and missing data excluded)
 All coefficients are significant beyond the .05 level

cent to the variance explained in class identification. However, as we noted earlier, maximizing explained variance is of secondary importance: more importantly, the addition of these variables to the model gives a more complete understanding of the structure and dynamics of the causal processes at work.

We can draw the following main conclusions about our results summarized in Figure 5. First, contrary to strict Marxist expectations, experiences associated with relationship to the means of production (as indexed by the self-employment, main-earner and union-membership variables) do not appear to be salient in the formation of subjective class identification in the United States. Capital ownership in the form of stocks, bonds, etc., has no *direct* impact on class identification once the other temporally prior and mediating variables have been entered into the model. These two results lead us to interpret the impact of capital ownership on class identification as arising from the additional wealth and prestige accruing from it rather than from any dynamics associated with relationship to the means of production

itself. This interpretation is congruent with the interest-group theory, as is the observed covariance between capital ownership and objective status characteristics.

Second, our results suggest that groups that have been assigned low prestige on the basis of non-economic, ascriptive criteria (in our analysis, American blacks) will not be sensitive to variations in their prestige resulting from achieved statuses. For such groups, the prestige of the ascriptive status appears to assume such overwhelming salience that prestige resulting from achieved statuses takes on relatively minor significance in the individual's self-location in the socioeconomic structure (we may note that this evidence of distinctive sub-group behavior runs contrary to pluralist expectations that in American society no single cleavage will be powerful enough to break one group off from the rest of society in its perceptions of its relationship to the socioeconomic structure).

Finally, if we replace the strict Marxist expectation that class identification is a function of relationship to the means of production with the more generalized assumption of the interest-group approach that social class awareness develops from general socioeconomic prestige and income, we are able to reject the pluralist argument that overlapping group memberships on several dimensions prevent socioeconomic status from becoming a constraining factor on people's social lives. Our results indicate that the likelihood of being able to name high-status friends and neighbors increases as a positive function of one's own socioeconomic status, and further, that the status of one's social contacts acts to mediate in part the relationship between one's own objective socioeconomic status and one's subjective class identification. Thus, in line with Marxian and interest-group expectations, the socioeconomic status of the individual has implications for the status of the friends and neighbors with whom he mixes; and in turn this social milieu partly channels the impact of his objective status on his development of a sense of class identification.

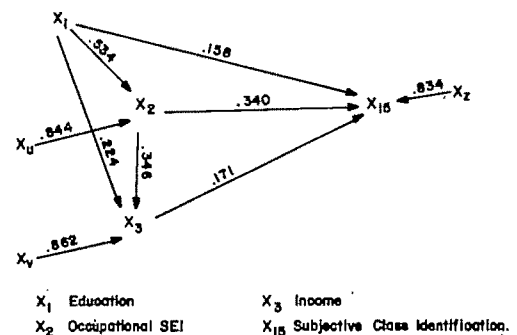
REFERENCES

- Cantril, Hadley
1943 "Identification with social and economic class." *Journal of Abnormal and Social Psychology* 38:74-80.
- Case, Herman M.
1955 "Marxian implications of Centers' interest-group theory: a critical appraisal." *Social Forces* 33(March):254-8.
- Centers, Richard
1949 *The Psychology of Social Classes*. Princeton: Princeton University Press.
- Coser, Lewis A.
1956 *The Functions of Social Conflict*. Glencoe, Ill.: Free Press.
- Duncan, Otis Dudley
1961 "Properties and characteristics of the socioeconomic index." Pp. 263-75 in Albert J. Reiss, et al., *Occupations and Social Status*. New York: Free Press.
- 1970 "Partials, partitions, and paths." Pp. 38-47 in Edgar F. Borgatta and George W. Bohrnstedt (eds.), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Duncan, Otis Dudley, David L. Featherman and Beverly Duncan
1972 *Socioeconomic Background and Achievement*. New York: Seminar Press.
- Featherman, David L.
1972 "Achievement orientations and socioeconomic career attainments." *American Sociological Review* 37(April):131-43.
- Fisher, R. A.
1946 *Statistical Methods for Research Workers*, Tenth Edition. Edinburgh: Oliver and Boyd.
- Haer, John L.
1957 "An empirical study of social class awareness." *Social Forces* 36(December):117-21.
- Hartman, George W. and Theodore Newcomb (eds.)
1939 *Industrial Conflict: a Psychological Interpretation*. New York: Gordon Company.
- Hodge, Robert W. and Donald J. Treiman
1968 "Class identification in the United States." *American Journal of Sociology* 73(March): 535-47.
- Jones, Alfred Winslow
1941 *Life, Liberty, and Property*. Philadelphia: J. B. Lippincott Company.
- Kornhauser, Arthur
1950 "Public opinion and social class." *American Journal of Sociology* 55(January):333-45.
- Laumann, Edward O. and Louis Guttman
1966 "The relative associational contiguity of occupations in an urban setting." *American Sociological Review* 31(April):169-78.
- Lipset, Seymour Martin
1960 *Political Man*. London: Heinemann.
- Marx, Karl
1963 *The Eighteenth Brumaire of Louis Bonaparte*. New York: International Publishers.
- Marx, Karl and Frederick Engels.
1971 *On Ireland*. London: Lawrence and Wishart.
- 1961 *The Civil War in the United States*. New York: International Publishers.
- Rosenberg, Morris
1953 "Perceptual obstacles to class consciousness." *Social Forces* 32(October):22-7.
- Schuessler, Karl F.
1969 "Covariance analysis in sociological research." Pp. 219-44 in Edgar F. Borgatta (ed.), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Selznick, Gertrude and Stephen Steinberg
1969 *The Tenacity of Prejudice: Anti-Semitism in Contemporary America*. New York: Harper.
- Weber, Max
1946 "Class, status, party." Pp. 180-95 in Hans H. Gerth and C. Wright Mills (eds.), *From Max Weber: Essays in Sociology*. New York: Oxford University Press.
- Wilensky, Harold L.
1970 "Class, class consciousness, and American workers." Pp. 423-37 in Maurice Zeitlin (ed.), *American Society*, Inc. Chicago: Markham.

APPENDIX: REPLICATIONS: DATA AND RESULTS

The data used for the replications reported in the text come from two area probability samples of the United States.

FIGURE 6: REPLICATION OF CLASS IDENTIFICATION AS A FUNCTION OF EDUCATION, OCCUPATION, AND INCOME



Source: SRC election study survey of the United States, 1968
 N=1051 (farmers and missing data excluded)
 All coefficients are significant beyond the .05 level

A. The first of these is the 1968 Election Study carried out by the Survey Research Center of the University of Michigan (total N=1557) which is available through the Inter-University Consortium for Political Research. In this sam-

ple, respondents' class identification was determined by asking the following questions:

There's been some talk these days about different social classes. Most people say they belong either to the middle class or to the working class. Do you ever think of yourself as belonging to one of these classes?

Those who answered "yes" ($N=940$) were then asked which class they identified with, while those who said "no" or "don't know" were asked:

Well, if you had to make a choice, would you call yourself middle class or working class? [IF MIDDLE CLASS] Would you say you are about average middle class, or that you are in the upper part of the middle class? [IF WORKING CLASS] Would you say that you are about average working class, or that you are in the upper part of the working class?

Of the 1,498 people identifying with a social class, the distribution was as follows:

Score	Category	Marginals
5	Upper middle	11.0
4	Middle	33.7
3	Upper working	9.5
2	Working	45.5
1	Lower	0.3
		100.0%

For the 1968 data, respondent's education is measured as a continuous variable ranging from zero to twenty years of education. Main earner's occupation is scored according to the detailed Duncan 2-digit SEI scores, and family income is coded in units of \$1,000 ranging from less than \$1,000 to \$25,000 or more. A dummy variable was set equal to 1 if the respondent is black, and zero otherwise. The exclusion of farmers and all observations with missing data leaves us with a sample of 1,051, 89 of whom are black.

B. The second data set comes from Selznick and Steinberg's study of anti-Semitism in the United States (total $N=1975$), collected by NORC in 1964—details on these data are presented in Selznick and Steinberg (1969). Respondent's class identification was determined by asking the following question:

By and large, do you think of yourself as being of the upper class, upper middle class, middle class, working class, or lower class?

Of the 1,931 people identifying with a social class, the distribution was as follows:

Score	Category	Marginals
5	Upper	2.4
4	Upper middle	11.1
3	Middle	46.3
2	Working	37.8
1	Lower	2.4
		100.0%

With this data set, respondent's education is measured as a continuous variable ranging from zero to seventeen years of education. Main earner's occupation is scored according to the abbreviated Duncan SEI for major occupational groups (Duncan, 1961:151), and family income is coded in units of \$1,000 ranging from less than \$1,000 to more than \$25,000. A dummy variable was set equal to 1 if the respondent is black, and zero otherwise. The exclusion of farmers and all observations with missing data leaves us with a sample of 1,532, of whom 190 are black.

Results. The replication of the model representing class identification as a function of education, occupation, and income (i.e., equations (1), (2) and (3)) with the 1968 data is presented in Figure 6. Turning to the three race interaction models (i.e., equations (11), (12) and (13)), the unstandardized regression coefficients are as follows:

Equation	Estimates			
(11)	a	1.371	b_2	0.762 ($p = .03$)
	b_1	0.152	b_3	-0.117 ($p < .01$)
	$R^2 = .191$			
(12)	a	2.089	b_2	0.208 ($p = .25$)
	b_1	0.023	b_3	-0.016 ($p < .01$)
	$R^2 = .264$			
(13)	a	2.401	b_2	-0.095 ($p = .60$)
	b_1	0.090	b_3	-0.058 ($p = .02$)
	$R^2 = .173$			

The unstandardized regression coefficients estimated with the Selznick-Steinberg data are:

Equation	Estimates			
(11)	a	1.797	b_1	0.155 ($p = .38$)
	b_1	0.087	b_2	-0.034 ($p < .05$)
	$R^2 = .126$			
(12)	a	2.207	b_1	0.200 ($p < .05$)
	b_1	0.014	b_2	-0.012 ($p < .01$)
	$R^2 = .127$			
(13)	a	2.406	b_1	-0.172 ($p = .07$)
	b_1	0.055	b_2	0.018 ($p = .40$)
	$R^2 = .097$			

A THEORY OF MIDDLEMAN MINORITIES*

EDNA BONACICH

University of California, Riverside

American Sociological Review 1973, Vol. 38 (October):583-594

Starting with the concept of "middleman minorities" developed by Blalock (1967:79-84), encompassing such groups as the Chinese in Southeast Asia, Jews in Europe, and Indians in East Africa, this paper presents a model which tries to explain the development and persistence of the form. A key variable is the orientation of immigrants towards their place of residence, with sojourning at first, and later a "stranger" orientation affecting the solidarity and economic activity of the ethnic group. These in turn arouse the hostility of the host society, which perpetuates a reluctance to assimilate completely, or "stranger" status.

RELATIONS between groups of different race or ethnicity have taken a variety of forms. One role an ethnic group can play is that of a "middleman minority" (Blalock, 1967:79-84). Although the form has not been precisely defined, nor clearly labelled (other appellations include "middleman trading peoples," Becker, 1956:225-37, "migrant intermediation," Schermerhorn, 1970:74-6, "marginal trading peoples," and "permanent minorities," Stryker, 1959) there is a general consensus that a number of ethnic groups around the world have occupied a similar position in the social structure. Among these are the Jews in Europe (perhaps the epitome of the form), the Chinese in Southeast Asia, Asians in East Africa, Armenians in Turkey, Syrians in West Africa, Parsis in India, Japanese and Greeks in the United States, and so on. The parallel between such groups has been noted not only by social scientists, but in some instances by the people among whom they live, as shown in such designations as "the Jews of the East," and "the Jews of India."

One of the principal peculiarities of these groups is the economic role they play. In contrast to most ethnic minorities, they occupy an intermediate rather than low-status position. They tend to concentrate in certain occupations, notably trade and commerce, but also other "middleman" lines such as

agent, labor contractor, rent collector, money lender, and broker. They play the role of middleman between producer and consumer, employer and employee, owner and renter, elite and masses. This accounts for Blalock's name for them; and although I shall question its applicability during the course of this paper, for ease of reference, I shall continue to use it.

The literature is not unanimous on the causes of this form. Most writers take the role as "given" and concentrate on the consequences of playing it. However, two prominent themes recur. The first sees the source of the pattern in the hostile reaction of the surrounding society to the cultural (including religious) and/or racial distinctiveness of these groups. They are pushed out of desirable occupations and forced to make a living in marginal lines. That they manage to escape the lowest rungs of the economic order, and sometimes acquire considerable wealth, is explained by their response to discrimination: a closing of ranks, the formation of solidary communities with considerable pride in group membership, and a special exertion to overcome handicaps (e.g. Kurokawa, 1970:131-3).

A second theme stresses the nature of the societies in which middleman groups are found. These are characterized by a "status gap" or marked division between elites and masses (Rinder, 1958-9). Examples include feudal societies with a gap between peasantry and landed aristocracy, or colonial societies with a gap between representatives of the imperial power and the "natives." Distinct ethnic minorities are seen to serve a number

* I am indebted to Ivan Light for many valuable exchanges on this topic. In addition, thanks go to Robert Goodman, Leo Kuper, Barbara Laslett, David McElroy, John Modell, Martin Orans, and Pierre van den Berghe, for reading and commenting on an earlier draft.

of functions in such societies.¹ First, since they are not involved in the status hang-ups of the surrounding society, they are free to trade or deal with anyone. In contrast, elites may feel that they lose status by dealing with the "masses" (Rinder, 1958-9:254). Second, their foreignness enables them to be "objective" in the marketplace; they do not have familistic ties with the rest of the society which can intrude on, and destroy business (Park, 1939:14). And third, they act as a buffer for elites, bearing the brunt of mass hostility because they deal directly with the latter. In a word, middleman minorities plug the status gap between elites and masses, acting as middlemen between the two.

This paper will present an alternative approach to middleman minorities. It develops a model which incorporates some of these ideas, but as part of a larger framework. The prevalent themes are found to be inadequate for two chief reasons. First, discrimination and hostility against minorities usually has the effect of hurting group solidarity and pride, driving a group to the bottom rather than the middle of the social structure. How then can we explain the closing of ranks reaction of these particular groups, and their peculiar ability to create success out of hatred? (Or to cite cases, why Japanese Americans been able to overcome racism, while Blacks have not?)

Second, the argument that middleman minorities arise in response to functional requisites may have merit. But it is clear these groups persist beyond the status gap. One finds them in post-colonial societies, after the elites have gone (e.g. the Chinese in Southeast Asia, Asians in East Africa, Parsis in India). And one finds them in modern industrial societies (e.g. the Indians in Britain, Jews in 20th century Germany, Chinese in New Zealand, Japanese in the United States).

¹ Note that Blalock extends the concept to include groups of mixed ancestry such as the Eurasians of Indonesia and mestizos of Brazil. This is an unusual usage, most writers reserving the comparison to distinct immigrant minorities. Clearly the "functional" advantages of foreignness are lost in the case of mixed-bloods. In addition, there is little evidence that Eurasians, for example, have concentrated in "middleman" occupations like trade (see van der Kroef, 1953:486-7).

Figure 1. Schematic Representation of the Development and Perpetuation of the Middleman Minority Position

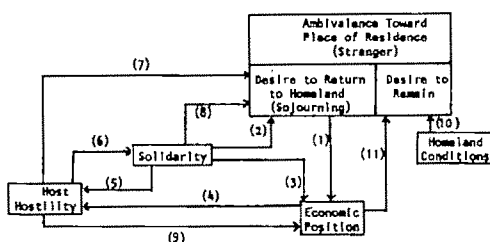


Figure 1 outlines the relationship between the major variables of our theory. We shall follow the links of this diagram through the remainder of the paper.

The Effects of Sojourning

An empirical generalization can be formed about all the middleman groups we have examined: they begin as sojourners in the territories to which they move. They are immigrants who do not plan to settle permanently. In contrast, other ethnic minorities include indigenous peoples of colonized territories, and immigrants who are forced to sever ties with a homeland (e.g. Blacks in the new world) or choose to do so (e.g. most of the "old" European immigration to the United States, see Garis, 1927:204). While individuals may vary in initial orientation, it appears there is a general "group" orientation, undoubtedly a product of conditions in the country of emigration. Thus Loewen (1971:26-7) generalizes about an entire group:

The early Chinese in Mississippi were not true immigrants, intending to become permanent settlers in a new homeland, but were sojourners, temporary residents in a strange country, planning to return to their homeland when their task was accomplished.

Statements of this sort abound for other middleman groups (e.g. Cator, 1936:55 on the Chinese in Indonesia; Miyamoto, 1939: 85 on the Japanese in America; Mahajani, 1960:xix on the Indians in Malaya and Burma; Dotson and Dotson, 1967:81-2 on Indians in Central Africa).

The orientation of groups like Jews, Armenians, and Parsis is a bit more complicated. Certainly Jewish immigrants to the United States had no plan to return to East-

ern Europe. But in all three cases there is an unusual attachment to an ancestral homeland; in the Jewish case, to Palestine. Stryker (1959:350) refers to the "continued attachment of the Parsis for their ancestral Persian home," for example. These groups are at a later stage in the cycle described by Figure 1, and their orientation toward place of residence will be taken up again later.

Sojourning is not a sufficient condition of the middleman form in that there are sojourners who do not become middlemen; but it is a necessary one, with important economic and social consequences directly related to the pattern. The economic effects (link 1) included a tendency toward thrift, and a concentration in certain occupations.

Thrift is the product of a willingness to suffer short-term deprivation to hasten the long-term objective of returning to the homeland. It is shown in excessively long hours of work, an emphasis on saving (often sending part of these savings to the homeland), and very little time or money spent on consumption. Sojourners are there to make money, not spend it, and this "future time orientation" enables them to accumulate capital. This orientation contrasts with that of settlers and "natives," who generally wish to live more rounded lives since they do not aim to live elsewhere.

Sojourning also leads the individual to select occupations which do not tie him to the territory for long periods. The sojourner wants a portable or easily liquidated livelihood. "Middleman" occupations (in the economic sense) have this characteristic, as Hoselitz points out (1963:23-4):

[T]he capital employed by a trader or money lender turns over much faster than that used in industrial establishments. A trader may carry on his business without ever attaining property rights in the objects he deals with. If he is a broker or commission agent, he may merely lose his earnings from a transaction, but not the capital invested in it. Moreover, a money lender or banker deals in that commodity that has the widest currency, that is accepted by anyone, that can easily be transported or hidden. . . . An industrial entrepreneur usually has more property tied up in his plant for a longer time than either merchant or banker.

Middleman minorities are noteworthy for

their absence from industrial entrepreneurship and investment in the kind of agriculture (e.g. cattle raising, orchards) that ties up capital. But there are other easily liquidated or transportable occupations besides trade, and these are also found among so-called "middleman" groups. Among them are the independent professions, prevalent among American Jews (Glazer and Moynihan, 1963:147); truck farming specializing in crops that have a rapid turnover, found among such diverse groups as the Chinese in New Zealand (Fong, 1959:85); Indians in Natal (Dept. of Economics, 1961:Chapter 2), and the Japanese in California (Ichihashi, 1932:178-206); and various skilled trades, such as barbers, shoemakers, goldsmiths, jewellers, restaurant-owners, tailors, launderers, and the like, found among Armenians in Syria (Sanjian, 1965:52), Jews in Poland (Eitzen, 1971:123), and Chinese and Japanese in this country. In other words, the term "middleman minorities" is really a misnomer. The more general occupational characteristic of these groups is liquidity.²

The Chinese in Southeast Asia illustrate the effect of sojourning on occupational preference:

[T]he intent of every emigrant was to work abroad in order to remit to China sums of money that would enrich his lineage when invested in land in his home village. He also intended to return home to enjoy during his old age the fruits of his arduous labours in exile. Although only a small proportion ever succeeded in this ambition, nevertheless the desire to return to China motivated the emigrants, at least at the outset, to enter a profession that would involve a minimum of fixed investment in the host country, and a maximum of liquid assets that could be returned to China (Wilmott, 1966:254).

That many sojourners do not achieve their goal of return is important, and will be dealt with later.

The principal non-economic result of sojourning is a high degree of internal solidar-

² Determining the liquidity or transportability of a particular occupation may, in fact, be problematic. Undoubtedly an occupational label can encompass a variety of firms which differ in the degree to which they tie up capital. However, there is a gross difference between professionals and skilled artisans who can carry their source of income with them, and a factory owner with a major investment in heavy machinery.

ity (link 2). Since they plan to return, sojourners have little reason to develop lasting relationships with members of the surrounding host society. But they have every reason to keep deeply alive the regional and broader ethnic tie, for these relationships will persist in the future towards which the sojourner points (see Siu, 1952, for an explication of this point). Thus ethnic and regional associations are strong, mutual assistance is prevalent, and trust retained among members from the same general area. (This is not to say that sojourner communities are completely unified. On the contrary, they are often riddled with division and conflict, based on regional, linguistic, political or religious differences found in the homeland. But in relation to the host society, these differences fade before an overriding "national" unity).

In contrast, settlers have much less reason to perpetuate such ties. As Garis (1927:204) points out for the "old" immigration to the United States:

Even though those already here objected at times to others coming in, yet once in they soon become Americans, so assimilated as to be indistinguishable from the natives; for this old immigration has consisted almost wholly of families who have come to this country with the full intention of making it their home.

Ethnic enclaves may develop among settlers out of convenience of common language; but the community tie tends to be much weaker and more likely to dissipate over time. The settler has reason to orient outwards and mix with his neighbors, while the sojourner "has no desire for full participation in the community life of his adopted land" (Siu, 1952:36).

Middleman minorities typically evince the following traits: a resistance to out-marriage, residential self-segregation, the establishment of language and cultural schools for their children, the maintenance of distinctive cultural traits (including, often, a distinctive religion), and a tendency to avoid involvement in local politics except in affairs that directly affect their group. They form highly organized communities which resist assimilation. These features, I contend, are related to an orientation toward a homeland.

Communal solidarity plays an important

role in the economic position of middleman groups (link 3). Family, regional, dialect, sect, and ultimately ethnic ties are used for preferential economic treatment. The "primordial tie" of blood provides a basis for trust, and is reinforced by multi-purpose formal and informal associations. Solidarity is interjected into economic affairs in two ways: it plays a part in the efficient distribution of resources, and helps to control internal competition. Resources distributed within the ethnic community include capital (through the use of partnerships, low interest loans, and rotating credit associations; see Desai, 1963:44-5 on Indians in Britain, and Light, 1972:23-30 on Chinese and Japanese in the United States), credit and easier terms to purchasers, information and training, and jobs and labor.

Let us concentrate on one area, the distribution of jobs and labor, for illustrative purposes. The typical middleman minority business is a family store (or truck farm), resting heavily on the use of unpaid family labor. If wage labor is needed, members of the extended family or of regional associations are preferred, and are treated like kin, sometimes living with the family behind the store. Employees work excessively long hours for low or no wages and are loyal to the owners. In exchange they are likely to become partners or to receive training and aid in setting up their own business in the same line. The middleman firm is labor-intensive but able to cut labor costs drastically through ethnically-based paternalism and thrift.

The effect of such arrangements on labor costs is shown for Japanese American laundries in San Francisco in 1909 (see Table 1). Not only did employees of Japanese firms work longer hours at less pay, but white laundry owners had to deal with a unionized work force.

The epitome of efficient distribution of resources is found in the vertical organization of a particular line, where one set of firms feeds another, within the ethnic community. Such was found in the clothing business among Jews in New York, where Jewish manufacturers sold to Jewish wholesalers who used Jewish retail outlets. Vertical integration exists between Indian wholesaler and retail grocers in Britain (Desai,

Table 1. Comparison of Labor Conditions in White, French, and Japanese Laundries, in San Francisco, 1909^a

	White	French	Japanese
Hours per week	49	50-63	60-72
Average wage with board and lodging per month	--	\$37.69	\$28.90 ^b
Average wage without board and lodging per month	\$69.74	\$48.56	--

^aAdapted from Millis (1915:65-6).

^bEstimated cost of room and board for Japanese owners was \$8 to \$10 per month.

1963:57), and between Japanese growers, wholesalers and retailers of fruits and vegetables in Southern California before World War II (Bloom and Riemer, 1949:92-6). Such arrangements are common among middleman minorities, and depend, in part, on the easier extension of credit within the group.³

Internal competition is sometimes controlled by the development of guild-like structures. Light (1972:68-70) describes a shoemakers' guild among Japanese Americans in the early part of this century which determined prices and controlled the location of shops, among other things. In the 18th century, Constantinople had seventy-

³ Efficient distribution of resources plus thrift have enabled middleman groups to avoid a pattern often found among disprivileged ethnic groups, i.e. debt peonage. In the early development of farming among the Japanese in California, for example, loans and credit were easily granted by suppliers of equipment, distributors of Japanese produce, and land owners, all of whom were non-Japanese (Bloom and Riemer, 1949:74). Similar arrangements have been found among black cotton farmers in the deep south (Davis, Gardner and Gardner, 1941:Chapter 15), and among the natives of Burma (Furnivall, 1956:293-4); undoubtedly crop mortgage is common around the world. Blacks and Burmans soon fell heavily into debt, the land became alienated from them, and they were reduced to a sort of serfdom. But the Japanese were able to turn the same arrangement to profit, and show a rapidly rising rate of land acquisition (Ichihashi, 1932:184, 193).

two Armenian guilds (Stryker, 1959:345), while Eitzen (1971:131) records their prominence among Jewish merchants and artisans in Poland after World War I. Chinese Chambers of Commerce serve some of the same multiple functions as guilds (e.g., Eitzen, 1971:128-9 on the Chinese in the Philippines), and both trade guilds and such chambers of commerce are found among Indians in Malaya (Sandhu, 1969:289).

In sum, middleman community organization combined with thrift, enables middleman firms to cut costs at every turn, so that they can compete effectively with other enterprises in the same line. Add to this a preference for liquidable occupations, and the result is a tremendous degree of concentration in, and domination of, certain lines of endeavor. Concentration is shown in Miyamoto's (1939:70-1) description of the Japanese in Seattle before World War II. He finds that 74 percent were in a "small shop" type of enterprise, while the community lacked a capitalist or true working class. An example of domination is provided in Table 2. In 1938 the Jews made up only five percent of the Stamford population, yet clearly dominated several business lines. Table 3 shows both concentration and domination. The degree of Chinese concentration in the "commercial" class is underestimated, in that "working class" includes employees of commercial establishments.

Indeed this pattern of concentration and domination may be seen as the hallmark of "middleman minorities," and examples of it are found at various times around the world.⁴ Thus in 1943, 63.6 percent of the Chinese in Jamaica were in trade compared to 2.2 percent of the total population. The Chinese made up 0.75 percent of the male wage-earning population but 22 percent of those in trade (Lind, 1958:154). In 1925, 49.3 percent of the Jews in Germany engaged in trade, commerce and peddling, compared to 9.5 percent of the rest of the population

⁴ A problem of definition can be raised: to what degree must an ethnic group be concentrated in, and dominate, certain lines before we term it a "middleman minority"? In other words, is it a discrete category or a continuum? I shall not try to deal with this problem here; but we should note that, if there is a continuum, our examples fall near the end of it.

Table 2. Comparison of Total Number of Trading and Commercial Establishments with Number Owned by Jews, for Selected Businesses, Stamford, Connecticut, 1938^a

	Total Number	Jewish Owned
Retail wearing apparel:		
Men's clothing stores	16	15
Children's and infants' wear	5	5
Dry goods	17	10
Women's and misses' clothing	30	22
Men's furnishings	15	13
Millineries	10	6
Corset shops	6	3
Shoe stores	19	10
Food distribution:		
Largest nonchain markets	5	5
Wholesale cattle slaughter	1	1
Wholesale produce dealers	2	2
Wholesale grocers	2	1
Butter, egg, and cheese dealers	3	2
Retail produce dealers	6	3
Wholesale meat firms	7	3
Delicatessens	9	3
Retail grocery stores	188	23
Retail package stores	51	18
Wholesale liquor dealers	7	2
Other businesses:		
Jewelry	15	13
Wholesale drug	2	2
Cut-rate cosmetics	3	2
Druggists	26	9
Furniture	25	14
Electrical supply	11	6
Hardware	11	4
Paint stores	11	3
Plumbing	5	3
Lumberyards	3	1
Department stores	7	3

^aAdapted from Koenig (1942:210-11).

(Stryker, 1959:342). In 1954, Hollingsworth (1960:1) reports that Asians in East Africa comprised one percent of the population but dominated trade. Loewen (1971:36) finds that 97 percent of the Chinese in Mississippi run grocery stores and hold a near-monopoly over the business. The list could be greatly extended. (It should be understood that domination by the group as a whole does not mean that there are no poverty-stricken members; often individuals and families do not "succeed.")

Table 3. Estimate of Chinese Economic Position in Cambodia, 1962-3^a

Economic Class:	Chinese No. (000s) %	All Cambodia No. (000s) %	Chinese as % of Total
Peasant and fisherman	0 0	4950 86	0
Working	64 15½	209 4	31
Commercial	395 84	379 6½	95
Professional and Gov't.	2 ½	202 3½	1
Total	425 100	5740 100	7.4

^aAdapted from Wilmott (1966:255).

Sojourning is clearly not the only cause of the middleman form; many groups of sojourning migrant laborers do not enter small business. Almost all that do derive from Asia and the Near East. In addition, these same groups become middlemen wherever they go. Chinese, Indians, Jews, in every country show a similar occupational concentration (thus, a status gap in the receiving country cannot explain the pattern). This regularity suggests that culture of origin is an important contributory factor.

Nevertheless, sojourning is necessary to this form, as two facts demonstrate. First, the bulk of middleman minority members are not small businessmen in their homelands; they usually emanate from more deprived classes. Only in the diaspora can they improve their economic condition, a factor which prompted them to leave their homeland in the first place. Second, some of the "source" countries themselves have sojourning immigrants who dominate "middleman" lines, such as Parsis and Armenians in India (on the latter see Basil, 1969), Koreans in Japan, and Armenians in Syria. Sojourning is important in that it creates a preference for liquidity, encourages thrift, and fosters a solidary community that is cooperative internally and "free" to compete with the surrounding society.

Middleman economic behavior is closely akin to preindustrial capitalism. As described by Sjoberg (1960:Chapter 7), the multi-purpose guild of the preindustrial city main-

tained a monopoly over a particular economic activity, stressed particularistic (especially kinship) rather than universalistic bases of recruitment into the occupation, apprenticed extended kin with a view to setting them up on their own, controlled internal competition, pooled resources to develop capital, and performed ceremonial functions.⁵ Max Weber (1958) contrasts pre-modern capitalistic forms (including the economic behavior of Jews and Parsis) with modern industrial capitalism. The distinguishing feature of the latter is "the rational capitalistic organization of (formally) free labour" (1958:21). The modern industrial capitalist treats his workers impartially as economic instruments; he is as willing to exploit his own son as he is a stranger. This universalism, the isolation of each competitor, is absent in middleman economic activity, where primordial ties of family, region, sect, and ethnicity unite people against the surrounding, often individualistic economy.

Host Hostility

Middleman minorities are noteworthy for the acute hostility they have faced, including efforts to cut off their means of livelihood, riots and pogroms, exclusion movements and expulsion, removal to concentration camps, and "final solutions." While their treatment has varied to some extent, certain themes recur in the accusations leveled at these groups.

The middleman and the host society come in conflict because elements in each group have incompatible goals. To say this is to deny the viewpoint common in the sociological literature that host hostility is self-generated (from psychological problems or

cultural traditions). Each party to the conflict has a "reasonable" point of view which arises from the interaction. I shall concentrate on the host perspective primarily because the middleman viewpoint is more commonly aired.

In middleman-host conflicts, the host society does not usually have a united set of interests. Rather, different interest groups come into conflict with the middleman group for different reasons. Indeed, in some cases the middleman's presence adversely affects the relationship or conflict between two host interest groups. That elements in the host society may form a temporary coalition against the middleman group should not obscure the fact that they do so for different reasons. This uniting of old foes has led some observers to define the hatred of middleman groups as scapegoating. It is as if some politician, trying to unite the nation seized an innocent victim for all to hate. Such a view, I would contend, is based on a surface impression.

Conflict between the middleman and the host society arises over economic matters (link 4) and solidarity (link 5). In the first case, middleman minorities conflict with three classes: their clientele, business, and labor.

1. Conflict with Clientele—There is an inevitable conflict of interest between buyer and seller, renter and landlord, client and professional, to which middleman minorities become heir. The Indian riots in Durban in 1949, in which Africans attacked Indian stores, homes, and persons, are an illustration. Palmer (1957:156) describes the pre-riot situation:

[Many Africans] are forced on to land on the outskirts of town which is mostly owned by Indians. Here they erect wretched shacks made of old timbers and corrugated iron without any means of sanitation. Consequently they live in filth and overcrowded conditions. . . . Their landlords are Indian who frequently exact rack rent for the wretched little pieces of land on which the Natives build their miserable huts. The post war years were a time of hardship in many other respects. Prices were rising and the Africans found that the Indian storekeeper with whom they dealt were demanding greatly increased prices for the flour, meal, meal, sugar, condensed milk, etc., which were

⁵ In describing commercial activity in an "economically developing" Indonesian town, Geertz (1963:Chapter 3) sees a number of the characteristics we have noted in middleman groups, including a preference for liquidity. However it is interesting that most of the traders (apart from the Chinese) hail from northern Java, and are not natives of the town. "A culturally homogeneous group, they formed a well-defined, sharply set-apart minority—*wong danang*, the Javanese word for trader, still also means 'foreigner' as well as 'wanderer' or 'tramp'—whose values deviated in major respects from those embraced by both the gentry and peasantry" (Geertz, 1963:43-4).

so important in their diet. Therefore, there grew up among the Africans, not unnaturally, profound irritation against the Indians.

It could be argued that this is a "status gap" conflict in which Indians are merely scapegoats for the real villains, the whites. Yet the same hostility is found toward the Chinese in Thailand where there is no dominant white group:

The Thai see the Chinese as exploiting unmercifully their advantageous economic position: the Thai are obligated to pay high prices to the Chinese for the very necessities of life, and on the other hand are forced to accept the lowest price for the rice they grow. Through deliberate profiteering, according to standard Thai thinking, this minority has driven up living costs (Coughlin, 1960:2).

2. Conflict with Business—Middleman minorities conflict with competing business groups in the settled population. These business groups may have predated the middleman's immigration, and may feel threatened by their lower-priced firms. Or they may be composed of potential businessmen who cannot compete against the entrenched middleman monopoly. This conflict occurs with both members of the superordinate group in society, as in white opposition to Indian business in South Africa, and members of subordinate groups as in African business competition with Indians. (Here is an instance where two otherwise conflicting groups unite in opposition to the middleman minority. Both whites and Africans are "settled" in South Africa, and for this reason are at a disadvantage in competing with Indian firms. If whites and Africans can agree on anything in South Africa, it is on their antagonism to the Indians.)

An example of superordinate business competition is found in white relations with the Japanese on the U.S. Pacific coast. Since farming (market gardening) was an important Japanese business activity, it became an area of conflict. Competition with white farmers is described in a report prepared for the California legislature of 1919, entitled *California and the Oriental* (State Board of Control of California, 1922), with a view to passing an amended Alien Land Law (the first, enacted in 1913, having left many loopholes) which would force the Japanese off the land. The same document

was submitted by the governor of California to members of the federal government in an effort to restrict further Japanese immigration:

The working and living conditions of the Japanese farmer and farm laborer make successful competition by American farmers almost impossible. The Japanese farmers and every member in the family, physically able to do so, including the wife and little children, work in the field long hours, practically from daylight to dark, on Sundays and holidays, and, in the majority of cases, live in shacks or under conditions far below the standards required and desired by Americans. . . . American farmers can not successfully compete with Japanese farmers if the Americans adhere to the American principles so universally approved in America, including clean and wholesome living quarters, reasonable working hours, the usual Sunday rest and holiday recreation and, above all, refraining from working the women and children in the fields (State Board of Control of California, 1922:116-17).

The Chinese in Jamaica illustrate business competition with a subordinate group. According to Lind (1958:156), "The establishment of Chinese grocery shops had extended throughout the island prior to 1911 and had thus brought vividly to the attention of the entire population that these once humble laborers were displacing the native Jamaicans as the shopkeepers of the country."

Business conflict with emerging subordinate groups has increased in post-colonial times. As liberated nations try to gain control of their economies, they come into conflict with middleman groups. In Southeast Asia and East Africa attempts have been made to curb Chinese and Indian business, to establish native peoples in lines long dominated by these groups. The efficient organization of the middleman economy makes it virtually impossible for the native population to compete in the open market; hence, discriminatory government measures (restrictions on the issuance of business licenses, special taxes, and the like) have been widely introduced.

3. Conflict with Labor—The presence of a middleman minority creates a variation of the "split labor market" (Bonacich, 1972), that is, conflict between cheap and higher priced labor. In the middleman family firm,

the interests of employer and employee are not clearly distinct. Employers are paternalistic, employees willing to work long hours at low pay. The result is a cheap and loyal workforce, which threatens to disrupt the relationship between business and labor in the host society; for the latter, in trying to improve its position *viz-a-viz* management (with whom it has a recognized conflict), could price the business out of the market.

Host management has some interest in opposing middleman cheap labor, as we have seen. But management can use this as a weapon against labor by arguing that, if labor insists on higher wages and better work conditions, both will lose. Labor is caught in a bind: either improve its position and accept the possibility of losing the job altogether, or accept a low standard of living and middleman work conditions.⁶

Host workers can, of course, try to get middleman workers to join their unions. But the latter often resist, feeling more closely tied to their co-ethnic employers than to the working class of a country of impermanent residence. Besides, most see their position in the "working class" as a temporary status; a gateway to a business of their own.

Modell (1969) describes a 1937 attempt by the Retail Food Clerks, Local 770, in Los Angeles to organize the sales force in the grocery business. "Since white-run concerns could not concede a substantial advantage in labor costs to their Japanese competitors without suffering losses in trade, Local 770 believed that, if it was to organize the white portion of the industry, it could not ignore the Japanese" (Modell, 1969: 198). The local appealed to Japanese workers to stand up to their employers and "fight for the American standard of living"; but the appeal was rejected, and Japanese-owned firms were black-listed and picketed by organized labor.

The host society can unite on one issue concerning middleman minorities: the solidarity of the middleman community (link 5). In all our examples, middleman groups

are charged with being clannish, alien, and unassimilable. They are seen as holding themselves aloof, believing they are superior to those around them (a "chosen people"), and insisting on remaining different.

The charge of unassimilability can be broken down into at least two substantive accusations. First, middleman minorities are disloyal to the countries in which they reside. They are accused of having "dual loyalties," a familiar cry against the Jews, but also raised against others. One indicator of dual loyalty is a resistance to becoming citizens of the host country. Most works on the Chinese in Southeast Asia devote a section to the "citizenship problem," the Chinese tending not to become citizens. The same is true for Indians in East Africa, and was one of the grounds for complaint raised by President Amin in expelling the Indians from Uganda. Even when middlemen become citizens, people suspect they are acting out of expedience. Coughlin (1960:11) describes this attitude in Thailand, where the Chinese are seen as adopting Thai ways for "protective coloration." In post-colonial societies this distrust was probably exacerbated by the fact that middleman minorities tended to be allied with the colonial masters. Indeed they have an interest in "law and order" for continued trade, hence tend to oppose disruptive political movements.

A second charge is that middleman groups drain the host country of its resources. This is epitomized by their sending money to the homeland, a point prominent in complaints against the Indians in South Africa collected by the Lange Commission in 1921 (Palmer, 1957:81). Other charges of drainage include land mining, not engaging in productive industry, and not contributing to local industries by importing necessities from the homeland. In a word, middleman groups are seen as parasites. As Amin said to the Indians of Uganda, "Some members of your community have no interest in this country beyond the aim of making as much profit as possible, and at all costs" (cited in the Los Angeles Times, August 14, 1972).

The resistance to assimilation of sojourning middleman communities would be no problem for the host society if these groups were economically isolated. Groups like the

⁶ A similar competition is found with firms in other countries which use "cheap" labor. The fact of a political border can enable governments to set up import duties, currency devaluations, and the like, to curb this competition, moves which cannot be used against firms in the same country.

Amish, who preserve cultural distinctiveness but combine it with economic self-sufficiency, do not provoke the same concern. However middleman minorities develop great economic power in a country toward which they feel essentially alien. Such power appears devastating to host members, who believe their country is being "taken over" by an alien group.

I hope the reader is convinced that host members have reason for feeling hostile toward middleman groups. Perhaps you are saying, "Yes, there is a rational component, but the extremity of the host reaction reveals a strong irrational force at work. Middlemen may be felt to compete unfairly, they may even appear dangerously disloyal, but surely the reactions to them are out of proportion to the offense? Usually these groups are tiny minorities with little or no political power. Is it necessary to incarcerate them, as were the Japanese Americans in World War II; to expel them, as were the Asians of Uganda; or to dislocate them, as occurred in the Group Areas Act of South Africa? Surely these acts mark hysteria and deep-seated hatred?"

While some irrational elements are probably at work, even the extremity of the host reaction can be understood as "conflict" behavior. The reason is that the economic and organizational power of middleman groups makes them extremely difficult to dislodge. For example, their wealth enables them to use bribery when necessary, another charge often leveled against these groups. The Chinese in Southeast Asia illustrate the point:

Attempts to control the Chinese have almost everywhere run into the bewildering maze of overlapping Chinese organizations which exist in every country of the area, and they have been frustrated by Chinese evasion, ability and indispensability. . . . Licensing systems have been thwarted by the willingness of inexperienced indigenous businessmen, whom they were intended to benefit, to sell their import and export permits to the Chinese who possessed what they lacked—organization, contacts, experience and capital (Thompson and Adloff, 1955:6-7).

The difficulty of breaking entrenched middleman monopolies, the difficulty of controlling the growth and extension of their economic power, pushes host countries to

ever more extreme reactions. One finds increasingly harsh measures, piled on one another, until, when all else fails, "final solutions" are enacted.

Effects of Host Hostility

Briefly, the host reaction solidifies and isolates the middleman community (link 6). Voluntary segregation gives way to forced segregation. The hostility also nurtures a love of the homeland (link 7), a sentiment reinforced by communal organization (link 8) through such institutions as language and cultural schools. In addition, host efforts to undermine the group's economic influence (link 9) by laws prohibiting ownership of land, for example, restrict their alternatives and increase their occupational concentration.

Ambivalence Towards Place of Residence

Some sojourners save enough and return to their homeland. Others, however, do not return; and it is these that come to form lasting middleman minorities. The typical middleman minority is the remnant (in some cases the majority) of a temporary movement.

The desire to remain overseas has two roots. First, political conditions in the homeland (link 10) may make an imminent return impossible. The conquest of their homeland by a foreign power has kept Jews, Armenians and Parsis in an involuntary diaspora, in two cases, for centuries. Second, sojourning produces a dialectic: it aids in business success, and that very success makes returning difficult (link 11). According to Miyamoto (1939:85) many Japanese in America did go home, but returned to America "after they failed in their native land, and found that life in Japan was harder than life over here." And doubtless failure was not required for many to realize they could not do as well in the homeland. For obvious economic reasons, removing the political barrier to a Jewish return to Israel has not led to an exodus from "Anglo-Saxon" countries.

Remaining in the land of one's sojourn can take two forms. One may relinquish his dream of the homeland and settle in the new country. This would entail engaging in more

non-economic activities, joining non-ethnic organizations, intermarrying with one's neighbors, employing and being employed by persons of different ethnicity, and the like. In other words, it would mean economic and social integration; the middleman form would disappear. Such has been the fate of many individual Jews, Chinese, Indians, Japanese, and such may be the fate of whole communities.

Or, the sojourner may keep alive the desire to return.⁷ His desire may appear to be mythical, finding expression in pious statements like "Next Year in Jerusalem." He may not intend to leave. Yet this orientation retains some substance in the sending of funds to the homeland, occasional visits, and continued solidarity and resistance to assimilation. Love of the homeland is kept alive by host hostility; one supports the homeland in part to have somewhere to go if things get too bad in the host country. And it helps keep that hostility alive through its economic and social consequences. The cycle is self-perpetuating, and the group becomes a permanent minority.

This ambivalence toward place of residence is captured by Simmel in his discussion of the stranger (Wolff, 1950:402-8), whom he describes "as the person who comes today and stays tomorrow. He is, so to speak, the *potential* wanderer: although he has not moved on, he has not quite overcome the freedom of coming and going." The classic example of the stranger, according to Simmel, is the Jew. Weber discusses some of the effects of a stranger or "pariah" status (1963:250):

The legally and factually precarious position of the Jews hardly permitted continuous, systematic, and rationalized industrial enterprise with fixed capital, but only trade and above all dealing in money. . . . As a pariah people, they retained the double standard of morals which is characteristic of primordial economic practice in all communities: what is

prohibited in relation to one's brothers is permitted in relation to strangers.

Middleman minorities are strangers. They keep themselves apart from the societies in which they dwell, engage in liquidable occupations, are thrifty and organized economically. Hence, they come into conflict with the surrounding society yet are bound to it by economic success.

Conclusion

In concentrating on the host point of view, I have not sought to justify their acts against middleman minorities, but to explain them. Indeed, the pattern we have been examining often concludes tragically. The recent Indian expulsion from Uganda has meant great personal loss. And other middleman groups have suffered worse fates. Lacking numbers and political power, in the long run they are likely to lose in their conflict with the host society.

REFERENCES

- Basil, Anne
1969 Armenian Settlements in India. West Bengal: Armenian College.
- Becker, Howard
1956 Man in Reciprocity. New York: Praeger.
- Blalock, Hubert M., Jr.
1967 Toward a Theory of Minority Group Relations. New York: John Wiley.
- Bloom, Leonard and Ruth Riemer
1949 Removal and Return. Berkeley: University of California Press.
- Bonacich, Edna
1972 "A theory of ethnic antagonism: the split labor market." *American Sociological Review* 37(October):547-59.
- Cator, W. L.
1936 The Economic Position of the Chinese in the Netherlands Indies. Chicago: University of Chicago Press.
- Coughlin, Richard J.
1960 Double Identity: The Chinese in Modern Thailand. Hong Kong: Hong Kong University Press.
- Davis, Allison, B. B. Gardner and M. R. Gardner
1941 Deep South. Chicago: University of Chicago Press.
- Department of Economics, University of Natal
1961 Studies of Indian Employment in Natal. Natal Regional Survey, Volume 11. Cape Town: Oxford University Press.
- Desai, Rashmi
1963 Indian Immigrants in Britain. London: Oxford University Press.
- Dotson, Floyd and Lillian Dotson
1967 "Indians and Coloureds in Rhodesia and

⁷ The factors that determine the choice between these two alternatives will be dealt with in another paper. But briefly, one important factor seems to be changing economic conditions (the development of chain stores, super markets, etc.), making the family firm less viable, and driving the younger generations to seek employment in higher-paying non-ethnic firms.

- Nyasaland." Pp. 77-95 in Milton L. Barron (ed.), *Minorities in a Changing World*. New York: Knopf.
- Eitzen, D. Stanley
1971 "Two minorities: the Jews of Poland and the Chinese of the Philippines." Pp. 117-38 in Norman R. Yetman and C. Hoy Steele (eds.), *Majority and Minority*. Boston: Allyn and Bacon.
- Fong, Ng Bickleen
1959 *The Chinese in New Zealand*. Hong Kong: Hong Kong University Press.
- Furnivall, J. S.
1956 *Colonial Policy and Practice*. New York: New York University Press.
- Garis, Roy L.
1927 *Immigration Restriction*. New York: Macmillan.
- Geertz, Clifford
1963 *Peddlers and Princes*. Chicago: Chicago University Press.
- Glazer, Nathan and Daniel P. Moynihan
1963 *Beyond the Melting Pot*. Cambridge: Massachusetts Institute of Technology Press.
- Hollingsworth, L. W.
1960 *The Asians of East Africa*. London: Macmillan.
- Hoselitz, Bert F.
1963 "Main concepts in the analysis of the social implications of technical change." Pp. 11-31 in Bert F. Hoselitz and Wilbert E. Moore (eds.), *Industrialization and Society*. UNESCO: Mouton.
- Ichihashi, Yamato
1932 *Japanese in the United States*. Stanford: Stanford University Press.
- Koenig, Samuel
1942 "The socioeconomic structure of an American Jewish community." Pp. 200-42 in Isaak Graeber and S. H. Britt (eds.), *Jews in a Gentile World*. New York: Macmillan.
- Kurokawa, Minako (ed.)
1970 *Minority Responses*. New York: Random House.
- Light, Ivan
1972 *Ethnic Enterprise in America*. Berkeley: University of California Press.
- Lind, Andrew
1958 "Adjustment patterns among Jamaican Chinese." *Social and Economic Studies* (University College of the West Indies) 7 (June):144-64.
- Loewen, James W.
1971 *The Mississippi Chinese: Between Black and White*. Cambridge: Harvard University Press.
- Mahajani, Usha
1960 *The Role of Indian Minorities in Burma and Malaya*. Bombay: Vora.
- Millis, H. A.
1915 *The Japanese Problem in the United States*. New York: Macmillan.
- Miyamoto, Shotaro F.
1939 *Social solidarity among the Japanese in Seattle*. Seattle: University of Washington Publications in the Social Sciences. 11 (December):57-130.
- Modell, John
1969 "Class or ethnic solidarity: the Japanese American company union." *Pacific Historical Review* 38(May):193-206.
- Palmer, Mabel
1957 *The History of the Indians in Natal*. Natal Regional Survey, Volume 10. Cape Town: Oxford University Press.
- Park, Robert E.
1939 "The nature of race relations." Pp. 3-45 in Edgar T. Thompson (ed.), *Race Relations and the Race Problem*. Durham, North Carolina: Duke University Press.
- Rinder, Irwin D.
1958-9 "Strangers in the land: social relations in the status gap." *Social Problems* 6(Winter):253-60.
- Sandhu, Kernial Singh
1969 *Indians in Malaya*. Cambridge: Cambridge University Press.
- Sanjian, Avedis K.
1965 *The Armenian Communities in Syria Under Ottoman Dominion*. Cambridge: Harvard University Press.
- Schermerhorn, R. A.
1970 *Comparative Ethnic Relations*. New York: Random House.
- Siu, Paul C. P.
1952 "The sojourner." *American Journal of Sociology* 58(July):34-44.
- Sjoberg, Gideon
1960 *The Preindustrial City*. New York: Free Press.
- State Board of Control of California
1922 *California and the Oriental*. Sacramento: California State Printing Office.
- Stryker, Sheldon
1959 "Social structure and prejudice." *Social Problems* 6(Spring):340-54.
- Thompson, Virginia and Richard Adloff
1955 *Minority Problems in Southeast Asia*. Boston: Beacon Press.
- van der Kroef, Justus M.
1953 "The Eurasian minority in Indonesia." *American Sociological Review* 18(October):484-93.
- Weber, Max
1958 *The Protestant Ethic and the Spirit of Capitalism*. New York: Scribner.
- Weber, Max
1963 *The Sociology of Religion*. Boston: Beacon Press.
- Wertheim, W. F.
1964 *East-West Parallels: Sociological Approaches to Modern Asia*. The Hague: W. van Hoeve.
- Willmott, W. E.
1966 "The Chinese in Southeast Asia." *Australian Outlook* 20(December):252-62.
- Wolff, Kurt H.
1950 *The Sociology of Georg Simmel*. Glencoe, Illinois: Free Press.

THE POTENTIAL FOR RESIDENTIAL INTEGRATION IN CITIES AND SUBURBS: IMPLICATIONS FOR THE BUSING CONTROVERSY *

ALBERT I. HERMALIN AND REYNOLDS FARLEY

The University of Michigan, Population Studies Center

American Sociological Review 1973, Vol. 38(October):595-610

Controversies over busing to achieve racial integration of schools result from the intersection of social trends and prevailing values. The movement to expand the civil rights of blacks conflicts with the tradition of neighborhood schools and the residential segregation of neighborhoods.

This paper examines the receptiveness of whites to school and neighborhood integration and explores the economic potential for residential integration. We find the receptiveness of whites to having black neighbors or having their children attend schools with Negroes has increased, and now a majority of whites endorse such integration. Data from the Census of 1970 reveal that economic factors account for little of the concentration of blacks within central cities, their absence from suburbia or the residential segregation of blacks from whites in either cities or suburbs.

The attitudinal receptivity and economic potential exist for extensive residential integration, and these can achieve the dual goals of integrated schools and neighborhood schools.

THE controversy over busing to effect school integration arises from the intersection of social trends and prevailing values. On the one hand, the legal, political, and value systems have been moving in the direction of expanding the civil rights of black citizens, with the focus on removing inequities in the educational system. Confronting these efforts has been the long standing residential segregation of blacks from whites, which limits the degree of school integration so long as neighborhoods remain the primary mode of student allocation.

To have both integrated and neighborhood schools one must have integrated neighborhoods. This paper examines the receptiveness to and economic potential for achieving higher levels of residential integration. Knowledge of the potential for residential integration may in itself affect public attitudes toward busing, schooling, and housing, and represents an important input to governmental decision-making. Particu-

larly so, since there is some evidence of ambiguity and misconception on this score. For example, in a 1968 study of fifteen cities, 56 percent of white respondents felt that Negro disadvantages in jobs, education, and housing were mainly due to "something about Negroes themselves" rather than mainly discrimination (Campbell, 1971:13). In the same study, 68 percent of the whites said that "many" or "some" Negroes "miss out on good housing because white owners won't rent or sell to them."

Assumptions about the causes and levels of residential segregation and its probable rate of change enter not only into popular arguments about busing but have figured prominently in several court decisions. In the years following *Brown versus the Board of Education* in 1954 which held that "racial discrimination in public education is unconstitutional," federal courts overturned those laws that prevented racial integration (*Brown v. Board of Education*, 349 U.S. 294, 1955). The Civil Rights Act of 1964, which included a provision to end de jure school segregation, led to further judicial and administrative action to remove the legal props bolstering racially segregated school systems. Despite such changes, school integration did not advance substantially from 1954 to 1967 (Farley and Taeuber, 1974; U.S. Bureau of the Census, 1971a, Tables

* This paper is a revision of a draft presented at the annual meetings of the American Sociological Association in New Orleans, Louisiana, August 30, 1972. This research was supported, in part, by a grant from the Center for Population Research of the National Institutes of Child Health and Human Development, NIH-71-2210, "The Distribution and Differentiation of Population within Metropolitan Areas."

176 and 177). As a result, emphasis shifted from the de jure aspects of school segregation to so-called de facto reasons among which residential segregation by race received considerable attention.

Cases brought before the federal courts required judges to consider the causes and nature of residential segregation in relation to school segregation to determine if a de jure pattern existed. Several important decisions resulted. Federal Judge Keith, hearing a case involving the Detroit suburb of Pontiac, Michigan, contended that a school board could not blithely observe the emergence of racially segregated neighborhoods, assign pupils to their neighborhood schools and then claim it bore no responsibility for the resulting school segregation. He ordered the busing of children in that suburb (*Davis v. School District of City of Pontiac*, 309 F. Supp., 1970). The Fourth Circuit Court similarly mandated busing to integrate schools in Mecklenburg County (Charlotte), North Carolina. The Supreme Court, in upholding this decision, noted the far reaching implications of the way school authorities constructed new schools and closed old ones. "People gravitate toward school facilities, just as schools are located in response to the needs of the people. The location of schools may thus influence the patterns of residential development of a metropolitan area and have important impact on composition of inner city neighborhoods" (*Swann v. Charlotte-Mecklenburg*, 402 U.S. 1, 1971). In these and other cases the courts recognized the long standing and pervasive nature of residential segregation, found this attributable in part to actions of school authorities, and held that, under certain circumstances, attendance zones which simply reflected the housing patterns were not acceptable.

In two recent cases federal courts took cognizance of the sharp differences in racial composition of cities and their suburbs. In many cities the majority of public school students are black; and even with busing, the racially integrated schools in these cities will be principally black. For instance, in the fall of 1970, in both Richmond, Virginia and Detroit, two-thirds of the public school pupils were Negroes (U. S. National Center for Educational Statistics, 1972:655 and 1,496). In both areas, suits were filed to integrate

the schools. Plaintiffs argued that local, state and federal policies resulted in out-migration of whites from the central cities, a piling up of blacks in the city and the general exclusion of blacks from the suburbs. Busing of students within these cities was seen as ineffective in eliminating racially segregated schools. This litigation led federal Judges Merhige in Richmond and Roth in Detroit to order cross-district busing to effect integration.

In this paper we focus on the actual and potential racial composition of central cities and their suburban rings. This topic is not only in keeping with the current attention to cross-district busing but has received less study than residential segregation in cities. Before turning to our analysis, we briefly review trends in white attitudes on the question of integrated schooling and housing; relevant black attitudes are reviewed in the final section of the paper.

White Attitudes Toward School and Residential Integration

Since the early 1940's national and local surveys have sampled white respondents for their views on the racial situation. In Table 1, we present selected findings from several such studies.

At present there appears to be consensus among whites supporting integrated schools but major disagreement on busing. Greeley and Sheatsley (1971:14), tracing the results of NORC surveys, show that the proportion of whites favoring integrated schools increased from 30 percent in 1942, to 49 percent in 1956, 64 percent in 1963 and 75 percent in 1970 (Table 1, line 1). Concern about busing seems not to have altered this view. A *Detroit Free Press* survey, conducted shortly after Judge Roth's decision ordering cross-district busing, found that about 90 percent of the suburban and city whites in the Detroit area were against busing at the same time that 70 percent disagreed with the opinion that white and black children should go to separate schools (*Detroit Free Press*, May 7, 1972, p. C1).

Though whites strongly endorse the general goal of school integration, other responses suggest this is a highly tempered endorsement. Thus only about one-third of

whites feel that blacks would be better educated in integrated classrooms; the proportion feeling that blacks are receiving an inferior education appeared to decline during the 1960's; and in the seven years following the landmark Supreme Court decision, no more than a fourth of whites favored school integration in the near future. For many whites, the disadvantages of busing may outweigh the severity of the problem it is designed to ameliorate. Gallup polls in 1970 and 1971 found that 80 percent of the white respondents opposed the busing of white and black children from one district to another (Gallup, 1972: 2,243 and 2,329). Propositions concerning busing appeared on the ballot in California and Florida in 1972, and a large majority of the voters in both states opposed busing (*New York Times*, March 16, 1972:30; November 12, 1972:38).

Panel B of Table 1 presents data for whites on attitudes related to residential integration. Line 1 here indicates that the receptivity of whites to having a black family with the same education and income move into their block has greatly increased over time and by 1972 was approved by four out of five respondents. It is worthwhile noting that whites have been as favorable to this posited situation of direct social contact as they have to the more generalized norm of school integration shown in line 1 of Panel A.

The other attitudes related to residential integration shown in Panel B—though showing lower proportions favorable—generally indicate growing receptivity during the 1960's (also see Pettigrew:1973). There is little direct comparability of the attitudes in Panels A and B. Line 3 of the former when compared with line 5 of the latter suggests that a circumstance in which whites are a minority is viewed no more adversely in the housing situation than in the school situation. We do not claim that these data show that whites are as receptive to residential integration as they are to school integration. Several scales designed to rank attitudes indicate they are not (Greeley and Sheatsley, 1971), though the matter requires further research into the structure of racial attitudes. The data do suggest that the receptiveness of whites to residential integration has grown over time and that differences in the two attitudes may be fairly small. Moreover, op-

position to integrated housing appears less strong than opposition to busing; hence, it is worthwhile to explore the economic potential for residential integration, both in its own right and as an alternative means for achieving greater school integration. This is the focus of the next section.

Racial Composition of Cities and Suburbs

We begin by noting the trend in racial composition of urbanized areas. An urbanized area "consists of a central city, or cities, and surrounding closely settled territory" (U. S. Bureau of the Census, 1972, Appendix A); it permits a sharper comparison of central cities with their nearby densely settled suburban rings than does the Standard Metropolitan Statistical Area. Our analysis includes those urbanized areas included among the twenty-five largest in either 1950, 1960, or 1970—a criterion which encompasses the twenty-nine areas shown in Table 2. These twenty-nine areas, which contain thirty-nine central cities, include all those of one million or more in 1970; and they comprised almost 40 percent of the total U. S. population in that year (U. S. Bureau of the Census, 1952, Tables 6 and 17; 1961a, Tables 6 and 23; 1971b, Tables 1 and 21).

Table 2 presents the proportion black in 1950, 1960 and 1970 in each urbanized area, its central city (or cities), and its suburban ring—defined as the area lying outside the central city or cities.¹ In the twenty-nine urbanized areas, the black population grew faster than the white from 1960 to 1970, continuing a trend evident in the previous decade. This differential rate of growth was particularly marked in the central cities, with the proportion black there rising from 13 percent in 1950 to 26 percent in 1970.

The proportion of population black rose in each central city between 1950 and 1970. This is true of those central cities which an-

¹ For each urbanized area, the same central city or cities were used at every date. They were those places defined as central cities in both the censuses of 1960 and 1970. If a city became a central city in 1970, it was not included. There were two exceptions to this rule: Clifton, New Jersey and Pawtucket, Rhode Island. They were excluded since data concerning their small black populations were not published by the Bureau of the Census in 1960.

Table 1 — White Attitudes Toward School and Residential Integration

	1942	1954	1956	1957	1958	1961	1963	1964	1965	1966	1968	1970	1971	1972
	Panel A—School Integration													
1. % saying white & Negro students should attend same schools	30 ^a		49 ^a				63 ^a		67 ^b			75 ^a		84 ^m
1a. Southern whites only	2 ^a		14 ^a				30 ^a		55 ^b			47 ^a		66 ^m
2. % in favor of "immediate" school integration			18 ^d	27 ^d	27 ^d	23 ^d								
3. % with no objection to sending own children to a school with a majority of Negroes		41 ^a			33 ^a		25 ^a		21 ^f	28 ^f	29 ^g			42 ^m
3a. Southern whites only		14 ^a			11 ^a		5 ^a		16 ^f	27 ^f	26 ^g			32 ^m
4. % agreeing that Negroes receive an inferior education							48 ^h			36 ^h				
5. % agreeing that Negroes would be better educated in integrated classrooms										33 ^{h,i}				35 ^o
	Panel B—Residential Integration													
1. % saying it would make no difference if Negro with same income and education moved into block	35 ^b		51 ^b				64 ^b		68 ^b					84 ^m
1a. Detroit SREA—% not disturbed by stated event					<40 ^j								68 ^j	
2. % disagree slightly or strongly that whites have a right to keep Negroes out of their neighborhoods							44 ^a					50 ^a		55 ^m
3. % saying Negroes have a right to live wherever they can afford to								53 ^k			65 ^k	67 ^k		
4. % saying they would not move if "colored people" came to live next door					55 ^l		55 ^l		65 ^l	66 ^l				
5. % saying they would not move if "colored people" came to live in great numbers in neighborhood					20 ^l		22 ^l		31 ^l	30 ^l				

Sources:

- a. National Opinion Research Center (NORC) surveys reported in Greeley and Sheatsley (1971: 13-14). For 1970 national figure, chart indicates 70%, text reports 75%.
- b. NORC survey reported in Sheatsley (1966: 222 and 235).
- c. Pertains to Detroit central city and suburbs, *Detroit Free Press* (May 7, 1972): C1.
- d. Gallup and Roper surveys reported in Schwartz (1967: 30). Questions are not exactly comparable, see source for details and meaning of "immediate."
- e. Gallup surveys reported in Schwartz (1967: 45-46 and 132). Questions not exactly comparable.
- f. Gallup surveys reported in Gallup (1972: 1941, 2010 and 2211). National figures estimated from regional figures.
- g. Poll conducted in 1969.
- h. Harris survey reported in Brink and Harris (1966: 130).
- i. Date not clear from source. Inferred from text.
- j. Pertains to Detroit central city and suburbs. *The Detroit Area Study 1971 Report to Respondents*, Ann Arbor, Michigan: University of Michigan, Detroit Area Study, Project 46822 (January 1972): 7.
- k. Institute for Social Research survey reported in Campbell (1971: 133).
- l. Gallup surveys reported in Schwartz (1967: 133) and in Gallup (1972: 1824, 1825, 1941, 1942 and 2022).
- m. NORC survey reported in NORC (1972).

nexed large surrounding areas such as Houston, Indianapolis and Dallas as well as of those whose boundaries remained fixed. By 1970 two of the areas—Washington and Atlanta—had central cities whose populations were predominantly black; and in four others—Detroit, St. Louis, Baltimore and New Orleans—the central city's population was in excess of 40 percent black.

Very different trends characterize suburban rings. The proportion black has remained at about 4 percent at all dates. In some areas black representation increased: from 4 to 6 percent in New York; from 2 to 5 percent in Los Angeles; and from 7 to 12 percent in Miami. In other areas, it decreased: from 6 to 4 percent in Detroit; from 12 to 6 percent in Kansas City; and

Table 2 -- Blacks as A Proportion of Total Population, 1950-1970

Urbanized Area	Total Urbanized Area			Central City or Cities			Suburban Ring		
	1970	1960	1950	1970	1960	1950	1970	1960	1950
New York	14.9%	10.9%	8.1%	22.6%	14.9%	9.7%	5.9%	4.5%	3.9%
Los Angeles	9.2	7.1	5.4	16.5	12.2	7.9	4.8	3.2	2.3%
Chicago	19.6	16.1	11.6	32.8	23.0	13.9	3.4	3.0	2.9%
Philadelphia	19.8	17.3	14.8	33.6	26.4	18.1	6.9	6.1	6.7%
Detroit	19.0	15.6	12.8	43.6	28.9	16.2	3.7	3.8	5.8%
San Francisco	11.2	9.5	7.0	20.5	14.3	7.9	6.0	5.5	5.9%
Boston	4.7	3.2	2.3	16.3	9.1	5.0	1.1	0.8	0.8%
Washington	27.0	24.9	23.9	71.1	53.9	35.0	7.6	3.7	5.4%
Cleveland	17.0	14.5	10.9	38.3	28.6	16.2	3.7	0.9	0.2
St. Louis	19.4	17.0	14.6	40.9	28.6	17.9	8.9	7.6	9.5%
Pittsburgh	8.5	8.0	7.7	20.2	16.7	12.2	3.9	3.6	4.0
Minneapolis	1.9	1.5	1.3	4.0	2.5	1.5	0.2	0.1	0.1%
Houston	20.4	20.7	19.4	25.7	22.9	20.9	5.8	10.5	10.7%
Baltimore	28.0	24.1	20.6	46.4	34.6	23.4	3.2	3.5	8.3%
Dallas	16.5	14.6	14.2	24.9	19.0	13.1	2.2	2.6	18.7%
Milwaukee	8.5	5.5	2.6	14.7	8.4	3.4	0.2	0.2	0.3%
Seattle	3.4	3.2	2.6	7.1	4.8	3.4	0.5	0.2	0.2%
Miami	14.7	13.4	12.1	22.7	22.4	16.2	11.7	8.7	7.8%
San Diego	5.0	4.3	3.7	7.9	6.0	4.5	1.4	0.7	1.1%
Atlanta	25.1	27.1	28.1	51.3	38.3	36.6	5.8	7.6	12.3%
Cincinnati	13.5	12.9	11.5	27.6	21.6	15.5	3.8	3.9	4.8%
Kansas City	13.5	12.4	12.1	22.3	17.5	12.2	6.1	7.0	11.9%
Buffalo	9.8	7.7	4.5	20.4	13.3	6.3	2.0	2.0	1.8%
Denver	4.7	3.9	3.1	9.1	6.1	3.6	0.4	0.3	0.1%
San Jose	1.7	0.7	0.5	2.5	1.0	0.6	1.2	0.5	0.1%
New Orleans	32.0	31.2	29.4	45.0	37.2	31.9	10.9	14.0	13.1%
Portland	2.8	2.5	2.1	8.3	4.4	2.6	0.3	0.3	0.1%
Indianapolis	16.4	15.4	12.7	18.0	20.6	15.0	0.2	0.4	0.1%
Providence	2.6	2.0	1.7	8.9	5.4	3.3	0.7	0.5	0.1%
Total ^a	14.4	12.1	9.9	25.9	19.1	12.7	4.6	3.7	4.1%

^aWeighted by the size of the urbanized area.

Sources: U. S. Bureau of the Census, Census of Population: 1970, PC(1)-B, Table 23; Census of Population: 1960, PC(1)-B, Table 21; Census of Population: 1950, Vol. II, Table 34.

from 14 to 10 percent in New Orleans. Some suburban rings, like those surrounding Milwaukee and Indianapolis, contained almost no blacks even though, at all dates, these central cities had large black populations.

The Commission on Civil Disorders, appointed by President Johnson following the riots of the 1960's, noted the continued immigration of blacks to large cities and the out-migration of whites. They contended that current governmental policies concerning housing, education and poverty were creating a situation wherein there would be in two decades: "... a white society principally located in suburbs, in smaller central cities, and in the peripheral parts of large central cities; and a Negro society largely

concentrated within central cities" (U. S. National Advisory Commission on Civil Disorders, 1968:407).

The status of this prediction as of 197 may be gauged in part from Table 2. In 1950, the proportion black in the central cities was not much different from the proportion black in the total urbanized area: 12.7 percent versus 9.9 percent, respectively. By 1970, the proportion black in central cities was 25.9 percent as against 14.4 percent for the total urbanized areas.

The dramatic effect of this pattern on the twenty-nine areas can be seen from the table on next page. Though the number of urbanized areas having a fifth or more of their pop-

Table 3 -- Blacks or Nonwhites as A Proportion of Total Public Elementary School Enrollment, 1970 and 1960^a

Urbanized Area	Total Urbanized Area		Central City		Suburban Ring	
	1970	1960	1970	1960	1970	1960
New York	22.7%	16.5%	40.0%	26.1%	5.5%	6.1%
Los Angeles	12.2	10.3	23.6	19.1	6.8	5.2
Chicago	30.0	27.5	54.8	42.6	4.8	4.6
Philadelphia	31.5	29.6	61.1	50.1	10.5	10.0
Detroit	23.4	22.2	64.3	45.8	4.5	5.6
San Francisco	16.2	19.9	40.0	39.3	7.9	9.6
Boston	6.8	5.1	31.9	18.6	1.1	1.2
Washington	34.2	32.8	93.3	79.9	10.4	5.9
Cleveland	25.7	23.0	57.1	47.5	5.4	1.5
St. Louis	29.9	28.5	65.2	50.6	13.9	13.1
Pittsburgh	13.7	13.7	41.9	36.2	5.7	6.1
Minneapolis	2.4	2.6	7.6	5.3	0.2	0.4
Houston	24.8	23.3	32.1	26.5	7.1	11.5
Baltimore	40.6	36.1	66.8	52.5	4.2	5.6
Dallas	20.9	16.1	34.3	21.6	2.3	2.9
Milwaukee	14.3	11.0	27.7	18.0	0.2	0.4
Seattle	4.3	6.6	13.0	11.5	0.5	0.8
Miami	24.9	18.5	38.1	34.2	20.8	12.6
San Diego	7.1	7.1	11.7	10.4	1.4	1.8
Atlanta	29.7	31.1	65.2	45.5	7.6	9.8
Cincinnati	20.5	20.8	45.8	34.2	5.5	6.4
Kansas City	18.3	15.5	32.4	24.2	7.7	8.4
Buffalo	16.0	13.9	39.5	27.7	3.2	3.7
Denver	6.1	5.1	15.2	9.1	0.5	0.7
San Jose	2.2	2.8	3.2	3.0	1.4	2.7
New Orleans	48.5	48.0	68.6	57.7	18.8	24.4
Portland	3.9	4.7	9.8	8.7	0.3	0.8
Indianapolis	21.3	19.2	23.4	27.0	0.1	0.4
Providence	4.0	4.0	19.2	12.7	1.0	0.8
Total ^b	20.7	18.4	41.7	32.4	6.3	5.7

^aData for 1960 refer to nonwhites; for 1970, to blacks.

^bWeighted by the size of the urbanized area.

Sources: U. S. Bureau of the Census, Census of Population: 1960, PC(1)-C, Tables 73 and 77; Census of Population: 1970, PC(1)-C, Tables 83 and 91.

NUMBER OF URBANIZED AREAS, CENTRAL CITIES, AND SUBURBAN RINGS WITH 20 PERCENT OR MORE OF THE POPULATION BLACK IN 1950, 1960, AND 1970. (Based on twenty-nine Urbanized Areas and thirty-nine Central Cities)

	1970	1960	1950
Number of urbanized areas with 20% or more of population black	5	5	4
Number of central cities with 20% or more of population black	23	16	6
Number of suburban rings:			
With 20% or more of population black	0	0	0
With 10% to 20% of population black	2	2	5

ulation black has changed little from 1950 to 1970, the number of central cities with this proportion has more than tripled. No suburban ring has one fifth of its population black and the number having a tenth or more of its population black has declined.

This city-suburban racial contrast is even more marked if the school age population is considered. Table 3 shows the proportion black among public elementary school pupils living in these central cities and suburban rings. In this table, unlike Table 2, the data for 1960 refer to nonwhites.

During the decade, the proportion of students black in these urbanized areas rose

from 18 to 21 percent. In the central cities the gains were much more substantial—from 32 to 42 percent—than in the suburban rings—a change from 5.7 to 6.3 percent black. In the central cities of about half the twenty-nine areas, the proportion black in the public elementary schools exceeds 40 percent. In nine of the thirty-nine central cities—Atlanta, Baltimore, Detroit, Gary, Newark, New Orleans, Philadelphia, St. Louis and Washington—at least three out of five students were Negroes at the end of the decade.

In the suburban rings, there were few sharp changes in the proportion of students black. In some areas this proportion decreased, while in others it increased, though in every case except Miami by a smaller amount than the increase in proportion black in the central city.

These changes in racial composition have been recognized by the federal judges. In the decision calling for cross-district busing in the Detroit area, Judge Roth stated:

By the year 1960, the largest segment of the city's white population was younger and of childbearing age. The population 0 to 15 years of age constituted 30 percent of the total population of which 60 percent were white and 40 percent were black. In 1970, the white population was primarily aging while the black population was younger and of childbearing age.

The percentage of black students in the Detroit public school system in 1975-76 will be 73 percent, in 1980-81, 81 percent and in 1992 it will be virtually 100 percent if present trends continue. (*Bradley v. Milliken*, Civ. Action 35257, F. Supp. 338, 585).

Judge Merhige, who wrote the decision ordering busing throughout the Richmond area contended:

Ever since the *Brown versus Board of Education of Topeka* ruling, population growth in the Richmond metropolitan area has consisted mainly of the addition of whites to the neighboring counties and blacks to the city. In 1955, the Richmond city schools were 43 percent black and in the Chesterfield and Henrico counties, about 15 percent black. In 1972, the city schools are 70 percent black and the county schools, 8 percent black. (*Bradley v. Richmond*, Civ. Action 3353, F. Supp. 338, 90).

Potential Representation of Blacks in Suburban Rings

The proportion of population black is higher in each of these central cities than in their suburban rings and has been increasing more rapidly in the cities than in the suburbs. In all these urbanized areas, the value of housing and the level of family income is greater in the suburban ring than in the central city. In addition, the average black family income is lower than that of whites. An obvious hypothesis is that the absence of blacks from suburban rings may be largely accounted for by their economic status relative to whites and the differential economic status of city and suburban dwellers.

This hypothesis can be promptly rejected. Blacks at all value levels of owned and rented housing were overrepresented in the central cities and underrepresented in the suburbs. The same was true according to family income—at each income level the proportion of blacks living in the suburban ring tends to be much lower than that of whites in the identical income group. This led us to compute the racial distribution in urbanized areas that would be expected if blacks were distributed throughout the urbanized area according to their own value of housing or family income but were represented in the suburbs to the same extent as whites at each economic level. For instance, we took the observed distribution of black families by income and at each income level applied the appropriate income-specific proportion of whites living in the suburban ring. Thus, if 60 percent of whites in the \$10,000 to \$15,000 income category of a given urbanized area lived in the suburban ring, we applied that proportion to the actual number of blacks with that income. This procedure estimated the number and proportion of blacks in the suburbs if blacks retained their own economic characteristics but had the suburban representation rates of the white population. In making this comparison, we dealt with the black and non-black populations. Since the majority of non-Negroes are whites, we designate this the white population.²

² In 1970, 1.6 percent of the non-Negro population consisted of nonwhites, that is, Orientals, Indians, Eskimos and other races (U.S. Bureau of the Census, 1972, Table 60).

Table 4 — Actual Proportion of Urbanized Area Population in Suburban Ring by Race and Expected Proportion of Blacks in Ring on Basis of White Representation Rates

Urbanized Area	Households--1970			Families--1970			Families--1960		
	Actual Proportion in Ring	Expected % of Blacks in Ring ^a		Actual Proportion in Ring	Expected % of Blacks in Ring ^b		Actual Proportion in Ring	Exp. % of Nonwhites in Ring ^c	
		Black	White		Black	White		Nonwhite	White
New York	16%	47%	29%	17%	51%	43%	15%	41%	34%
Los Angeles	27	61	59	31	65	64	25	60	59
Chicago	7	51	30	8	54	46	6	39	32
Philadelphia	17	57	35	18	60	54	16	51	44
Detroit	11	70	48	12	73	67	12	59	53
San Francisco	32	62	53	34	70	66	29	58	55
Boston	16	77	71	16	80	76	19	74	70
Washington	17	84	75	20	91	90	10	75	70
Cleveland	13	71	35	14	72	64	3	59	50
St. Louis	31	73	46	31	76	68	25	61	50
Pittsburgh	32	74	67	33	77	73	29	70	67
Minneapolis	5	51	32	6	58	49	6	42	35
Houston	7	29	27	7	31	29	9	20	18
Baltimore	5	53	36	5	58	51	5	43	36
Dallas	5	39	34	5	43	39	5	30	27
Milwaukee	1	44	22	1	46	39	1	37	31
Seattle	7	52	44	8	59	56	5	37	35
Miami	51	73	56	56	76	71	42	69	63
San Diego	8	40	38	8	45	45	9	34	34
Atlanta	12	68	54	13	73	67	10	46	34
Cincinnati	15	62	50	17	66	62	14	55	50
Kansas City	25	56	45	26	60	57	27	51	46
Buffalo	11	62	37	11	63	56	12	52	45
Denver	3	46	35	3	53	49	6	40	37
San Jose	40	58	56	40	58	56	63	66	65
New Orleans	12	46	30	12	50	43	11	32	26
Portland	5	50	39	5	56	53	8	44	42
Indianapolis	<1	11	7	<1	12	11	1	30	25
Providence	19	77	67	19	79	76	16	70	65
Total ^d	16	57	43	17	60	55	14	50	44

^aExpected on the basis of white suburban representation by value of housing. ^bExpected on the basis of white suburban representation by family income. ^cWeighted by the size of the urbanized area.

Source: See Table 5.

Table 4 presents results of this investigation. We indicate the actual proportion of the urbanized area's total black and white households living in the suburban ring. In 1970, for example, the proportion of black households living in the ring ranged from a low of less than one percent in Indianapolis to a high of 51 percent in Miami. The proportion of white households in the ring ranged from a minimum of 11 percent in Indianapolis to a peak of 84 percent in Washington. Data for families are available for both 1970 and 1960 and are included in Table 4; the actual proportions by family do not differ greatly from those by household.⁸

In comparison, the expected proportions of blacks in the suburban ring are then presented. In Detroit in 1970, 11 percent of the black and 70 percent of the white households in the urbanized area actually resided in the suburban ring. If black households retained their value-of-housing distributions—both owned and leased—but were represented in the suburbs in the same proportion as whites at each level, the overall proportion of black households found in the ring

would rise to 48 percent. If Detroit area black families were represented in the suburbs to the same extent as whites with comparable incomes, 67 percent rather than the observed 12 percent of black families would have suburban residences.

Substantial changes in the racial distribution in urbanized areas would occur if the representation rates of blacks at every value of housing level equaled those of whites. In many areas roughly similar proportions of whites and blacks would be located in the suburban ring. Overall in 1970, the proportion of black households in the ring would increase from 16 to 43 percent, a proportion much nearer the white figure of 57 percent in the suburban ring.

When family income is the criterion, the racial gap would narrow even more. If there were no racial difference in representation rates, then the proportion of total whites and blacks found in the suburban ring would be about the same in Los Angeles, Washington, Houston, Seattle, San Diego, Kansas City, Indianapolis, San Jose, Portland, and Providence. Overall, 55 percent of the black families, not the observed 17 percent, would live in suburban rings. Similarly, when data for 1960 are examined, we discover that racial differences in population distributions would

⁸The 1960 data compare the white and nonwhite populations. At that date blacks comprised 92.1 percent of the nonwhite population (U. S. Bureau of the Census, 1961b, Table 44).

greatly decrease if blacks in each income category were as well represented in suburbia as whites.

Figures in Table 4 imply that economic factors do not account for much of the concentration of blacks in the central city. Merely changing the representation rates while holding constant the economic variables would greatly increase the proportion of blacks living in the suburbs. Nevertheless, we did observe that as economic levels increase, the proportion of population living in the ring tends to increase. This led us to further assess the relative importance of economic factors and representation rates in the areal distribution of blacks. Results of this analysis are contained in Table 5.

The first three columns present data concerning black family income and its relation to that of whites. These show that in 1970, average black family income was \$8,000 or more in twenty of the twenty-nine urbanized areas. Between 1960 and 1970, the ratio of black to white mean income rose in a majority of the areas. These data probably understate the relative improvement of blacks, since the ratios for 1960 compare nonwhite income to that of whites while the 1970 figures refer to Negroes.

The center and right hand columns of Table 5 measure components of black underrepresentation in the suburban ring. For every urbanized area we show the observed percentage point difference in the proportion of whites and blacks in the suburban ring. In the Philadelphia urbanized area in 1970, for example, 60 percent of the white and 18 percent of the black families lived in the suburban ring, yielding a difference of 42 percentage points (see Table 4). If blacks had their own family income distribution but the income-specific suburban representation rates of whites, 54 percent of the blacks would live in the suburbs. Thus, we can estimate that of the total 42 percentage-point difference, a proportion equal to 6 percentage points (that is, 60 minus 54) is attributable to racial differences in income distributions. We are, in effect, standardizing for representation rates to measure the effects of racial differences in income.

Similarly, we can assess the effect of racial differences in suburban representation rates by standardizing for income. That is, we can assume that the black income distribution changes and becomes identical to that of whites. We can then apply the blacks'

Table 5 -- Income of Black Families, 1970; Ratio of Black to White Family Income, 1970 and 1960; Racial Difference in Proportion of Urbanized Area Population in the Suburban Ring and Components of That Difference

Urbanized Area	Family Income Data			Racial Difference (White minus Black) in Percentage of Urbanized Area Population in Suburban Ring					
	Black Family Income	Ratio of Black to White Mean Family Income	1970 ^a	1970			1960a		
				Total Difference	Income	Representation Rates	Total Difference	Income	Representation Rates
New York	\$8,800	.60	.58	34	8	30	26	7	24
Los Angeles	8,900	.63	.66	34	1	31	35	1	33
Chicago	9,400	.61	.57	46	8	46	33	7	32
Philadelphia	8,600	.63	.57	42	6	41	35	7	34
Detroit	9,800	.64	.55	61	6	61	47	6	46
San Francisco	9,200	.62	.64	36	4	34	29	3	28
Boston	8,000	.57	.62	64	4	56	55	4	50
Washington	10,400	.59	.47	71	1	68	65	5	66
Cleveland	9,000	.61	.60	58	8	55	56	9	55
St. Louis	7,900	.57	.52	45	8	43	36	11	39
Pittsburgh	7,500	.60	.56	44	4	43	41	7	41
Minneapolis	9,200	.65	.65	52	9	48	36	7	32
Houston	7,300	.52	.47	24	2	24	11	2	12
Baltimore	8,600	.64	.60	53	7	51	38	7	37
Dallas	7,400	.51	.43	38	4	39	25	3	25
Milwaukee	8,500	.62	.64	45	7	44	36	6	35
Seattle	9,400	.66	.70	51	3	48	32	2	31
Miami	7,100	.55	.54	20	5	18	27	6	23
San Diego	8,500	.69	.64	37	0	37	25	0	25
Atlanta	7,600	.53	.43	60	6	61	36	9	39
Cincinnati	7,800	.60	.56	49	4	46	41	5	40
Kansas City	8,200	.62	.57	34	3	36	24	5	23
Buffalo	8,200	.66	.61	52	7	51	39	7	38
Denver	8,700	.67	.66	50	4	49	34	3	33
San Jose	11,400	.78	.86	18	2	19	3	1	-2
New Orleans	6,100	.48	.47	38	7	38	21	6	22
Portland	8,100	.71	.67	51	3	50	36	2	35
Indianapolis	8,900	.65	.60	11	1	11	29	5	29
Providence	7,000	.60	.61	60	3	55	54	5	49
Total ^b	8,700	.61	.58	43	5	41	36	5	34

^aFigures for 1960 are based upon data for nonwhites. ^bWeighted average.

Sources: U. S. Bureau of the Census, Census of Population: 1960, PC(1)-C, Tables 76 and 78; Census of Population: 1970, PC(1)-B, Table 25; PC(1)-C, Tables 89 and 94.

own suburban representation rates to measure how the proportion of blacks in the suburban rings would be altered by raising the income level of blacks to that of whites. In Philadelphia, were this accomplished, the proportion of blacks in suburbia would be 19 percent, a discrepancy of 41 points (that is, 60 minus 19) from the observed proportion among whites. This discrepancy measures the effect of racial differences in suburban representation rates.

The observed and hypothetical discrepancies are presented in Table 5 for twenty-nine areas. The two components do not necessarily sum to the total difference for there is an interaction component.

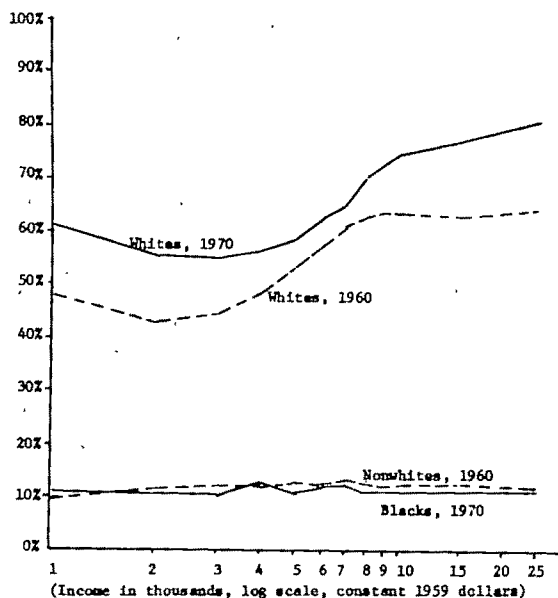
In each urbanized area in 1960 and 1970, the income component is smaller than the representation rate component; indeed in most urbanized areas it is much smaller than the representation component. Between 1960 and 1970, the income component changed very little in most areas; but the representation component increased. These figures indicate that if black incomes continue to increase more rapidly than those of whites, the representation of blacks in suburbia will

change very little if the present income-specific representation rates persist. On the other hand, if the representation rates were changed, the proportion of blacks living in the suburban ring would rise sharply even if there were no change in income.

This pattern exists because the gap between black and white income tends to be smaller than the gap in white and black suburban representation. This is illustrated graphically in Figures 1A and 1B which pertain to the Detroit urbanized area. The left hand panel shows the income-specific proportions of white and black families living in the suburban ring. At both dates, increments in income among whites were matched by increases in the percent living in the ring. Between 1960 and 1970, the share of whites living in suburbia rose at all income levels; and when the recent census was conducted, three out of four Detroit area whites reported a suburban address.

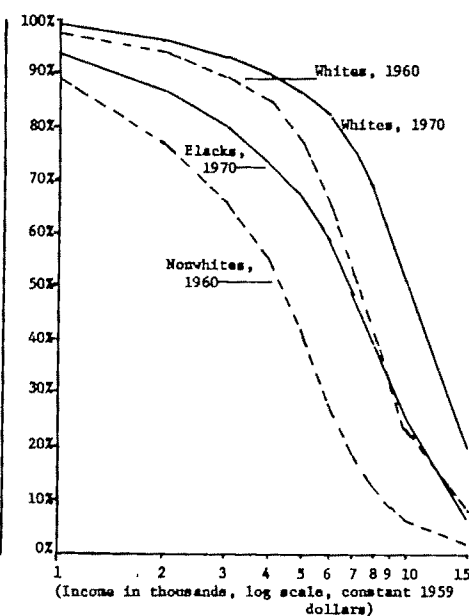
During this decade the income distribution of blacks improved as can be seen by examining the right hand panel, Figure 1B. Between 1960 and 1970, these curves shifted to the right—meaning higher real incomes

Figure 1A. Proportion of Families in Each Income Level Living in the Suburban Ring, Detroit Urbanized Area



Source: See Table 4.

Figure 1B. Proportion of Families Having Incomes of a Designated Amount or More, Detroit Urbanized Area



POTENTIAL FOR RESIDENTIAL INTEGRATION

and greater purchasing power—and the racial gap narrowed somewhat. By the end of the decade, more nonwhites were at those incomes at which a high proportion of whites live in the suburbs.

Despite rising income, 12 percent of Detroit area blacks lived in the suburbs at the beginning and end of the decade. The left hand side of Figure 1 indicates the reason. Among blacks, rises in income were not matched by increases in the proportion living in the suburbs. Furthermore, between 1960 and 1970, almost no change occurred in the proportion of blacks at any income level living in the suburban ring.

We examined analogous information for other urbanized areas, and the findings were similar. To be certain, the levels are not exactly the same in all areas and in certain areas the graphs contain particular wrinkles or indentations. Nevertheless, the overall pattern is clear. The increasing income of blacks has done little to reduce their concentration in central cities, and further rises in the income of blacks will not alter this pattern unless the representation rates change. In all areas, blacks in the higher in-

come categories are less represented in suburbia than are whites in middle or low income brackets. Figure 2 illustrates this finding. It indicates the proportion of black families with incomes \$15,000 to \$24,999 in 1969 living in suburbs, plotted against the corresponding proportion of white families having incomes of \$5,000 to \$6,999. Points above the diagonal indicate that a higher proportion of the lower income white families are in the suburban ring than higher income black families. In every area, the proportion in the suburbs is much greater among the white families than among the black. Apparently a low income white family can obtain a suburban home or apartment more readily than a high income black family.

The Distribution of the Black Population in the Suburban Ring

Although blacks comprise a small share of the suburban ring population, the actual number of blacks in the rings in 1970 ranged from a low of less than one hundred in Indianapolis to a high of almost 450,000 in New York and Los Angeles. We wished to determine whether these suburban blacks were distributed throughout the ring or concentrated in a few suburbs. The data for 1970 reveal that, in most suburban rings, the vast majority of blacks live in those few suburbs which have a large black population.

Table 6 presents findings for the Detroit urbanized area. For each suburb of 25,000 or more, we indicate the total number of occupied dwelling units and the actual and expected units occupied by blacks. Looking at the actual numbers first, we see that more than 95 percent of the 17,000 Negro households in these twenty-six suburbs lived in four places—Hamtramck, Highland Park, Inkster and Pontiac. Twenty of the twenty-six suburbs contained fewer than fifty black households.

We determined what proportion of housing units in each economic category throughout the entire urbanized area was occupied by blacks. We next considered the distribution of owned and leased housing by value in each suburb and ascertained what number of blacks would be in that particular suburb were they present, at each value of housing to the same extent that they are throughout

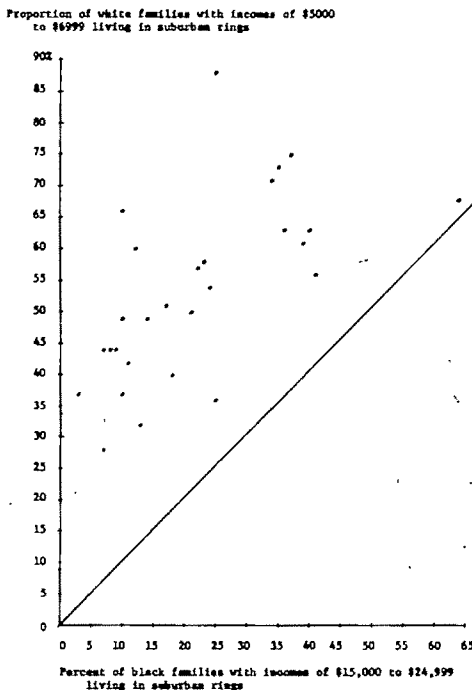


FIGURE 2. Proportions of Black Families with Incomes of \$15,000 to \$24,999 and White Families with Incomes of \$5,000 to \$6,999 Living in Suburban Rings; Twenty-nine Urbanized Areas, 1970

Source: See Table 5.

Table 6 -- Actual Blacks in Detroit Suburbs and Blacks Expected on Basis of Value of Housing and Family Income; 1970 (Suburbs of 25,000 or More)

Suburb	Value of Housing				Family Income	
	Occupied Housing Units	Number Occupied by Blacks		Prop. Occupied by Blacks		Prop. of Families Black
		Act.	Exp.	Act.	Exp.	
Allen Park	11,489	1	856	<0.1	7.5	<0.1 14.1
Birmingham	8,636	3	494	<0.1	5.7	<0.1 12.4
Dearborn	34,614	2	4670	<0.1	13.5	<0.1 15.9
Dearborn Heights	22,481	1	2011	<0.1	8.9	<0.1 14.8
East Detroit	13,077	2	1385	<0.1	10.6	<0.1 16.0
Ferndale	10,137	12	1618	0.1	15.9	0.2 17.4
Garden City	10,482	2	1076	<0.1	10.3	<0.1 15.1
Hamtramck	10,302	1060	3633	10.3	35.3	11.0 21.4
Highland Park	12,412	5190	4315	41.8	34.8	52.8 22.6
Inkster	10,443	4443	1870	42.5	17.9	42.8 18.3
Lincoln Park	15,999	1	2363	<0.1	14.8	<0.1 16.5
Livonia	27,686	2	1584	<0.1	5.7	<0.1 13.1
Madison Heights	10,963	4	1154	<0.1	10.5	<0.1 15.7
Oak Park	10,940	14	712	0.1	6.5	0.1 14.5
Pontiac	25,581	5784	5221	22.6	20.4	23.9 20.7
Roseville	16,453	159	2075	1.0	12.6	0.9 16.5
Royal Oak	27,451	8	2649	<0.1	9.6	<0.1 15.0
St. Clair Shores	24,554	41	2000	0.2	8.1	0.2 14.9
South Gate	9,148	2	957	<0.1	10.4	<0.1 15.1
Southfield	20,159	12	996	0.1	4.9	0.1 12.1
Sterling Heights	16,325	9	1115	0.1	6.8	0.1 14.3
Taylor	18,498	4	2384	<0.1	12.9	<0.1 16.6
Troy	12,195	7	850	0.1	7.0	<0.1 13.7
Warren	48,595	38	4876	0.1	10.0	0.1 14.9
Westland	23,046	473	2406	2.1	10.4	2.1 15.6
Wyandotte	12,922	5	2530	<0.1	19.6	<0.1 18.0
Total	464,689	17279	55,800	3.7	12.0	3.5 15.7

Sources: U. S. Bureau of the Census, Census of Housing: 1970, HC(1)-A24, Tables 8, 10, 11, 18 and 20; Census of Population: 1970, PC(1)-24B, Tables 26 and 29; PC(1)-24C, Tables 89, 94 and 107.

the entire urbanized area. We show the actual and expected proportions black in Table 6. Similarly we ascertained what number of whites (non-Negroes) would be expected in each suburb if whites occupied homes there in each economic bracket to the same degree that they did throughout the entire urbanized area. The expected numbers of blacks and whites in each suburb sum to the total number of households located in that place.

For all twenty-six suburbs together, fewer than 4 percent of the total households are black; but the proportion expected on the basis of housing value is 12 percent. In nineteen of the twenty-six suburbs, one-tenth of one percent or less of the households are black, though five percent is the minimum

expected. In most suburbs, the expected proportion black is between 9 and 18 percent. The actual proportion black falls within that range in only one suburb—Hamtramck.

Similar calculations were made using family income as the criterion. In the right hand columns of Table 6 we show the actual proportion of families black and the proportion black expected if each suburb had its own income distribution, but blacks at each income level were as well represented in that suburb as throughout the entire urbanized area. Using family income as the criterion, the discrepancy between the actual and expected proportions black is even greater.⁴ In

⁴The method used to calculate the expected proportion here differs from that described in the previous section. Here we assume that the propor-

Table 7 -- Actual and Expected Proportions of Households and Families Black and Actual and Expected Residential Segregation Scores for Suburbs of 10,000 and Over, 1970

Urbanized Area	No. of Subs.	Households				Families			
		Prop. of Households Headed by Blacks		Residential Segregation Scores		Prop. of Families Headed by Blacks		Residential Segregation Scores	
		Act.	Exp. ^a	Act.	Exp. ^a	Act.	Exp. ^b	Act.	Exp. ^b
Cleveland	38	4%	9%	77	21	4%	13%	77	5
Dallas	13	2	11	46	13	2	12	50	7
Detroit	46	4	12	86	22	4	16	87	6
Miami	15	3	8	66	12	3	11	65	8
St. Louis	30	9	12	77	27	9	15	78	10
San Fran.-Oakland	40	5	8	68	13	5	10	70	6
Washington	42	5	19	36	17	5	23	37	8

^aExpected on the basis of value of housing occupied by blacks.

^bExpected on the basis of black family income.

Sources: U. S. Bureau of the Census, Census of Housing: 1970, HC(1)-A, Tables 8, 10, 11, 18 and 20; Census of Population: 1970, PC(1)-B, Tables 26 and 29; PC(1)-C, Tables 89, 94 and 107.

every suburb the expected proportion of families black is within the range of 12 to 23 percent. Yet, the actual proportion of families black is one-tenth of one percent or less in eighteen of the twenty-six suburbs.

The concentration of blacks in suburban rings was studied for seven urbanized areas and results are displayed in Table 7. We assembled data for all suburbs of 10,000 and over in 1970 in each suburban ring. First, the actual and expected proportions of households and families black are indicated for all suburbs together. In each case, the expected proportion black is much higher than the actual. In Washington, for instance, the actual proportion of families black is 5 percent; the expected proportion, 23 percent. In Cleveland, the actual is 2 percent; the expected, 12 percent. This illustrates

further that economic factors do not account for the low proportion black in these suburban rings.⁵

To examine the distribution of blacks in these suburbs, segregation indices were calculated using individual suburbs as the units of analysis. The index of dissimilarity was the measure used to do this. A value of one hundred indicates complete segregation, and in such a case all suburbs would be exclusively black or white. A value of zero indicates complete integration—all suburbs would have identical racial compositions.⁶

⁵In a table not shown here we performed a similar calculation for the total suburban ring, including suburbs under 10,000, and total central city population of each of the twenty-nine urbanized areas. In every case, the expected proportion of the central city which is black was lower than actual; and the expected proportion of the suburban population black increased sharply. For the value of housing criterion, the expected proportion of households black in the suburban ring was at least double that actually found in twenty-two of the twenty-nine urbanized areas.

⁶The size of the segregation index depends on the size of the areal units used. The smaller the

tion of blacks found at a given economic level for the entire urbanized area holds for each suburb; so that if black families account for 15 percent of all families in the \$10,000 to \$15,000 income category, we assume they account for this proportion of this income level in each suburb.

Segregation indices were calculated twice. First we used the actual distributions of blacks and whites, and then we compared the expected distributions of blacks and whites. As shown in Table 7 in every case, the actual segregation score was at least double that expected on the basis of housing value. With regard to the distribution of families by income, the actual segregation scores were four or more times as large as the expected. This indicates that if blacks and whites were distributed throughout the suburban rings on the basis of the value of their housing or family income, there would be relatively little residential segregation by race on a suburb by suburb basis.

Lastly, we considered the level of residential segregation in central cities. Taeuber and Taeuber (1965) showed that there was a high degree of residential segregation in central cities in 1960 and that this had changed relatively little over the two previous decades. They furthermore demonstrated that the expected level of segregation, based on value of housing criteria similar to those used here, was much lower than that existing. Using census tracts as units, we calculated the actual index of segregation for a few central cities and found that segregation in 1970 was about at the same high level as in 1960.

Though expected indices cannot readily be calculated for 1970, other evidence strongly indicates that residential segregation in central cities in 1970 is far in excess of what would be expected on economic grounds alone.

Conclusion

This report corroborates and extends previous findings on racial changes in metropolitan areas and residential segregation. We find, first, a continuing growth of black population in these urbanized areas, particularly within the central cities. Second, economic criteria account for little of the observed concentration of blacks in central cities and their relative absence from the suburbs. Third, there is a high level of racial

subareas, the larger the index, so that an index value computed from suburbs as units will be less than if census tracts were used across the same geographic area.

residential segregation within suburban rings which cannot be accounted for by economic criteria. In most instances, the majority of suburban blacks reside in a few suburbs, whatever their economic level. Fourth, based on scantier evidence, we believe that economic factors continue to account for little of the racial segregation of neighborhoods in central cities.

If one eliminates economic characteristics as the major source of residential segregation, then one must look to preference among blacks for residing in black neighborhoods or discrimination by whites, either explicit or covert, as the general causes. Recent surveys of the attitudes of urban blacks have found that they generally prefer racially mixed neighborhoods rather than the segregated neighborhoods in which they live, and they prefer to send their children to integrated public schools. The *Newsweek* polls found that the proportion of blacks indicating they wished to live in racially mixed neighborhoods increased during the 1960's reaching 67 percent toward the end of the decade. Seven of ten blacks claimed they wished to have their children attend schools with white youngsters (Brink and Harris, 1967:232-4). In the 1968 fifteen city study, only 13 percent of the blacks reported they preferred to live in an all or mostly Negro neighborhood; and only 6 percent held that black children should go to all or mostly Negro schools (Campbell and Schuman, 1968:15-16).

We believe then that the current level of residential segregation must be attributed largely to actions and attitudes, past and present, which have restricted the entry of blacks into predominately white neighborhoods. Our general conclusions are well captured by Judge Roth's findings in the Detroit case:

The city of Detroit is a community generally divided by racial lines. Residential segregation within the city and throughout the larger metropolitan areas is substantial, pervasive and of long standing. Black citizens are located in separate and distinct areas within the city and are not generally to be found in the suburbs. While the racially unrestricted choice of black persons and economic factors may have played some part in the development of this pattern of residential segregation, it is, in the main, the result

POTENTIAL FOR RESIDENTIAL INTEGRATION

of past and present practices and customs of racial discrimination, both public and private, which have and do restrict the housing opportunities of black people.

Governmental action and inaction at all levels: federal, state and local, have combined with those of private organizations, such as loaning institutions, real estate associations and brokerage firms to establish and maintain the patterns of residential segregation throughout the Detroit metropolitan area. (*Bradley v. Milliken*, Civ. Action 35257. F. Supp. 338, 587).

A number of implications for policy and further research follow from these findings.

1. Increasing the proportion of blacks in white areas of the city or in the suburban ring does not require locating low income or subsidized housing in the suburbs, nor must residential integration await the further upgrading of black income. Policy makers should be made aware that this change does not require massive expenditures of federal, state or local money. Further steps to equalize the economic status of blacks and whites or to provide better housing to all low income people are indeed desirable on other grounds, but these steps need not delay the increased amount of residential integration already possible.

2. With the removal of explicit federal and state actions which hindered residential integration, such as Federal Housing Administration lending practices, the focus in removing existing impediments should be on what Amos Hawley has described as the "web of discrimination"—the real estate practices, the mortgage lending arrangements, the climate of opinion and the like—which deter blacks from obtaining the housing for which they are economically qualified (National Academy of Sciences, 1972:20-31; Hawley and Rock, 1973:19). Guidance in removing these impediments should be aided by research which further specifies the components of this "web" and their mode of operation as well as by research into regional and other factors associated with differing levels of residential integration by urbanized area.

3. It would be desirable to make widely known to policy makers and the population at large the considerable economic potential blacks now have to occupy housing at all value levels in the cities and suburbs. Glazer (1960:7) conjectured that the perceived so-

cioeconomic distribution of a minority group affects the receptivity of white homeowners to having them as neighbors. If whites were made aware of the large middle class existing among blacks, their concern about racial residential integration might be lessened.

Further attitudinal research might well seek to determine how whites perceive the income distribution of blacks. In addition, surveys among both whites and blacks should obtain more precise measures of how each group defines an integrated neighborhood. It may well be that whites will typically consider a neighborhood integrated if it has 10 to 20 percent blacks, while blacks prefer to live in neighborhoods 30 to 40 percent black.

4. The bus is not the only vehicle for integrating schools. The economic potential exists to largely achieve both integrated schools and neighborhood schools through greater racial residential integration. Moreover, school integration achieved through greater residential integration may have different educational implications than that achieved through busing. Armor (1972) distinguishes "induced" school integration, as in a busing program, from "natural" school integration, which would arise from residential integration, and contends that "natural" school integration will promote a contact among blacks and whites of approximate socioeconomic equality that may better accomplish both educational goals and racial harmony.

However that may be, the available data demonstrate that the receptiveness and economic potential now exist for a high degree of residential integration. To the extent that this comes about, it will have far reaching implications for race relations; not the least important, in the context of current concern, is a form of school integration which is neighborhood based.

BIBLIOGRAPHY

- Armor, David G.
1972 "The evidence on busing." *The Public Interest* 28(Summer):90-125.
- Bradley v. Milliken, Civ. Action 35257 (E.D. Mich. 1971).
- Bradley v. Richmond, Civ. Action 3353 (D.C. Va. 1972).
- Brink, William and Louis Harris
1967 *Black and White*. New York: Simon and Schuster.

- Brown v. Board of Education, 349 U.S. 294 (1955).
 Campbell, Angus
 1971 *White Attitudes Toward Black People*. Ann Arbor: Institute for Social Research.
- Campbell, Angus and Howard Schuman
 1968 *Racial Attitudes in Fifteen Cities*. Ann Arbor: Institute for Social Research.
- Davis v. School District of City of Pontiac, 309 F. Supp. (1970).
 Detroit Free Press
 1972 (May 7).
- Farley, Reynolds and Alma F. Taeuber
 Forth- "Racial segregation in the public schools." coming *American Journal of Sociology*.
- Gallup, George H.
 1972 *The Gallup Poll*, New York: Random House.
- Glazer, Nathan
 1960 "Introduction" in Nathan Glazer and Davis McEntire (eds.), *Studies in Housing and Minority Groups*, Berkeley: University of California Press.
- Greeley, Andrew M. and Paul B. Sheatsley
 1971 "Attitudes toward integration." *Scientific American* 225(December):13-19.
- Hawley, Amos H. and Vincent P. Rock (eds.)
 1973 *Segregation in Residential Areas*. Washington: National Academy of Sciences.
- National Academy of Sciences
 1972 *Freedom of Choice in Housing*. Washington: National Academy of Sciences.
- National Opinion Research Center
 1972 *National Data Program for the Social Sciences. Codebook for the Spring 1972 General Social Survey*. Chicago: National Opinion Research Center, University of Chicago.
- New York Times
 1972 (March 16 and November 12).
- Pettigrew, Thomas F.
 1973 "Attitudes on race and housing: a social-psychological view." Pp. 21-84 in Amos H. Hawley and Vincent P. Rock (eds.), *Segregation in Residential Areas*. Washington: National Academy of Sciences.
- Schwartz, Mildred A.
 1967 *Trends in White Attitudes Toward Negroes*. Chicago: National Opinion Research Center, University of Chicago.
- Sheatsley, Paul B.
 1966 "White attitudes toward the Negro." *Daedalus* 95(Winter):217-38.
- Swann v. Charlotte-Mecklenburg, 402 U.S. 1 (1971).
 Taeuber, Karl E. and Alma F.
 1965 *Negroes in Cities*. Chicago: Aldine.
- U. S. Bureau of the Census
 1952 *Census of Population: 1950, P-A1*.
 1961a *Census of Population: 1960, Vol. I, Part A*.
 1961b *Census of Population: 1960, PC(1)-1B*.
 1971a *Statistical Abstract of the United States: 1971*.
 1971b *Census of Population: 1970, PC(1)-A1*.
 1972 *Census of Population: 1970 PC(1)-B1*.
- U. S. National Advisory Commission on Civil Disorders
 1968 *Report of the National Advisory Commission on Civil Disorders*. New York: Bantam Books.
- U. S. National Center for Educational Statistics
 1972 *Directory of Public Elementary and Secondary Schools in Selected Districts, Enrollment and Staff by Racial-Ethnic Group*. Fall 1970.

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

THE CAUSES OF RACIAL DISORDERS: A GRIEVANCE-LEVEL EXPLANATION *

WILLIAM R. MORGAN

Indiana University

TERRY NICHOLS CLARK

University of Chicago

American Sociological Review 1973, Vol. 38 (October): 611-624

This paper presents three basic arguments that draw on data from a sample of forty-two American cities. First, the data reveal that three separate factors—frequency, precipitation conditions, and severity—are important to the structure of racial disorders. Much earlier work, especially that of Spilerman, focused only on frequency. Hence, in part two we consider disorder frequency, replicating and extending Spilerman's findings. In particular, we show that certain city-specific differences (nonwhite population size and police force size) have strong effects on disorder frequency. Third, consideration of disorder severity shows the need for a model quite different from that used for frequency. The conditions critical to the dynamics of initial confrontation are not those critical to the dynamics of escalation. City-specific grievance variables, especially black-white differences in jobs and housing, are particularly important.

INTRODUCTION

SEVERAL recent studies (Lieberson and Silverman, 1965; Bloombaum, 1968; Downes, 1968; Wanderer, 1969; and Spilerman, 1970, 1971) have correlated measures of racial disorders and structural characteristics of American cities. Their results generally support various deprivation-producing conditions; but as for which structural characteristics are crucial, the studies do not agree. One cause of this inconsistency may be the theoretical and empirical complexity of the link between structural preconditions and the actual manifestation of collective behavior episodes. A second possibility, argued by Spilerman, is that during the 1960's any local variations in structural conditions were greatly outweighed in their possible causal effects by a uniformly-perceived national climate of protest among blacks.

* A much earlier version of this paper was originally presented by the authors at the joint meeting of the Midwest-Ohio Valley Sociological Societies, Indianapolis, May, 1969. The authors are grateful to Karl Schuessler for comments on an earlier draft, to Elton Jackson, Jack Sawyer, and George Dowdall for helpful suggestions at various stages of the research, and to Carolyn Mullins for editorial assistance.

Clark's final contributions were made while he was Visiting Associate Professor at the Departments of Sociology and Political Science, and Institution for Social and Policy Studies, Yale University (Fall 1972) and at the Sorbonne (Paris V, Spring 1973). We are grateful for partial support from the National Science Foundation (GS-1904, GS-3162).

Spilerman's evidence (1970:639-45) comes largely from analysis of community disorder proneness, i.e., community characteristics which best account for the fact that some cities have more disorders than others. His principal finding was that sixteen indicators of social disorganization, black deprivation, and responsiveness of the political structure could not explain variation in disorder proneness as well as the single variable of nonwhite population size. Although the sixteen variables (together with a dummy variable for southern region) explained 42% of the variance, the single variable nonwhite population with southern region explained slightly more: 47%. The sixteen variables entered into the regression equation after non-white population, explained only 4% more of the total variation. Spilerman rejected the possibility that nonwhite population might simply be a proxy for the other community characteristics on the ground that when included in the regression after the other sixteen variables, nonwhite population explained an additional 9% of the variation.

Because these sixteen community characteristics had no substantial effect on disorder proneness independent of nonwhite population size, he argued that they were not causal conditions; rather "they are the incidental characteristics of cities with large Negro populations" (Spilerman, 1970:645). Spilerman essentially concluded that the more blacks in a city, most of whom perceived and supported a national climate of

protest, the more able was the city's black community to mobilize a disorder, and hence the more likely was an occurrence of collective racial aggression.

Our data both expand and modify Spilerman's findings. Our argument is that variables other than nonwhite population size affect disorder potential. Furthermore, disorder frequency by contrast with disorder potential is governed by quite different dynamics. Our analysis was conducted on a set of cities drawn from the NORC Permanent Community Sample. Earlier work based on this sample used fifty-one cities, selected as a representative sample of places of residence of the American population. These ranged in population size from 50,000 to 750,000 (1960 census). Further details are reported elsewhere (Clark, 1971, 1972). The present paper used only forty-two of the fifty-one because nonwhite census data were unavailable for the nine cities with less than 1,000 nonwhites. The majority of eliminated cities were generally white suburban towns; not surprisingly, none had experienced racial disorders.¹ The data on racial disorders were obtained from the Lemberg Center for the Study of Violence and are exclusively for 1967. They show that disorders occurred in twenty-three of the forty-two cities in the sample.

The results are reported in three main sections. First is a factor analysis of the racial disorder variables. In conjunction with it we suggest reasons why community correlates of disorders vary with different disorder dimensions. Second is an analysis of the relationship Spilerman found between nonwhite population size and disorder frequency. Finally, using a grievance-level framework, we analyze the impact of specific community conditions on disorder severity.

THE STRUCTURE OF RACIAL DISORDERS

Operating Definitions of Disorders

Spilerman used four criteria to define racial disorders: (1) occurrence during 1961-1968, (2) involvement of thirty or more individuals, (3) an act of Negro aggression rather than white aggression or interracial

violence, and (4) "spontaneous" or unplanned origin. The last three criteria, while difficult to operationalize with precision, enabled him to define a fairly uniform set of disorders for constructing his dependent variable, the number of disorders. However, the very uniformity of these criteria necessarily reduces the range of variation in disorder activity and, correspondingly, possibilities of correlation with structural variables.

For our analysis, racial disorders were defined as (1) crowd behavior (the activities of four or more people acting in concert) (2) which represented episodes of racial tension. Racial tension was defined as aggressive behavior either by blacks against whites or whites against blacks, involving either damage to persons or property and/or defiance of civil authority.² By contrast with Spilerman's requirement of thirty persons, the minimum lower limit of four used in this definition avoids the risk of falsely eliminating an incident that has, say, only twenty-five participants but all other disorder characteristics. Furthermore, the risk of falsely including an incident is quite small; an incident involving as few as four participants would probably fail to meet the necessary behavioral criteria. As it turned out, all the disorders included here were reported to have at least fifty participants.

Our expanded definition combined with our restriction to 1967 disorders produces a data set different from Spilerman's (which spanned 1961-1968). The difference gave our analysis at least one major advantage. While any cross-section analysis is bound to specific historical conditions, correlations found within a single year may be less ambiguous than correlations that aggregate several years of disorders. Several general conditions during the 1960's could have affected the causal structure behind racial disorders. These include (1) the escalation of

¹ The cities excluded were Amarillo, Tex.; Bloomington, Minn.; Clifton, N.J.; Duluth, Minn.; Euclid, Ohio; Fullerton, Calif.; Irvington, N.J.; Manchester, N.H.; Warren, Mich.

² A full working definition of the concepts used here is presented in the Lemberg Center's *Riot Data Review* (1968b:2). The data used here were cross-checked against those in the *Report*, National Advisory Commission on Civil Disorders (1968). For 1967, the screening process and definition used by the Lemberg Center produced 218 incidents over 184 communities, as compared with the 164 in 128 communities reported by the Commission. The cross-check resulted in our adding to the analyses one city not included in the Lemberg data.

the Indo-China War and several political assassinations (both contributed to a national climate of violence and civil disobedience among both civilians and police); (2) the belief of many black citizens in the efficacy of racial disorders as political action (apparently this belief first swelled, then ebbed); (3) the increasing ability, with experience, of local and federal agencies to contain and prevent disorders; and (4) the change in national political leadership as it affected both white and black citizens.

In 1967 the probable net effect of these conditions was to heighten, at least within black communities, the perceived value of racial disorders as social protest (cf. Tomlinson, 1968; Paige, 1971). A particularly strong social protest impetus undergirded black participation in that year. Therefore, one would expect grievance-producing community characteristics to be more important in 1967 than in other years.³

Despite the differences, our analysis and Spilerman's have one overriding similarity: Much riot research has focused only on "communal" riots (Janowitz, 1969) which occurred, for the most part, prior to World War II.⁴ Other research (e.g., Lieberman and Silverman, 1965) has included both "communal" riots and the "commodity" riots of the 1960's. Both our analysis and Spilerman's focus only on the commodity riots.

Disorder Variables

Nine separate disorder variables were used in this analysis—number of active participants, number of police at scene, number of arrests, duration (days), number of injuries, estimated property damage, a precipitation index, a militancy index, and the frequency of 1967 disorders. For the eight sampled

cities with more than one disorder, the first eight variables were defined as applying only to the most severe disorder. In every case, consistent with the national trend, the most severe disorder was the first one (see the National Advisory Commission on Civil Disorders, 1968:113).

These nine variables were subjected to an orthogonal factor analysis employing a principal component solution and varimax rotation (Table 1). The first three factors explained 75% of the total variance. The first factor, the disorder severity dimension, accounted for 48% of the variation and was defined by the seven variables measuring extent of participation and intensity of action (see boxed variables on Table 1). The second and third factors were each defined by a single variable, precipitation index and disorder frequency respectively.

Disorder Precipitation Conditions

The precipitation index shown in Table 1 was measured by a four-point scale of degree of instigation by blacks rather than by civil authorities or whites. Certainly there are many more dimensions to a precipitating event (the final incident before a disorder begins) than this "who started it" measure; but more sophisticated measures are generally unrealistic, given the available descriptions of most events. At the zero-point of black instigation, a group of whites attacked blacks, who in turn resisted (9% of the disorders studied); at the next level, black on-lookers became enraged at police action against a fellow black who had been stopped for a minor infraction (35%); at the third level, the leaders were unable to retain control of an organized black protest rally (9%); at the fourth level, a group of blacks initiated attacks on white property or persons in the apparent absence of an immediate issue (48%).⁵

³ By contrast, the second large wave of disorders in the spring of 1968 (precipitated by the assassination of Martin Luther King) more accurately fit Smelser's (1963) conceptualization of disorders as nonrational, "hostile outbursts."

⁴ Communal riots were usually struggles between residents of mixed black-white neighborhoods; these riots, then, did not occur in areas of clear racial domain. Commodity riots, by contrast, begin within the black community rather than at its periphery. They are commodity riots in the sense that the blacks' violence often centered on property (e.g., retail establishments), owned usually by outside white proprietors.

⁵ This distribution of precipitating events compares with the national distribution in 1967 of the twenty-four disorders classified as serious in the National Advisory Commission Report (1968). Of those disorders, 50% were precipitated by spontaneous protest against police action; 21% started after organized protest rallies; 21% started after police mobilization in response to rumors; 4% involved interracial violence, and 4% had no apparent instigating event. The range of precipitating condi-

Table 1. Factor Analysis of Racial Disorder Variables^a

Variable ^b	Factor 1 Severity	Factor 2 Precipitation	Factor 3 Frequency	Communality
Arrests, total	.85	-.12	-.02	.74
Duration, days	.84	-.17	.15	.76
Injuries, total	.84	-.15	.27	.79
Property Damage, dollars	.83	.07	.02	.69
Militancy Index	.76	.20	.11	.64
Rioters, total	.68	.50	.08	.71
Police at Scene, total	.64	-.02	.31	.51
Precipitation Index	-.11	.94	-.06	.91
Disorder frequency	.14	-.04	.97	.97
Percent of Total Variance Explained (Cumulative)	48%	62%	75%	

^aSource of data for the racial disorder variables: Lemberg Center for the Study of Violence, (1968a), Report of the National Advisory Commission on Civil Disorders (1968). Data on property damage, total rioters, and total police at scene were unavailable for five cities. Correlations with these measures were computed only for cities with complete data.

^bIn order to approximate conditions of normality and homogeneity of variance, disorder frequency was transformed by \sqrt{x} , and the other variables, by $\log(x + 1)$. The average intercorrelation of the seven severity variables was .56. Their average correlation with precipitation index was .06; with disorder frequency, .23.

Spilerman's definition of disorders would have excluded disorders at the first and third levels of instigation. (The black aggression criterion excluded instances of white aggression or interracial violence; the spontaneity criterion excluded disorders "which had their origins in civil rights demonstrations, in school settings, or in other activities which might provide a focus for contending groups" [Spilerman, 1970:630]). His reason was that riots of these two types would differ either in basic nature or in frequency or severity from riots of the other two types. The fact that

our precipitation index loaded on a factor independent of the severity and frequency variables suggests that these exclusions may have been unnecessary; in our data, all four types are undifferentiated on these variables. Of course, this noncorrelation could simply indicate a high degree of measurement error in our index. However, if we assume the accuracy of the measurement, then this variable's independence suggests substantively that the degree of immediate black instigation is *not* associated with either the number of blacks participating in disorders or the frequency of disorders.

tions is similar in both samples of disorders, although the distribution within that range is somewhat different. The lower proportion of disorders reported by the Commission to be without any known precipitant may be related to that study's restriction to "serious" disorders only.

Disorder Frequency and Severity

Spilerman limited his analysis to disorder frequency, contending that the "underlying level of frustration in the community"

(1970:630, n. 8) was more likely to be captured by a measure of the number of outbreaks than by a measure of severity. His position is based on the assumption that disorder severity, in contrast to frequency, results largely from the response of social control agencies to the precipitating incident. This position is doubtful on two counts. First, the substantial number of disorders precipitated by routine police-citizen encounters contradicts the assertion that social control agencies are somehow less important for the outbreak of disorders than for their escalation. Second, disorder escalation, as represented by disorder severity variables, involves a joint spiraling process of widened civilian and police involvement. We grant that the higher levels of participation by blacks in some cities, as manifest in the more severe disorders of those cities, may have resulted partly from ineffective control strategies in those cities. However, without any evidence to the contrary, the alternative explanation—that a higher level of participation grew largely out of deeper and more widespread discontent—must remain at least equally plausible.

The results of the factor analysis document the association between disorder intensity (or violence) and level of participation. The three measures of participation (number of arrests, number of participants, and number of police) all load on the first factor together with four measures of disorder intensity (duration, number of injuries, estimated property damage, and militancy index). The militancy index is a four-point scale including (1) disorders involving only rock-throwing, window-breaking, and/or police taunting (in five cities), (2) those involving (1) plus looting (in four cities), (3) those involving (1) and (2) plus fire-bombing (in five cities), and (4) those involving (1), (2), and (3) plus alleged sniping (in nine cities).⁶ Another indication of the correspondence between participation and disorder intensity is that in seven cities with 0–5 persons arrested, the median number of injuries was one; in nine cities with six to fifty persons arrested, the median number of injuries was ten; in

five cities with fifty-one to eight hundred, the median was fifteen; and in the two cities with over eight hundred, the median was six hundred.

The association between severity and participation is crucial. If a grievance-level model is to be proved satisfactory in explaining disorder severity (as will be done below), level of participation must first be presented as a more significant attribute of disorder severity than the more commonly perceived attribute, disorder intensity. *The more widespread the grievances among blacks, the greater the number of potential disorder supporters, and hence the more severe a disorder is likely to be, once it occurs.*

Also important in considering whether grievance-level variables would relate equally to disorder frequency and severity as dependent variables, this analysis suggests that the outbreak of a disorder in 1967 followed a far less uniform pattern than did subsequent escalation. Escalation, by definition, has always involved increased levels of participation; whereas, an outbreak only indicates some form of civil disobedience by a minimum number of blacks acting in concert. We argue that such outbreaks in the past, regardless of their frequency, may have had little or no community-wide significance. Substantial escalation, however, at least in 1967, seems to have coincided with widespread feelings of grievance among blacks.

DISORDER FREQUENCY AND CONFRONTATION PROBABILITY

The Size-Occurrence Effect

In this section we focus on disorder frequency and its correlates before turning to disorder severity. Certainly city size is an important factor in frequency. Larger cities experience more disorders. Larger cities also have more blacks. Along with disorders and blacks, larger cities include more of many other things, especially those relating to social interaction. This basic idea has been elaborated and documented in detail by many years of urban research (e.g., Zipf, 1949; Schnore and Varley, 1955; Ogburn and Duncan, 1964).

Spilerman did not deal explicitly with this phenomenon, although there is evidence that

⁶ The scale is equivalent to Downes' (1968) intensity index and Wanderer's (1969) severity index except that the latter scaled the actions of both civil authorities and black civilians.

this city-size effect influenced his data: After nonwhite population size, the variable in his analysis having the strongest zero-order correlation with disorder frequency was population per councilman (Spilerman, 1970:642). The low variance across cities of council size relative to population size gives this ratio variable a strong community-size component. Consequently, it is not surprising that in Spilerman's analysis, population per councilman correlated .48 with disorder frequency, second only to the .59 value for nonwhite population; whereas the average correlation (absolute value) for the sixteen other independent variables with disorder frequency was only .13.

To determine which variables differentiate between cities that had disorders and those that did not, we performed a stepwise discriminant function analysis. In two group discrimination problems such as this one, this procedure is equivalent to stepwise regression analysis with a dummy dependent variable (Rao, 1965:480-3). The variables used included most of those in Spilerman's design plus two size measures—police force size and total population size—in addition to nonwhite population.⁷ Since total population had the strongest zero-order correlation with disorder occurrence, it entered the discriminant function first. This variable alone correctly classified all but ten cities, producing a highly significant between group/within group variation ratio ($F = 12.6, p < .01$).⁸ No

⁷ The data source for the number of police was *The Municipal Yearbook* (1967). On the assumption that the 5-19 cohort in 1960 should correspond closely to the 12-26 cohort in 1967, the actual variable used for number of blacks was the log number of nonwhite males aged 5-19 in the 1960 census. The fact that black male youth were the ones most active in the disorders gives this variable somewhat greater theoretical merit than the total nonwhite population figure. Since, over cities, this variable correlated above .99 with log of nonwhite population (Spilerman's measure), for purposes of intercity correlation analysis they are identical and interchangeable.

⁸ Following the advice of Winch and Campbell (1969), tests of significance have been presented throughout this analysis as evidence for the stability of the measured relationships. Given the nonprobability nature of the sample, the reader should not infer that these tests bear directly on whether the findings can be generalized (to some larger universe either of cities or disorders) except insofar as internally stable relationships are usually more replicable than unstable ones.

other variable, when entered after city size, had a significant discriminating effect. Cities with disorders had an average population of 382,738, compared with 171,244 for cities without disorders. Most incorrectly classified units were small cities within large metropolitan areas.

The Probability of Confrontation

A reasonable explanation for the effect of city size could be based on the probability of confrontation. Disorders have often begun after confrontations between black youths and police. Large cities are likely to contain not only more protest-prone blacks but also larger absolute numbers of contacts between black youths and police simply by virtue of the greater numbers of each. Consequently, more opportunities exist for social contacts that could precipitate a disorder.

Table 2. Zero-Order, Partial, and Multiple Correlations of Three Size Measures with Disorder Frequency^a

Correlations with Frequency (X_4)	Nonwhite Population Size (X_1)	Police Force Size (X_2)	Total Population Size (X_3)
Zero-order r	.48	.55	.59
Partial r , controlling for other two size measures	.05	.11	.16
Partial r , controlling for relative size	.33	.47	---
Multiple $R_{4.12}$.58		
Multiple $R_{4.123}$.60		

^aAll correlations except those in the second row are significant at $p < .01$. Those in the second row are nonsignificant at $p > .05$.

Our analysis (see Table 2) showed that the zero-order correlations with disorder frequency of total population, number of police, and nonwhite population are .59, .55, and .48, respectively ($p < .001$ for each). When disorder frequency was regressed on both nonwhite population and police force size, 34% of the variation was explained ($p < .001$).

Partial correlation permits exploration of two alternative explanations: (1) The presence of sufficient numbers of blacks alone was the critical factor in determining disorder frequency. If this were the case, one would expect a residual correlation between nonwhite population and disorder frequency after controlling the other two size measures. This residual did not occur. The second-order partial correlation of nonwhite population with frequency was only .05 which, in addition to being absolutely low, was even lower than the analogous partial correlations for the other two size measures. (2) The police and nonwhite population variables may have operated not in the physical contact sense proposed but rather in the attitudinal sense of generating a climate of hostility conducive to disorders. A city with a large police force might have a "law and order" climate that created resentment among blacks. Similarly, the more blacks in a city, the greater the possibility that whites and the predominantly white police force would develop a fear of the unknown implications of "black power."

If either attitudinal effect was present in the correlations of police and nonwhite population with disorder frequency, we would expect these correlations to drop substantially when we control each on its respective relative size variables (number of police per capita, and proportion of the population nonwhite). These reductions would be expected because of the substantial overlap between the absolute and relative size measures (the average intercorrelation over the two pairs is .79) and because the relative size of the presumed threat, whether blacks or police, should be at least as critical as the absolute size. As Table 2 shows, however, the relative size controls reduced the absolute size correlations only slightly; the data thus show negative support for the climate hypothesis and positive support for the contact hypothesis.

In summary, the argument in this section

is two-fold. First, total population, not nonwhite population, was the single best predictor of disorder frequency. Second, the best theoretical explanation of disorder frequency requires using the number of both blacks and police as predictors. One remaining issue is whether total population has any additional explanatory power when added to the regression after these two variables. If it did, then some explanation in addition to confrontation probability would be needed to account for the city-size effect. However, when total population was added to the regression *after* the police and nonwhite population measures, the multiple correlation increased only .02 to a value of .60 (Table 2). The answer to this remaining issue, then, is negative.

Let us now consider disorder severity instead of frequency. The measure of disorder severity used is the log of total number of arrests. This variable has the highest loading on the severity factor; further, because of the more accurate enumeration of arrests relative to other variables (e.g., property damage), it is probably the most reliable of the set.

The simple model which we developed to account for disorder frequency does not explain the severity of disorders very well. The two variables—black population and police force size—which explained 34% of disorder frequency only explained 11% of the variation when severity was regressed on them. This finding provides further evidence (supporting the results from our analysis of the structure of racial disorders) that the community conditions critical to the dynamics of initial confrontation were not those critical for the dynamics of escalation.

GRIEVANCE LEVEL AND DISORDER PARTICIPATION

The Grievance-Level Model

Two different causal perspectives might explain variance in disorder escalation. Both are similar to those discussed more generally by Gamson (1968). One is based on the relative ability of local authorities to manage discontent among blacks. The city characteristics most relevant to this perspective are those that reflect the effectiveness of local government and other control agencies. The other perspective has to do with the degree of discontent among blacks insofar as this

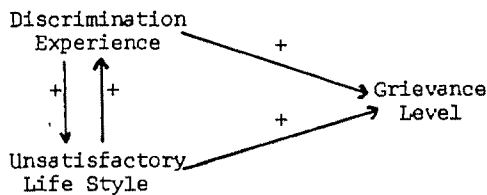


Figure 1. The Grievance-Level Model.

discontent would mobilize participation once a disorder was initiated. The relevant city characteristics here are those that affect racial discrimination and life style dissatisfaction among blacks. A full analysis integrating both perspectives is beyond the limits of this paper. Because more empirical indicators are presently available for the second rather than the first, we have chosen to concentrate on the second, a grievance-level approach.

Grievance level—or the “underlying level of frustration,” to use Spilerman’s term—is thus our principal theoretical link between the objective social conditions of cities and the subjective decision of individuals to participate in disorders (see Mattick, 1968, for a related discussion of grievances among blacks). The relationship between the two main components of the grievance level, experience of discrimination and life style dissatisfaction, is diagrammed below (see Figure 1) using conventional path notation to indicate direct and indirect effects. Conditions that foster an unsatisfactory life style directly raise the grievance level, as do conditions that maintain racial discrimination. The direct effect of the former means that if certain cities had more blacks who were dissatisfied with their life style, the grievance level in that city would be higher even if the specific discrimination-producing conditions were constant across cities.

Unsatisfactory life style conditions also have an indirect effect on grievance level through their effect on discrimination experience; they further raise the grievance level by making the experience of discrimination more difficult. This indirect effect implies that the less cushioned a person is by a satisfying life style, the more sensitive he will be to daily discrimination. Similarly, the

indirect effect of discrimination conditions works by lowering the quality of life style experienced and thereby increasing the grievance level.

Because most available indicators pertained more to life style than to discrimination conditions, the present test of the grievance-level model emphasizes conditions more relevant to the former than the latter. Consequently, the net effect of an unsatisfactory life style may be overestimated since the indirect effect of discrimination conditions is included in the estimate.

Determinants of Life Style

Three sets of community variables affect life style satisfaction among blacks: (1) amount of racial inequality, (2) the “central city syndrome,” and (3) government service capability. The relevant variables and their zero-order correlations with disorder severity are presented in Table 3.

Racial inequality was measured using differences between blacks and whites in (1) proportion of occupied dwelling units in substandard condition,⁹ and (2) proportion of employed males in low-status jobs.¹⁰ The substantial mean differences between blacks and whites (see Table 4) make these variables good reflections of the prevalent inequalities in jobs and housing as they varied from city to city.

Duncan (1966) has argued that, before

⁹ It appears that the definition of substandard housing, that is, dilapidated structures, varied greatly across cities and thus reflected conditions of local housing markets. The proportion of black dwelling units in substandard condition correlated across cities only .25 with the proportion of black dwelling units rented ($p > .05$), .12 with the proportion of black dwelling units having one or more persons per room, $-.09$ with the proportion of black dwelling units in a building with five or more dwelling units, and .25 with the proportion in a building more than twenty years old. Consequently, we used the substandard housing variable alone, on the assumption that it came closer to reflecting the extent of satisfactory housing than any other variable separately or in combination.

¹⁰ The proportion of employed black males in low-status jobs correlated across cities .65 ($p < .01$) with the proportion of black families earning under \$3,000 annual income. However, the former was chosen for the regression analysis for its substantially lower correlation in difference score form with the other inequality measure, the proportion of families in substandard housing.

using a model employing difference scores as independent variables, investigators should test the adequacy of a model employing simple additive effects of the variables comprising the difference scores. Although his comments were directed specifically to the use of social mobility scores, they presumably apply to racial inequality scores as well. Consequently, a comparison of the additive and the interaction (or difference score)

Table 3. Zero-Order Correlations with Disorder Severity
Of Selected City Variables^a

Variable ^b	r
Racial Inequality ^c	
1. % housing substandard	.40
2. % males with low status jobs	-.48*
The "Central City Syndrome"	
3. Total population	.31
4. Residential segregation index	-.14
5. Population density	.42*
6. Increase in % population nonwhite, 1950-60	.36
7. % population nonwhite	.11
8. % population foreign born	.35
9. Median age white males	.18
10. % whites college educated	-.40
Government Service Capability	
11. City budget expenditures per capita	.20
12. Urban renewal expenditures per capita	.20
13. Police per capita	.51*
14. Police salary level	.08

*p < .05, two-tailed test.

^aSource of data for the 14 community structure variables: variables 1-3, 7-10, Census Tracts for SMSA's (U.S. Bureau of the Census, 1960); variable 4, Negroes in Cities (Taeuber and Taeuber, 1965); variables 5, 6, County City Data Book (U.S. Bureau of the Census, 1967); variables 11, 12, Finances of Municipalities (U.S. Bureau of the Census, 1962); variables 13, 14, The Municipal Yearbook (International City Manager's Association, 1967).

^bSkewed variables were transformed as follows: variable 4 by x^2 ; variable 6 by $\sqrt{x + 7}$; and variables 5, 7, 9-14 by $\log_{10}x$.

^cBoth variables are in black minus white difference score form. Substandard housing consists of all occupied dwelling units rated in a condition other than 'sound.' Low-status employment consists of employed adult males working as service workers and nonfarm laborers.

Table 4. Mean City Scores For Blacks and Whites On Housing and Job Status Variables^a

	Blacks	Whites	Difference (B-W)
Percent in Substandard Housing	38.9%	6.6%	32.3%
Percent in Low status Employment	37.7%	12.3%	25.4%

^aSource of data: Table 3.

models was conducted by examining the following three regression models:

(1) Additive model:

$$S = B_J + W_J + B_H + W_H$$

(2) Additive plus interaction model:

$$S = B_J + W_J + B_H + W_H + (B - W)^2_J + (B - W)^2_H$$

(3) Interaction only model:

$$S = (B - W)^2_J + (B - W)^2_H$$

Subscript J indicates a job measure; subscript H, a housing measure; B, a job or housing measure for blacks; W, a measure for whites; and B - W, a difference score measure. For this analysis, only the difference scores were squared to avoid any linear dependency in the second regression model.

Models one and three explained 48% and 43%, respectively, of the variation in severity; model two explained only an additional 2% of variation over model one. Duncan asserts that when a simple model of additive effects is sufficient to reduce the data, use of a more complex model violates the principle of parsimony.

Parsimony notwithstanding, however, we believed a racial inequality model (in contrast to mobility effect studies) based on difference scores provided a representation of the relevant community variation theoretically superior to a model postulating effects for community conditions of blacks and whites separately. Our reason for this choice is that use of an inequality model to explain community variation in disorder severity enables a relatively direct translation to an individual-level, perception-of-inequality explanation of disorder participation. The more objective inequality in a city, as measured by the difference scores, the greater the amount

of inequality perceived by blacks and thus the higher the grievance level.¹¹ Such a translation requires assuming that the use of whites as a reference group and the application of the norm of equality to this racial comparison were done in roughly the same amounts by blacks across cities. However, if we recall that in 1967 racial polarization (and thus the relevance of white, out-group comparisons) and black militancy (and thus the relevance of equality norms for blacks) were national phenomena, these working assumptions do not seem unreasonable.

The "central city syndrome" here refers generally to changes experienced most strongly by older and larger American cities. Eight variables derived from the U. S. Census were used to measure this global quality of cities: (1) total population, (2) density (population per square mile), (3) index of residential segregation (Taeuber and Taeuber, 1965), (4) increase in percent of population nonwhite, 1950-1960, (5) percent population nonwhite, (6) percent population foreign born, (7) median age of white males, and (8) percent of white college educated. Downes (1968), the main proponent of the "central city" explanation, suggests this syndrome adversely affects the life style of the total citizenry, but especially that of blacks, for whom it is the most serious.

Government service capability refers to the ability of urban institutions to deliver desired services, particularly to blacks. Four variables from the 1967 Municipal Yearbook were used to measure this ability: (1) budget expenditures per capita, (2) urban renewal expenditures per capita, (3) police per capita, and (4) police salary level. The term roughly corresponds to the explanation offered by Lieberman and Silverman (1965)

¹¹ We have refrained from using the concept of relative deprivation to interpret the impact of racial inequality on black grievance level in a city. Objective inequality may be grievance-producing by arousing increased feelings of deprivation in a strictly relative sense. However, Homans' (1961) social exchange principle of distributive justice suggests that objective inequality is grievance-producing insofar as it arouses feelings of injustice. Our preference is for the latter conceptualization; but our data, of course, cannot be addressed to this individual-level problem (cf. Morgan and Sawyer, 1967).

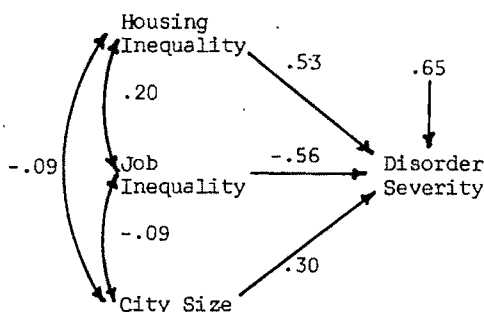


Figure 2. Path Diagram for Grievance-Level Model of Disorder Severity.

when they summarized the differences between riot and nonriot cities in their analysis of American race riots from 1913 to 1963. Obviously government service capability, like the central city syndrome, is a rather demanding label to place on these particular variables; we introduce the terms simply to represent as clearly as possible the arguments of earlier writers.

Findings

The best three-variable regression model utilizing the above variables to explain variance in disorder severity proved to consist of the two inequality variables plus city size. This model is presented as a single stage path diagram whose exogenous variables are job inequality, housing inequality, and city size, and whose endogenous variable is disorder severity (Figure 2). Since our analysis is exploratory, we do not claim that this set of variables fits all the specifications for a formal system of structural equations. However, for variance explained, they are quite adequate.

The three variables together explained 58% of the variation in disorder severity ($p < .01$). No other variable, added singly to the model, increased the percent of variance explained by more than five points. Furthermore, no three of these variables produced a multiple correlation with disorder severity as high as did the two inequality variables. The two inequality variables without city size still explained 48% of the variance.

Housing and job inequality both had strong, direct effects on disorder severity; but unexpectedly they worked in opposite

directions. Inequality in housing increased the severity of disorders while inequality in jobs decreased it.

Job Inequality. The unexpected negative effect of this variable necessitated a reinterpretation. Instead of generating active dissatisfaction among blacks, high rates of job inequality appeared to contribute to complacency (i.e., it suppressed rather than raised the grievance level). High job inequality in a city (indicating a more racially stratified occupational structure) could produce this complacency in at least two ways. First, a more stratified structure would reduce the opportunity for equal-status contacts with whites and thus the salience of white standard-of-living norms as referents for blacks. Second, a more racially stratified structure, presumably of long duration, would reduce occupational mobility expectations among blacks, and thus dissatisfaction with one's current job. These possibilities contradict our earlier working assumption that the use of whites as a reference group, and of the norm of equality as a basis for comparison, would operate for blacks in the same way across cities. This interpretation is supported by the fact that cities with more job inequality were often in the South, the region in which such effects should be expected to operate most effectively. The zero-order correlation of job inequality with a dummy variable for South was .61 ($p < .01$). (Further analysis of the relation of region to grievance level is reported below.)

Housing Inequality. The effect of this variable, as expected, was strong and direct. The underlying dynamic was presumably that blacks living in cities with more housing inequality were more likely dissatisfied with their life style; this situation should raise the grievance level in those cities. The critical issue here is why housing inequality had the expected positive effect on grievance level and disorder severity when job inequality did not. Perhaps it is that when blacks compare social reforms regarding housing and jobs, less has been accomplished with the former, relative to what was promised. Or it may simply be that the conception of an adequate life style for blacks is more tied to housing equality than to job equality.

Whatever the reason, independent survey

Table 5. Reported Satisfaction of Blacks and Whites With Their Housing and Jobs, 1949-1969^a

Year of Survey	Satisfied with Their Job ^b			Satisfied with Their Housing		
	Whites	Blacks	Difference	Whites	Blacks	Difference
1949	69%	55%	14%	67%	59%	8%
1963	90%	54%	36%	76%	43%	23%
1966	87%	69%	18%	77%	51%	26%
1969	88%	76%	12%	80%	50%	30%

^aSource of columns 1, 2, 3, 5, and 6: Gallup Opinion Index (1969:8, 12).

^bThe wording of the question was: "On the whole would you say you are satisfied or dissatisfied with: The work you do? Your housing situation?"

evidence suggests that the different effects of housing and job inequality were not simply statistical artifacts. Table 5 reports the results of four national Gallup polls (taken between 1949 and 1969) concerning the satisfaction of blacks and whites with their jobs and housing. These attitudinal data reveal the pattern that we found. Over the twenty-year period, the proportion of blacks satisfied with their jobs increased 21 percent; while the proportion satisfied with their housing decreased 9 percent. By contrast, increasing proportions of whites were satisfied with both their jobs and housing (the overall increases being 19 and 13 points, respectively). Moreover, although whites were more satisfied than blacks on both situations, in the last poll the difference between whites and blacks in percent satisfied was two and one half times greater for housing than for jobs. In short, the striking overall trend is that while blacks in the 1960's became more satisfied with their jobs relative to whites, they became less satisfied with their housing.

Regional Variation. The possibility of regional influence on the job satisfaction variable deserves further investigation. Five of our twenty-three sample cities experiencing disorders were in the South. The zero-order correlation between a dummy variable for the South and disorder severity was an insignificant $-.12$, indicating that, at least for this sample, southern disorders were no more or less severe than those elsewhere. By contrast, the job and housing variables corre-

lated $.61$ and $.42$, respectively, with the South. Apparently, in these southern cities the mitigating effect on grievance level of high job inequality counteracted the aggravating effect of high housing inequality; the result was a distribution of disorders similar to that for the cities outside the South.

Disorder Frequency. The final analysis turned again to disorder frequency as the dependent variable. Disorder frequency was regressed on the two inequality variables. Only 1% of the variation in frequency was explained by this analysis (compared with 48% for disorder severity). This finding complements the earlier one that the size-related variables which explained disorder frequently could not explain much variation in disorder severity; the finding further supports the prior distinction between community conditions related to the dynamics of initial outbreak and those related to the dynamics of participation and escalation.

CONCLUSIONS

Community conditions made a significant difference in both the frequency and severity of 1967 disorders. Cities with a greater confrontation probability (with respect to number of blacks and number of police) had more frequent disorders. Cities with a higher grievance level among blacks (with respect to the two inequality measures) had higher rates of disorder participation and hence more severe disorders. In particular, a high

degree of housing inequality raised the grievance level, while a high degree of job inequality tended to suppress it.

In our sample, as in Spilerman's, the number of blacks in a city did predict the frequency of disorders; but it did so less well than two other measures related to city size—total population and police force size. This overall city-size effect, and not the black population size effect as Spilerman argued, was explained simply by its ability to increase the probability of a disorder-creating confrontation between blacks and police.

Spilerman argued that a large black population increased the opportunity for mobilizing a disorder. At this point we go one step further, suggesting that mobilizing for a disorder involves social control agents as well as black civilians. In fact, as we observed in our analysis of precipitating events, the initial mobilization in some cities apparently came exclusively from the police.

Our findings support Spilerman's assertion that no community conditions besides the size factor are causally related to disorder frequency. But again, our interpretation must differ. Spilerman suggested that this nonrelationship was generated by a national climate of protest among blacks, which suppressed the effect of local conditions on disorder frequency. We suggest, instead, that the variable "disorder frequency" has even less utility than Spilerman implies. The "underlying level of frustrations" (Spilerman) or grievance level (our preferred concept) among blacks is not tapped by disorder frequency, as Spilerman contended, but by disorder severity. When community conditions inducing grievances were in fact examined for their effects on disorder severity, strong effects, independent of the size relationship, were found.

What generalizations can be drawn from our results? To what extent will our explanation hold for a different set of disorders, occurring in a different sample of cities? As in any research, the final answer will come from replications of our research. The current study, however, presents three pieces of affirmative evidence. First, despite our smaller sample of cities and disorders, we were able to replicate Spilerman's positive finding. Second, we showed that the range of disorder precipitants in our sample resembled

that reported in the National Advisory Commission Report (1968). Third, the importance of the particular grievances studied was reinforced by data from four national surveys on changes in the relative satisfaction of blacks concerning jobs and housing over a twenty-year period.

While our findings have yet to be corroborated by others, two policy conclusions may be inferred. Those in a position to influence policies concerning black-white differences in jobs and (especially) housing should be conscious of the import of decisions in these areas compared to others. If significant differences between blacks and whites continue in these areas, grievances and hence potential disruption, are more likely. To decrease these tensions, housing conditions for blacks should be improved.

Those concerned with organizing black can conclude from these findings that many grievances may be common to blacks in American cities. Nevertheless clear discrepancies in job and housing patterns exist across cities, and these significantly affect the grievance level. These differences should be heeded, both in decisions regarding the selection of cities for organizational effort, and in the selection of grievances upon which to build an organization.

REFERENCES

- Bloombaum, Milton
1968 "The conditions underlying race riots. *American Sociological Review* 33(February):76-91.
- Clark, Terry Nichols
1971 "Community structure, decision-making budget expenditures, and urban renewal in 51 American communities." Pp. 293-313 in Charles M. Bonjean, Terry N. Clark, and Robert L. Lineberry (eds.), *Community Politics*. New York: Free Press.
- 1972 "The structure of community influence. Pp. 283-313 in Harlan Hahn (ed.), *People and Politics in Urban Society*, *Urban Affairs Annual Reviews*, 6. Beverly Hills: Sage.
- Downes, Bryan T.
1968 "Social and political characteristics of riot cities: a comparative study." *Social Science Quarterly* 49(December):504-20.
- Duncan, O. D.
1966 "Methodological issues in the analysis of social mobility." Pp. 51-98 in Neil J. Smelser and S. M. Lipset (eds.), *Social Structure and Mobility in Economic Development*. Chicago: Aldine.

- Gallup Opinion Index
1969 Report No. 47 (May). Princeton, N.J.
- Gamson, William A.
1968 *Power and Discontent*. Homewood, Illinois: Dorsey Press.
- Homans, George C.
1961 *Social Behavior: Its Elementary Forms*. New York: Harcourt, Brace, and World.
- International City Managers Association
1967 *The Municipal Yearbook*. Chicago.
- Janowitz, Morris
1969 "Patterns of collective racial violence." Pp. 412-44 in Hugh D. Graham and Ted R. Gurr (eds.), *The History of Violence in America*. New York: Bantam.
- Lemberg Center for the Study of Violence
1968a *Compilation of the 1967 Disorders*. Brandeis University. Unpublished.
1968b *Riot Data Review*. Number 1 (May). Brandeis University. Mimeographed.
- Liebertson, Stanley and Arnold R. Silverman
1965 "The precipitants and underlying conditions of race riots." *American Sociological Review* 30(December):887-98.
- Mattick, Hans W.
1968 "Form and content of recent riots." *Midway* 9(Summer):3-32.
- Morgan, William R. and Jack Sawyer
1967 "Bargaining, expectations, and the preference for equality over equity." *Journal of Personality and Social Psychology* 6 (June):139-49.
- National Advisory Commission on Civil Disorders
1968 *A Report*. Washington: Government Printing Office.
- Ogburn, William F. and O. D. Duncan
1964 "City size as a sociological variable." Pp. 129-47 in E. W. Burgess and D. J. Bogue (eds.), *Contributions to Urban Sociology*. Chicago: University of Chicago Press.
- Paige, Jeffery M.
1971 "Political orientations and riot participation." *American Sociological Review* 36 (October):810-20.
- Rao, C. R.
1965 *Linear Statistical Inference and Its Applications*. New York: Wiley.
- Schnore, Leo F. and David W. Varley
1955 "Some concomitants of metropolitan size." *American Sociological Review* 20(August):408-14.
- Smelser, Nell J.
1963 *Theory of Collective Behavior*. New York: Free Press.
- Spilerman, Seymour
1970 "The causes of racial disturbances: a comparison of alternative explanations." *American Sociological Review* 35(August):627-49.
1971 "The causes of racial disturbances: tests of an explanation." *American Sociological Review* 36(June):427-42.
- Taeuber, Karl E. and Alma F. Taeuber
1965 *Negroes in Cities*. Chicago: Aldine.
- Tomlinson, T. M.
1968 "The development of a riot ideology among urban Negroes." *American Behavioral Scientist* 2(March):27-31.
- United States Bureau of the Census
1960 *Census Tracts for SMSA's*. Washington: Government Printing Office.
1962 *Finances of Municipalities*. Washington: Government Printing Office.
1967 *County and City Data Book*. Washington: Government Printing Office.
- Wanderer, Jules J.
1969 "An index of riot severity and some correlates." *American Journal of Sociology* 74 (March):500-5.
- Winch, Robert F. and Donald T. Campbell
1969 "Proof? no. evidence? yes. the significance of tests of significance." *The American Sociologist* 4(May):140-3.
- Zipf, George K.
1949 *Human Behavior and the Principle of Least Effort: An Introduction to Human Ecology*. Cambridge: Addison-Wesley.

ATTITUDE AND ACTION: A FIELD EXPERIMENT JOINED TO A GENERAL POPULATION SURVEY *

ROBERT BRANNON, GARY CYPHERS, SHARLENE HESSE, SUSAN HESSELBART,
ROBERTA KEANE, HOWARD SCHUMAN, THOMAS VICCARO, DIANA WRIGHT

The University of Michigan

American Sociological Review 1973, Vol. 38 (October):625-636

We investigate the relationship between survey-elicited attitudes toward open housing and willingness to sign and have published a petition consistent with these attitudes for a probability sample of the white metropolitan Detroit population. The action phase was carried out three months after the survey and in complete dissociation from it. The overall attitude-action relationship is high, and improves when we include additional belief items from the survey as predictors. Information on perceived reference group expectations, material self-interest, and educational level does not prove to be related to attitude-action consistency. We also present data on the actions of survey non-respondents, and on the proportion of people willing or unwilling to sign any petition. Hypotheses are offered for the more positive findings on attitude-action consistency in this research as compared to some past studies.

MOST attitude-action studies since LaPiere's (1934) have been negative or mixed in results. A recent comprehensive review concluded that there is "little evidence to support the postulated existence of stable, underlying attitudes within the individual which influence both his verbal expression and his actions . . ." (Wicker, 1969: 75).¹ These studies have not, however, concentrated on the validity of typical survey questions in general populations, but have often used unusual inquiries (for example, willingness to pose for opposite sex interracial photographs, as in DeFleur and Wes-

tie, 1958, and Linn, 1965); special college populations (most recent racial studies except DeFries and Ford, 1968); or settings such as classrooms which might influence responses in ways different from the household interview (Warner and DeFleur, 1969; Himmelstein and Moore, 1963). None of these factors necessarily invalidates evidence from such studies, but we are left uncertain of their implications for the validity of standard cross-section attitude surveys.

The present investigation began with a fairly typical attitude question on an important social issue ("open housing") which was administered as part of a larger survey to a cross-section sample of a major American city. We then carried out a separate action experiment with this same sample. Obviously no single question can definitively test predictive validity, but executing a complex action experiment using a probability sample of the general population is a formidable undertaking, and this study is an initial step in a necessary direction. Beyond assessing predictive validity in a real survey context, we have three aims. First, we are interested in whether the two attitudinal sides of the issue, which differ markedly in proportions of proponents in this population, show similar associations between survey response and real action. Second, we ask whether the perceived attitudes of two im-

* This experiment was carried out within the University of Michigan 1969 Detroit Area Study, directed by Irwin Katz and Howard Schuman. The present report was written by Brannon and Schuman. The other authors, all graduate students in the Detroit Area Study at the time, were co-designers with Brannon of the action experiment and also contributed to revising this report. The suggestions of an anonymous referee were valuable.

¹ Wicker covers thirty-two studies, but a number are marginal to the basic attitude-action problem. The sixteen studies reviewed in the area of racial attitudes include much of the major research up to 1969, beginning with the more naturalistic studies of LaPiere (1934) and Kutner, Wilkins, and Yarow (1952), and shifting to more elaborate experiments with college students, from DeFleur and Westie (1958) through Warner and DeFleur (1969). Other recent sceptical reviews are by Deutscher (1966, 1969).

portant reference groups (neighbors and spouses) affect the attitude-action association, again with the two different sides in mind.

The third aim is more complex. A basic variable in all attitude-action research is the degree to which one seeks congruence between the survey question (s) and the action stimulus. At one extreme, the goal is to use a variety of attitude questions to predict particular behaviors different from, but presumably connected to, the attitudes. At the other, one tries to measure a precise "behavioral intention" (Triandis and Davis, 1965) in the survey and then determine whether this behavior occurs as predicted in real life. Our data allow us to test the predictive power of several points along the continuum, from direct behavioral intention to sets of beliefs more indirectly relevant to the focal action.² At the same time, we also vary "action" in terms of the degree to which its public character is made salient to respondents.

OUTLINE OF THE ATTITUDE-ACTION EXPERIMENT

The present experiment was carried out within the framework of the 1969 Detroit Area Study. A two-stage probability sample of 640 white adults was interviewed in the metropolitan Detroit area in the spring of 1969. Interviews were conducted with the head or spouse of head, chosen at random, within each selected household. Persons seventy years of age and over were screened out, as were black households; but otherwise the sample is representative of the total Detroit city and suburban population.³ The

² Tittle and Hill (1967) insist that multi-item instruments always be used, but this does not confront the issue LaPiere and others have raised about whether hypothetical survey responses can simulate non-survey behavior. We do use a multi-measure approach in the course of analysis; but when several modestly intercorrelated items are combined from the start into a single complex index, its identity tends to become unclear. Validity, in the sense of understanding what one measures and why measures should be linked, is sacrificed for high reliability. Nor can one tell from such an index whether individuals are consistent in an absolute sense, as distinct from the relative standings provided by correlation.

³ For a full description of the sample design,

"action experiment" was carried out three months later with this same sample as subjects.

We chose the issue of "open housing" as a suitable one to examine from an attitude-action standpoint. On the one hand, it is an important social issue about which respondents are likely to have thought and formed opinions. On the other, we were able to create an action opportunity that could be totally divorced from the survey: a set of public petitions for or against open housing, supposedly for presentation to state leaders and publication in newspapers.⁴

Our main survey question, referred to as the housing law question, is shown in Table 1. The respondent was asked, during the hour-long survey, how he would vote on a referendum on open housing, with choices that we will henceforth refer to as (1) "Owner's Rights" and (2) "Open Housing."

geographic boundaries, and details of non-response, see Fields, 1969.

⁴ Open Housing is also used as the substantive issue in the research reported by DeFries and Ford (1969), but in that case the action measure was not separated from the interview situation.

Table 1. Housing Law Survey Question, with Percentages for Detroit White Adults

<p>Suppose there is a community-wide vote on the general housing issue. There are two possible laws to vote on: (PRESENT CARD TO RESPONDENT AND READ ALTERNATIVES). Which law would you vote for?</p>	
	Detroit Whites
1. One law says that a homeowner can decide for himself who to sell his house to, even if he prefers not to sell to Negroes.	82%
2. The second law says that a homeowner cannot refuse to sell to someone because of their race or color.	16
Neither (Volunteered)	1
D.K., N.A.	1
	100%
	N (640)

The marginal distribution for Detroit shows much greater opposition to Open Housing than support for it; both sides, however, included sufficient numbers to allow a prediction to action.

The action experiment involved the following steps. A member of a group of "concerned citizens" (actually graduate students dressed neatly and conventionally) circulated petitions supporting *either* "Open Housing" or "Owner's Rights" to each address at which we had earlier interviewed, as well as to some surrounding addresses for verisimilitude. To keep the action realistic, only one housing petition was presented to each respondent, in contrast to the forced choice provided in the survey. The petition was presented as a genuine attempt to influence the Governor and other state officials, and the circulators were unanimous in believing that all individuals contacted accepted the "realness" of the action situation and did not connect it with the earlier survey. The gap of three months, the difference in personnel, and the dissimilarity in presentation and content between the hour-long survey and the doorstep petition succeeded in dissociating the two completely.⁵

The action situation had one other important feature. We were concerned that some persons would hesitate to sign any petition from an unknown group, or indeed that some would refuse to open their doors. It seemed useful to identify chronic non-signers of this type, and to separate them from those who were refusing to sign because of the particular content of the petition. To handle this problem our "concerned citizens" first presented for signature a petition on what we had previously determined was a non-controversial issue in Michigan,

the pollution of the great lakes.⁶ Addressed to the Governor and Senators of the state the petition read as follows:

Yes, stop Pollution! As citizens of Michigan we are standing behind you 100% in your fight to stop the pollution of our lakes and rivers, which is now threatening fish, wildlife and PEOPLE. Don't give up your fight to stop this threat.

This screening device will be referred to as the "pollution petition," and results bearing on it will be reported below.

Following the pollution petition, the respondent was told: "Our group has also taken a stand on another public issue. Would you please read this petition and, if you agree, sign it?" The respondent was then handed either a petition endorsing Open Housing or a petition endorsing Owner's Rights (following a sample design presented below).⁷ The two petitions, each addressed to the Governor, read as follows (minus the bracketed phrases):

[Owner's Rights Petition:] We believe that

⁶ This petition, and others to be described, consisted of single sheets containing the heading "Michigan Council of Concerned Citizens," followed by the petition statement, which in turn was followed by twenty-four lines for signatures in two columns. To avoid making the respondent the first signer, a standard set of signatures was entered on the first six lines. Thus fresh petitions were used for each address. The pollution petition was introduced by the statement: "Hello, I'm circulating a petition to help stop the water pollution that's about to destroy most of the lakes around Michigan. We'd like you to support this fight by signing this petition, if you agree with it." [HAND PETITION TO R].

⁷ The procedures used to locate the interview respondents within households were as follows: Only one adult at each address had actually been interviewed, but the petition circulators obviously could not directly ask to speak with this person. Each circulator therefore had a description by age and sex of the household composition and of the target respondent. The circulator visited the house at the time most appropriate for locating the respondent. If the spouse answered the door, the pollution petition was presented directly; then, before proceeding, the circulator asked the spouse to call the respondent (i.e., "your husband/wife") to learn if he or she would sign it. The housing petition was then presented to the correct respondent, as well as to the spouse. Only actions from original respondents are used in this report. Houses that did not yield such a respondent are counted as "not at home" in Figure 1.

⁵ The success of the experiment obviously depended on deception regarding the real purpose of the action. This was one of two ethical problems that troubled the staff—the other being the circulation of some petitions that might reinforce opposition to open housing. Although absolute confidentiality has been maintained for all respondents and no participant has been injured in any way to our knowledge, deception itself is undesirable. We must justify it on the grounds that the experiment was important and that there was no other way to carry it out. Unlike most laboratory experiments, it was not practical to debrief respondents in this case.

it is the right of each homeowner to decide for himself to whom he will sell his house.

We urge you NOT to support any law which would force homeowners to sell to someone against their wishes, and if such a law is passed by the current legislature we ask you to veto it.

Signed:

[Open Housing Petition:] We believe that a person who has a home for sale Does Not and SHOULD NOT have the right to choose buyers on the basis of race, color, or religion.

We therefore urge you to support legislation this year which will end racial discrimination in housing, once and for all, in Michigan.

Signed:

To avoid any reminder of the earlier survey, the petitions were written with somewhat different wording than the two alternatives to the original housing law question. Unfortunately, in making these changes the term "religion" was added to the Open Housing petition—a blunder in retrospect. However, we are able, at least in part, to estimate the effect of this change in wording, and to control for it in our analysis. The other changes in wording are probably insignificant, but again in retrospect it seems both undesirable and unnecessary to have made even minor changes.

Those who signed the petition were

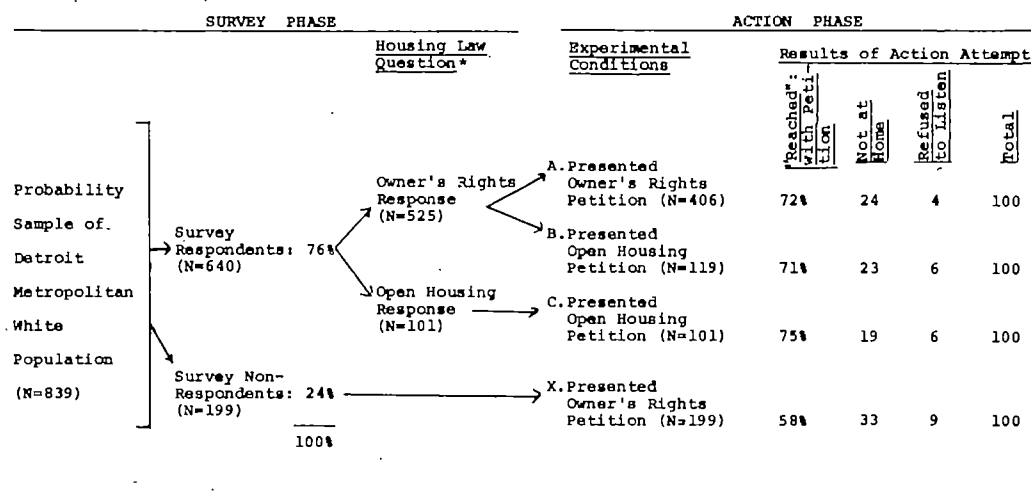
thanked and then asked to perform a stronger action: "We are also hoping to be able to take out some ads in the [two major Detroit newspapers], and reprint our petitions with lots of names of supporters. Would you be willing to be included in either or both of these if we do buy the ads?" Agreement to this action is referred to below as "signed with publicity."

Experimental Design

Figure 1 shows the design of the experiment. Those who had favored Owner's Rights in the survey were randomly divided into two groups: members of one group (Condition A) were presented the Owner's Rights petition, which was consistent with their survey response; members of the other group (Condition B) were presented the Open Housing petition, which expressed a position they had rejected in the earlier survey.⁸ Ideally we would have created a similar random allocation for respondents favoring

⁸ Examination of these two groups by age, education, sex, and several attitude questions show them to be virtually identical on each characteristic. The division of three-quarters (Condition A) and one-quarter (Condition B) was intended to allow at least one large group for finer analysis; in retrospect a fifty-fifty division might have proved more useful.

BASIC DESIGN OF ATTITUDE-ACTION EXPERIMENT



* Fourteen respondents (2%) were unable to make a choice on this question or said they would vote for neither law; due to the small size of this group they have not been included in subsequent tables or discussion, and percentages have been recalculated without them.

FIGURE 1

Open Housing in the survey; but since there were only 101, it seemed better to maintain the group intact for more reliable estimates of action; therefore all were presented the Open Housing petition (Condition C).

Not all respondents interviewed in the original survey could be located and presented a petition. The main problem was respondents not at home; only one call-back was attempted, since repeated call-backs in an action of this type could jeopardize credibility. A total of 144 respondents (28% of the three target categories combined) were finally classified as not-at-home for the action experiment. However, unlike most survey and experimental sampling, we have complete questionnaire information on these persons, and can thus assess the effect of their loss from the sample. The most germane information is their original response to the housing law question on the survey: as Figure 1 shows, the not-at-home loss is essentially the same over all three main experimental conditions ($\chi^2 = 0.76$, 2 d.f., n.s.). Comparisons on other survey questions also show no demographic or attitudinal differences from the total interview sample. The most reasonable assumption is that had these people been reached, they would not have changed our results.

More problematic are those who were contacted but refused to sign the pollution petition screening device and were therefore never presented a housing petition. Most of these simply refused to listen to the petition circulator, either not opening their door or closing it before the petitions were intro-

duced. Fortunately the number of such refusals is quite small (only twenty-eight respondents), and again Figure 1 shows them to be equally distributed over the three main action categories. We omitted these respondents from later results because apparently they were reacting to petitions in general, or perhaps simply to strangers. One might argue, however, that they should be considered attitude-action "inconsistents," in which case one could use the data presented in Figure 1 to recompute later findings. In either case, the number is so small that the changes in findings are slight and the changes in conclusions nil.

The addresses from the original survey sample that had not been successfully interviewed (survey non-respondents) were also included in the action design; for simplicity they were all presented the Owner's Rights petition (Condition X) the results of which will be discussed later. Both not-at-homes and refusals-to-listen during the action phase are somewhat larger among the survey non-respondents. This finding is not surprising, since these were the people not at home or unwilling to cooperate in the original survey.

RESULTS

Our initial findings on attitude-action consistency are presented in Table 2 and summarized below:

Extent of Consistent Agreement: Over two-thirds of the respondents on both sides of the issue (Conditions A and C) were willing to affirm their survey-elicited stand

Table 2. Consistency between Attitude Question and Action*

	Attitude Preference	Housing Petition Presented	Percentage Taking each Action			
			Refused to Sign	Signed Petition Only	Signed with Publicity	Total (N)
Condition A	Owner's rights	Owner's rights	15	26 85	59	100 (293)
Condition B	Owner's rights	Open housing	78	10 22	12	100 (85)
Condition C	Open housing	Open housing	30	12 70	58	100 (76) (453)

*This table includes all respondents in the survey who took a position on the housing issue and who in the action experiment were located and signed the Pollution petition.

by signing a petition. The percentages decline, though not remarkably in Condition A and C; when stronger action is requested: nearly three-fifths signed with publicity. We will discuss below design features that may have raised or lowered these percentages.

Comparison of Owner's Rights and Open Housing Respondents: Those espousing Owner's Rights in the survey showed more willingness to sign an Owner's Rights petition (85%) than did Open Housing proponents to sign an Open Housing petition (70%). The difference ($\chi^2 = 9.44$, 1 d.f., $p < .01$) suggests that those who characterize themselves in a survey as favoring open housing are more timid about affirming this in action (or conceivably less sincere in their survey responses) than those who oppose open housing. However, of those who did agree to sign the petition, Open Housing proponents were more apt to follow through with stronger action;⁹ and thus the two sides do not differ when signing with publicity is considered the criterion of action. We can offer no neat interpretation of this pattern of results, but are inclined to regard the two sides as essentially similar in attitude-action consistency.

Contradiction Between Attitude and Action. Although a surprisingly high proportion of the sample shows itself to be consistent with respect to agreement, such apparent affirmation could be exaggerated by the pressure to sign a petition in the action situation. It is essential to know what happens to respondents asked to sign a petition which contradicts their original position. (We know of no events in the Detroit area between the survey and the action phase which would have led to systematic real change in attitude over this period.) We have data on this for Owner's Rights respondents who were presented the Open Housing petition: In Condition B we see that 78% of this group refused to sign, and 22% did sign. Thus, most respondents did not simply sign whatever was presented to them. Moreover, the decline for signed with publicity is greater in Condition B than

in Condition C ($\chi^2 = 5.36$, 1 d.f., $p < .05$) or A ($\chi^2 = 1.50$, n.s.), suggesting that inconsistent action is weaker than action in line with attitude.

Overall Consistency. To estimate overall consistency for Owner's Rights respondents, one can simply average those in Condition A who acted consistently and those in Condition B who were consistent in withholding action. If the two conditions are weighted equally, then of those who had originally favored Owner's Rights, 81.5% acted by signing, and 73.5% by signing with publicity, as would be consistent with their attitude. These figures seem high indeed. Similar figures cannot be computed for Open Housing proponents because the necessary "Condition D" is not available.¹⁰

The above results do not take into account the wording change noted earlier between the Open Housing alternative to the survey question and the Open Housing petition. The latter referred to outlawing discrimination with respect to "race, color, or religion," while the former referred only to "race or color." Once the import of the discrepancy was fully recognized, we hypothesized that its effect would be to make Catholics and Jews—the groups more likely to be sensitive to religious discrimination—sign the Open Housing petition (Conditions B and C) more readily than they would have had we paralleled the survey question exactly. There are nonsignificant trends in this direction. Although Catholics and Jews do not differ from Protestants in their survey responses, they are more likely than Protestants to sign the open housing petitions in both Conditions B and C. (When signing

⁹ Based on signed petition vs. signed with publicity, Conditions A vs. C, ($\chi^2 = 3.51$, 1 d.f., $p < .10$). All Pearson χ^2 statistics are corrected for continuity, and all are evaluated as two-tailed.

¹⁰ Note that it is incorrect, however, to compare the 70% signing in Condition C with a chance expectation of 50% signing (as from coin-flipping). To calculate the "chance" rate of signing for the total population, one must average the percentages of signers in Conditions B and C in Table 2, but with each condition weighted by the proportion of the total survey sample it represents: 378 for Condition B (since that condition estimates what all Owner's Right respondents would do if presented an Open Housing petition) and seventy-six for Condition C. This weighted average is only 30%; hence without prior knowledge of survey responses, we expect only 30% of the total Detroit population to sign an Open Housing petition. The same considerations hold for signing with publicity.

with publicity is the criterion of action, these differential trends are quite small.) In Condition A, where no such change of wording is involved, there is no difference at all by religion in willingness to sign the Owner's Rights petition. All further analysis was carried out with a control for religion, but that level of control does not affect the findings to be presented and will not be included in tables below.

THE CONTRIBUTION OF OTHER SURVEY INFORMATION

We will now examine several types of additional information provided by the original survey in an effort to increase the basic attitude-action association and to interpret it. To conserve space, only the results for the stronger action, "signed with publicity," will ordinarily be presented; identical analyses using all signers yield the same conclusions.

Reference Group Pressures

Both the open housing issue and the doorstep setting for the action should make the opinions of neighbors particularly relevant. Therefore, after respondents gave their own preferences to the housing law question in the survey, they were asked which law "your neighbors right around here would want you to vote for." The great majority (93%) replied without qualification that the Owner's Rights position would be supported in their neighborhood. Respondents who favored Owner's Rights were even more unanimous in this perception, leaving too few cases to permit further analysis. Respondents favoring Open Housing (Condition C) show some variation: 67% perceived their neighbors as favoring Owner's Rights, but 33% thought that at least some of their neighbors favored Open Housing. But these groups did not differ in their behavior: of those perceiving their neighbors in unanimous support of Owner's Rights, 62% (of forty-seven cases) signed the Open Housing petition; of those perceiving some Open Housing support among their neighbors, 57% (of twenty-three cases) signed. The difference is slight, and opposite to that predicted by reference group theory.¹¹

¹¹ Fields (1971) provides evidence from this same

We did not ask respondents how their spouses would answer the housing law question, but later in the interview we did ask whether their spouses' views were generally "more favorable toward Negroes than yours, the same as yours, or less favorable." N's become quite small for this analysis, but again there is no sign that attitude-action consistency is higher when respondents perceive attitudinal support from their spouses.

Material Self-Interest

Consistency might be expected to be greater among those whose material interests supported (or were believed by respondents to support) their original survey response. Two tests of this assumption were devised. First, we hypothesized that home-owning respondents (82% of the sample) would be more consistent than non-owners in Condition A (Owner's Rights) and less consistent than non-owners in Condition C (Open Housing). A control of the attitude-action relationship by home ownership for each condition gave no evidence of such an effect. Results are slightly and non-significantly opposite to those predicted by this hypothesis.

Second, we hypothesized that consistency in Condition A would be highest in white neighborhoods within Detroit, since black pressure for housing can be presumed greatest there; less in white suburbs contiguous to Detroit; and least in white suburbs remote from the central city. The exact opposite ordering was expected for Condition C. In fact, analysis with this control shows only slight and non-significant ($\chi^2 = 0.87$, 2. d.f.) predicted variation by area for Condition A, and a non-significant trend ($\chi^2 = 1.51$) generally opposite to prediction for Condition C (least consistency for remote suburb respondents). Thus, for neither test do the material interests of respondents, as they are likely to perceive them, help explain inconsistency between survey response and action.

Cognitive Sophistication of Respondents

It seemed probable that consistency would survey on the degree of accuracy of perceptions of neighbors' expectations. However, only if the perceptions were related to respondent consistency would this become a relevant variable for the present analysis.

be higher among better educated persons. Such respondents might be more likely to integrate their attitudes in word and deed and to feel a need for consistency (Converse, 1964); they should also be better at understanding both the survey and the action situations. However, examination of the percentage signing at each of three educational levels (≤ 11 grades, 12, 13+) does not reveal significant improvement in consistency in any condition, nor parallel trends for the three conditions.

Behavioral Intention

All the predictions from attitude thus far have been based on the question in Table 1 about preference for one housing law or the other. We also asked respondents in the survey whether or not they would sign a petition supporting the law they favored: 94% said they would sign and 6% that they would not, with almost identical percentages for the two sides of the housing law issue. The distribution is so skewed that the question could not have made much difference in our basic findings, but we would expect that those who claimed to be willing to sign would be more likely to do so than those who claimed they would not.

Signing both with and without publicity are combined in Table 3 to maximize congruence between the survey question on behavioral intention and the action tested. The trend difference (11%) is in the expected direction, but not significant. Even if the difference were reliable, it is striking that three-quarters of the twenty persons

who said in advance they would not sign such a petition did in fact sign. Quite likely, the act of having signed the pollution petition encouraged respondents to sign a second petition with which they also tended to agree (Freedman and Fraser, 1966), though we saw earlier that it did not force many respondents to sign a petition with which they disagreed (Condition B). Thus consistency appears to be much greater when an important substantive issue is at stake for respondents; where the decision to sign must be based on other than substantive grounds, the pressures of the immediate situation understandably have more power to effect action or inaction. In any case, contrary to our expectation, the need to measure "behavioral intention" directly does not find much support in this experiment.

Other Racial Beliefs Related to the Housing Law Issue

Our measure of housing law preference (Table 1) was a single dichotomous item. It forced respondents to choose between alternatives, but did not measure the intensity, certainty, or meaningfulness with which they made the choice, nor the extent to which that choice fits their other beliefs and attitudes. At this point we will consider two types of racial beliefs which might be relevant to the housing law issue.

The first we measured through an open question on the consequences of residential integration: "If two or three Negro families move into a white neighborhood, do you see any sorts of problems as likely to arise?"

Table 3. Percentage Signing Owner's Rights Petition of Those Who Said in the Survey They Would or Would Not Sign (Condition A Only)

Behavioral Intention	Action		Total*	
	Refused to Sign	Signed, with or without Publicity	%	N
Yes, would sign Owner's Rights petition	14	86	100%	(266)
No, would not sign Owner's Rights petition	25	75	100%	(20)

*Seven respondents failed to answer the survey question on signing a petition, thus reducing the total from the 293 shown in Condition A in Table 2. There are too few cases saying they would not sign in Conditions B and C to allow testing.

Table 4. Percentage Signing and Not Signing by Belief about Whether Problems Occur when Negroes Move into a Neighborhood

	Action		Total	
	Not Signing, Signing with- out Publicity	Signed with Publicity	%	N
Condition A--(Owner's Rights Attitude- Owner's Rights Petition)				
Yes, problems when Negroes move in	34	66	100	(228)
No, not problems	76	24	100	(53)
Condition B--(Owner's Rights Attitude- Open Housing Petition)				
Yes, problems when Negroes move in	90	10	100	(60)
No, not problems	80	20	100	(20)
Condition C--(Open Housing Attitude- Open Housing Petition)				
Yes, problems when Negroes move in	46	54	100	(50)
No, not problems	30	70	100	(23)

Condition A: $\chi^2 = 29.08$, 1 d.f., $p < .001$.

Condition B: $\chi^2 = 0.61$, 1 d.f., n.s.

Condition C: $\chi^2 = 1.00$, 1 d.f., n.s.

One-fourth of the respondents felt that no problems were likely to arise, while three-fourths mentioned a variety of problems that might ensue, some due to the supposed behaviors of Negroes and others to the probable reactions of whites. The association between this variable and the original housing law question is slight for the entire survey sample ($\phi = .05$), but parallel differences in consistency occur within all three experimental conditions when the "problems" item is introduced as a control (see Table 4). The greatest differentiation appears for Condition A: 66% of those Owner's Rights respondents who believed that problems occur when Negro families move into a neighborhood also signed with publicity the Owner's Rights petition, as against 24% of those who thought no problems would occur.¹² We interpret the high degree of inconsistency for the latter group as a result of internal conflict between attitude and supporting beliefs on the part of the small subsample involved ($N = 53$), although one

can also regard the two questions as simply alternative indicators of a more general attitudinal construct and thus attribute the inconsistency to "measurement error." Our data do not allow us to distinguish these interpretations.

The differences in Conditions B and C, where the N's are smaller, are not significant taken separately; but in each condition our prediction of consistency rises noticeably if we consider responses to the problems question along with the original housing law question. However, for Condition C the improvement covers only a third (twenty-three) of the original seventy-six respondents, suggesting that much inconsistency in this condition is due to the adherence of many open housing respondents to a principle in conflict with their beliefs about the results of putting the principle into effect.¹³ Again, however, the improvement could simply be the result of better measurement because more items are added.

If Conditions B and C are treated as cate-

¹² The types of problems mentioned on the open-ended part of this question had almost no differential influence on whether a respondent signed; those who saw the problems as due to Negroes' misbehavior and those who blamed panic or overreaction by whites were equally willing or unwilling to sign the petition and to allow publicity.

¹³ Judging from the types of problems they mentioned, these people tend to blame whites rather than blacks for the "problems." Nevertheless, the conscious awareness of any problem seems to reduce willingness to sign an open housing petition, although an even lower rate of signing occurs for those few in Condition C who blamed blacks.

gories of one independent variable and problems/no problems as categories of a second, their combined effect in Table 4 on signing with publicity can be examined. The effect is additive (the likelihood-ratio χ^2 for rejecting the additive model is 0.03, 1 d.f., n.s.) and the partial association of each independent variable with signing is significant beyond the .05 level. The variable "condition" appears to be a good deal stronger than the variable "problems," as indicated by a comparison of the percentage difference in signing with publicity that each makes. The unweighted average difference due to "condition" is 47%, that due to "problems" is 13%. Since "condition" here represents response to the housing law question, this result simply shows that our main attitude item is a better predictor of the signing action than is the question on problems.

If beliefs about the consequences of residential integration are important in reinforcing attitudes toward the housing law issue, what about more general white beliefs about blacks? A four-item index of the tendency to believe in common negative racial stereotypes was used, high scores being obtained

by saying that whites have superior morals, more ambition, take better care of their families, and are less inclined toward violence than blacks.¹⁴ Table 5 presents the effect of this variable (trichotomized) within each attitude-action condition.

Within each condition the effect of the stereotyping index is in the direction expected: those high on stereotyping were more likely to sign an Owner's Rights petition (Condition A), and less likely to sign an Open Housing petition (Conditions B and C). But the effects are a bit less strong here than for the single question on housing problems, despite the greater differentiation and reliability provided by the four-item

¹⁴ The questions were worded as follows: "Regardless of how differences come about, do you think that being inclined toward violence is more true of whites, more true of Negroes, or about equally true of Negroes and whites." The same format was used for the other three items. Each item is scored 1 if the negative belief was held about blacks, 0 in other cases, yielding an index that runs from 0 to 4. Intercorrelations among the four items range from .21 to .36, and coefficient alpha for the four-item scale is .61. The association (Cramer's ϕ) between this index and the original housing law question is .18.

Table 5. Percentage Signing and Not Signing by Tendency to Stereotype^a

	Action		Total	
	Not Signing, Not Signing with Publicity	Signed with Publicity	%	N
Condition A--(Owner's Rights Attitude- Owner's Right Petition)				
High stereotyping (3 & 4) ^b	35	65	100	(128)
Medium stereotyping (2)	33	67	100	(57)
Low stereotyping (0 & 1)	53	47	100	(87)
Condition B--(Owner's Rights Attitude- Open Housing Petition)				
High stereotyping (3 & 4)	94	6	100	(36)
Medium stereotyping (2)	85	15	100	(27)
Low stereotyping (0 & 1)	85	15	100	(13)
Condition C--(Open Housing Attitude- Open Housing Petition)				
High stereotyping (3 & 4)	50	50	100	(16)
Medium stereotyping (2)	44	56	100	(18)
Low stereotyping (0 & 1)	39	61	100	(36)

^aSignificance tests--Condition A: $\chi^2 = 8.26$, d.f. = 2, $p < .02$.

Condition B: $\chi^2 = 1.80$, d.f. = 2, n.s.

Condition C: $\chi^2 = 0.58$, d.f. = 2, n.s.

^bStereotype index scores are shown in parentheses.

stereotyping index. This finding suggests that a general measure of racial beliefs adds less to predicting action than questions about beliefs tied more directly to the action issue.

INFORMATION ON SURVEY NON-RESPONDENTS

The non-respondents' addresses from the original survey were added to our action phase, and more than half (58%) were reached and asked to sign the Owner's Rights petition. Of these non-respondents, 51% signed with publicity, as against 59% of the survey respondents (Condition A, Table 2). If we assume that the non-respondents would have divided on the housing law issue in the survey in the same way as the respondents (84% favoring Owner's Rights, 16% favoring Open Housing), the 51% signing figure is exactly what we would expect.¹⁵ Perhaps survey non-respondents in our study do not differ appreciably, at least on the issue of open housing, from those actually interviewed. If so, the view sometimes put forth (Schuman and Gruenberg, 1970, Appendix A) that white non-respondents are disproportionately conservative on racial issues would be contradicted.

DISCUSSION

This study revealed greater attitude-action consistency than past research and reviews would suggest. Setting aside chance as an explanation, we can hypothesize for future research three reasons for the greater consistency.

1. The survey question and action dealt with an issue on which attitudes in the general population are better-formed and more firmly held than is true for much past research. It is likely that attitude-action consistency is substantially correlated with attitude stability over time.
2. The attitude object (housing laws) in

this research remained relatively constant between survey and action. When attitude is measured toward abstract social categories ("blacks" in general) and action is tested toward concrete individuals, the object itself changes. In this connection, it should be emphasized that the present research does not challenge the position that behavior is highly particularized, rather than flowing from a few master motives or attitudes. But this is true even within the same survey questionnaire (Schuman, 1972), and represents a different problem from that of whether we can predict from attitude item to non-survey action.

3. Household interviews may obtain a more valid measure of attitudes than questionnaires distributed in classrooms by faculty members. Respondents generally perceive professional interviewers to be neutral (Hyman, 1954), but this may be less true of student perceptions in a classroom or laboratory setting administered by faculty members. The balanced forced-choice structure of the original housing law question may also have improved the validity of the main attitude question.

We did not find perceived reference group expectations, material self-interests, or educational level to affect attitude-action consistency. These factors are no doubt important in shaping both attitudes and their expression, but perhaps here such influences were already incorporated into responses to the original survey question. Reference group expectations might have had more impact had they been called to the attention of subjects during the action phase, (e.g., neighbors' signatures on the petition) but without such explicit notice they seem not to have distinctive effects on action. We did find additional related belief questions useful in predicting action, though we cannot demonstrate that beliefs as such are critical; perhaps additional measures of the attitude itself (e.g., intensity) would have served as well or better. Finally, our findings on the limited value of measuring behavioral intention *per se*, and on the decrease in pre-

¹⁵ We must assume here that the 16% Open Housing non-respondents would sign the Owner's Rights petition at the same rate of inconsistency (22%) as indicated in Table 2, Condition B, and that the 84% Owner's Rights non-respondents sign at the rate of 59% shown in Table 2, Condition A. The expected signing figure on these assumptions is 51%.

dictive power as the action called for became stronger (or perhaps simply more distant from the attitude question), indicate the complexity of the terms "attitude" and "action." Exactly what we predict from, and what to, in attitude surveys needs further theoretical clarification.

REFERENCES

- Converse, Phillip E.
1964 "The nature of belief systems in mass publics. Pp. 206-61 in David E. Apter (ed.), *Ideology and Discontent*, New York: Free Press.
- DeFleur, Melvin L. and Frank R. Westie
1958 "Verbal attitudes and overt acts: an experiment on the salience of attitudes." *American Sociological Review* 23(December):667-73.
- DeFries, Gordon H. and W. Scott Ford
1969 "Verbal attitudes, overt acts, and the influence of social constraint in interracial behavior." *Social Problems* 16(Spring):493-504.
- Deutscher, Irwin
1966 "Words and deeds: social science and social policy." *Social Problems* 13(Winter):235-54.
1969 "Looking backward: case studies on the progress of methodology in sociological research." *American Sociologist* 4(February):35-41.
- Fields, James M.
1971 *Factors Affecting the Perceptions of Others' Racial Attitudes*, Unpublished Ph.D. Dissertation, University of Michigan.
1969 "Sampling report on the 1969 Detroit Area Study." Ann Arbor: Detroit Area Study, University of Michigan.
- Freedman, Jonathan L. and Scott C. Fraser
1969 "Compliance without pressure: the foot-in-the-door technique." *Journal of Personality and Social Psychology* 4(August):195-202.
- Himmelstein, Philip and James C. Moore
1963 "Racial attitudes and the action of Negro and white background figures as factors in petition signing." *Journal of Social Psychology* 61(December):267-72.
- Hyman, Herbert H.
1954 *Interviewing in Social Research*. Chicago: University of Chicago Press.
- Kutner, Bernard, Carol Wilkins and Penny R. Yarow
1952 "Verbal attitudes and overt behavior involving racial prejudice." *Journal of Abnormal and Social Psychology* 47(October):649-52.
- LaPiere, Richard T.
1934 "Attitudes vs. actions." *Social Forces* 13(December):230-37.
- Linn, Lawrence S.
1965 "Verbal attitudes and overt behavior: a study of racial discrimination." *Social Forces* 43(March):353-64.
- Schuman, Howard
1972 "Attitudes vs. actions *versus* attitudes vs. attitudes." *Public Opinion Quarterly* 36(Fall):347-54.
- Schuman, Howard and Barry Gruenberg
1970 "The impact of city on racial attitudes." *American Journal of Sociology* 76(September):213-61.
- Tittle, Charles R. and Richard J. Hill
1967 "Attitude measurements and prediction of behavior." *Sociometry* 30(June):119-213.
- Triandis, Harry and Earl Davis
1965 "Race and belief as determinants of behavioral intentions." *Journal of Personality and Social Psychology* 2(November):715-25.
- Warner, Lyle G. and Melvin L. DeFleur
1969 "Attitude as an interactional concept: social constraint and social distance as intervening variables between attitudes and action." *American Sociological Review* 34(April):153-69.
- Wicker, Allan W.
1969 "Attitudes versus actions: the relationship of verbal and overt behavioral responses to attitude objects." *The Journal of Social Issues* 25(Autumn):41-78.

VOLUNTARY ASSOCIATIONS AND MINORITY STATUS: A COMPARATIVE ANALYSIS OF ANGLO, BLACK, AND MEXICAN AMERICANS *

J. ALLEN WILLIAMS, JR.

The University of Nebraska-Lincoln

NICHOLAS BABCHUK

The University of Nebraska-Lincoln

AND

DAVID R. JOHNSON

The University of Nebraska-Lincoln

American Sociological Review 1973, Vol. 38 (October):637-646

The study focused on the voluntary associations of Anglo, Black, and Mexican Americans in Austin, Texas. A sample of 380 respondents provided the data. An information interview was used; trained interviewers coming from the same ethnic background as the respondent were employed. Ethnicity proved to be an important variable in predicting social participation, with Blacks having the highest and Mexican Americans having the lowest participation rate. Using multiple classification analysis, a number of structural variables were introduced as controls; and these variables, particularly education, were found to be responsible for the difference between Anglo and Mexican Americans. Blacks continued to have significantly higher rates of participation in voluntary associations after controlling on other variables. Both isolation and cultural inhibition theories can be found in previous literature to account for low participation rates among people having a subordinate status, and compensatory and ethnic community theories have been used to account for high rates of affiliation among these same groups. The findings from this study tend to cast doubt on isolation and cultural inhibition theories and to support compensatory and ethnic community theories.

THERE is mounting evidence that participation in voluntary associations enhances the likelihood of voting and actively participating in the polity (cf. Lipset, et al., 1956; Almond and Verba, 1963; Rose, 1967; Sallach, et al., 1972). Membership is typically seen as bringing individuals into contact with those who are already active, especially persons who join instrumental groups. Association with others is a catalyst for involvement. Furthermore, it is generally recognized that groups can be more effective in bringing about social change than can individuals working alone. In a related vein, several studies have pointed out that members of the working and lower classes who become involved with voluntary associations, e.g., Community Action Programs, and who

remain affiliated, acquire social skills highly useful in bringing about needed reforms (cf. Vanecko, 1969; Lyden and Thomas, 1969). For example, such persons learn how to present grievances to appointive bodies, acquire knowledge of how governmental agencies operate and which strategies will be most effective in bringing about a particular goal. Also, there is support for the proposition that social participation in associations produces a more favorable self-image and decreases feelings of powerlessness and isolation (cf. Erbe, 1964; Aberback, 1969; Zurcher, 1970).

Given these and other important consequences of participation in voluntary associations, it's important to know who participates and why. Some of the answers to these questions are already known. Indeed, as can be seen from the reviews of the literature presented in Babchuk and Booth (1969) and Curtis (1971), a substantial and essentially consistent list of findings has emerged from studies undertaken in this area. However, despite an impressive literature, some aspects of participation are not clear. The literature related to the extent of participa-

* This study is part of a larger study supported by the U.S. Department of Housing and Urban Development (H-1037 LIHD). We wish to thank J. Neils Thompson, principal investigator of the larger study, and our colleague John Lane, co-investigator for the Sociological Studies Section. This paper is a revision of a paper read at the annual meeting of the American Sociological Association, New Orleans, 1972.

tion among minorities is one of the most confusing. One often cited set of studies, Hyman and Wright's (1958, 1971), indicates that Black Americans are less likely to belong to voluntary groups than whites. On the other hand, several studies have reported that Black people are more likely to participate than whites (cf. Mayo, 1950; Babchuk and Thompson, 1962; Orum, 1966; Olsen, 1970). As for the nation's second largest ethnic minority, the Mexican American, many investigators claim that they seldom belong to voluntary associations (cf. Heller, 1966; Rubel, 1966; Briegel, 1970). Others have strongly disputed this proposition (cf. Romano, 1968; Montiel, 1970; Alvarez, 1971).

Reasons often given for the limited participation of minority groups fall within what might be called "isolation" theory. Essentially, the argument is that these persons do not participate because they are not integrated into the society. They lack the social skills necessary for participation and are not aware of the possible benefits of affiliating. In addition, minority persons may be barred from groups because of their ethnicity. While these reasons apply equally to Black and Mexican Americans, an additional argument maintains that Mexican American culture inhibits joining voluntary groups because the culture emphasizes loyalty to the home and family. Men are said to confine activities outside the family to informal relations with male friends. Women are said to be restricted to working in the home, visiting relatives, and attending church services.

Suggestions given for active participation by minorities can generally be subsumed under the heading of "compensatory" theory.¹

It has been noted that he (a Black person) may become an inveterate joiner in clubs or cliques with high-sounding names and much ritualism . . . these are attempted compensations for a lack of capacity for relatedness.

Whereas these writers tended to view this behavior as "pathological," Orum (1966:34) was the first to label the exaggerated tendency for Blacks to affiliate as "compensatory" in the more general sense of fulfilling needs not readily available in the larger society. He did so to describe Babchuk and Thomp-

son's (1962) rationale for the high rate of Black participation found in their research.

While this theory has been applied mainly to Blacks, its propositions are relevant to any segment of society which is subordinate socially. The contention is that those in lower status positions affiliate and participate in voluntary associations for prestige, ego enhancement, and achievement restricted or denied them in the larger society. In a recent paper, Olsen (1970) suggested an alternative thesis to account for active participation which he calls "ethnic community" theory.² In this theory, those in a given ethnic community develop a consciousness of each other and hence cohesiveness because of pressures exerted against them by outsiders. For example, the Polish Americans organized such groups as the Polish Roman Catholic Union and Polish Falcons (Lopata, 1964) and Italian Americans formed the Mazzini Society not only to maintain ethnic identity and help immigrants assimilate, but as pressure groups. In other words, through a sense of ethnic community, minority members form groups to deal with an alien environment and problems forced on them by the majority.

Isolation and cultural inhibition theories posit that minorities rarely participate in voluntary associations, while compensatory and ethnic community theories suggest that these characteristics generate much participation. The purpose of this paper is to provide data related to these seemingly contradictory theories. Despite the large literature on voluntary associations, no studies have compared participation rates of Mexican Americans with those of Blacks and Anglos. The present study includes all three groups and thus allows for a clearer test of the possible role played by minority status in relation to

² In earlier works, Blacks were not seen as creating pressure groups to change the condition of caste life, even though such groups existed. Yet many writers were aware that pressure groups might be effective were Blacks to develop an ethnic consciousness and press for change; indeed, a chapter in Myrdal (1944: 810-57) entitled "Negro Improvement and Protest Organizations" was devoted to the subject, and Lane (1959) suggested this idea in relation to political participation. However, the specific idea of ethnic community as an explanation for participation in voluntary associations was not articulated until Olsen did so in 1970.

¹ Myrdal, et al. (1944:952-55) suggested that Blacks were prone to join associations, particularly expressive ones, to compensate for discrimination. This view was reiterated some years later by Kardiner (1959:418) who observed that:

participation than is afforded by comparing a single minority group to the dominant group. Furthermore, though some studies have measured the effect of ethnicity while controlling for education, occupational prestige, and age, several other factors have been found to limit or enhance participation which have not been introduced systematically as controls. These variables are sex, presence of young children in the household, length of residence in the community, home ownership, and whether the individual is head of the household. Failure to control for these factors, or to seek their possible interaction with ethnicity, suggests potentially spurious associations between ethnicity and social participation. The present study includes all the aforementioned variables and uses a method which estimates the ethnic effect on participation, controlling for these other variables simultaneously.

SAMPLING AND DATA COLLECTION

The data for this study were collected in late 1969 and early 1970 in Austin, Texas. Primarily due to housing discrimination, the vast majority of Austin's Black and Mexican Americans are confined to certain clearly defined sections of the city. These ghettos and barrios are low-income areas. A sampling problem was efficiently locating Anglos comparable to the minority persons who would be drawn into the sample. After some preliminary testing, the procedure used was to randomly select households from all city blocks having an average rent of less than \$50 or the equivalent in housing value. This method provided sufficient variation within ethnic categories on the control variables while at the same time producing enough minority group members for data analysis.³

An information interview was used as the data-collecting instrument. The schedule was pretested on households chosen by the same method used for selecting the study sample. To minimize interviewer bias, trained inter-

viewers were used who were familiar with the city's low-income areas and whose ethnic descent matched those they interviewed (cf. Williams, 1964, 1968). Many interviews with Mexican Americans were conducted in Spanish, and Black interviewers were given freedom to "translate" the wording of interview questions into the language of the ghetto whenever they deemed it necessary. Using this procedure more than compensated for the possible lack of comparability in phrasing that might have ensued from a totally structured interview; it enhanced the likelihood of communication between the interviewer and the respondent on the meaning of the questions.

Respondents were interviewed in their homes. Both the household head and spouse (among married couples) were interviewed. The sample included 147 married household heads, their wives, sixteen unmarried male heads, and seventy unmarried female households heads, yielding a total of 233 households and 380 respondents. To the extent that there was a sampling error in the sample of household heads, there could be a correlated bias in the sample of their spouses. However, as we discuss below, precautions were taken in the data analysis which controlled for this possibility.

Aided recall was used to measure membership and participation in voluntary associations. To illustrate, respondents were asked, "Now I would like to ask you about possible kinds of memberships in different organizations (right now, not years ago). Do you presently belong to any church-related organizations?" This questioning procedure was repeated for each category of association; moreover, types of associations were mentioned to assist the respondent to recall and identify memberships. Under veteran's groups, for example, the American Legion, the American G.I. Forum, the Veterans of Foreign Wars (or their auxiliaries) were mentioned to insure that respondents would include all groups they were currently affiliated with.

METHOD OF DATA ANALYSIS

The first step in analysis was developing categories for the research variables. The dependent variable, membership in voluntary

³ It is possible that middle-class people living in low-rent areas differ in social participation from middle-class people living in medium- or high-rent areas. However, the participation rates of the higher status persons in the present sample are similar to rates reported in other studies using sampling procedures which included medium- and high-rent areas.

associations, was computed as a mean score, i.e., the average number of organizations belonged to by a particular population.⁴ The primary independent or treatment variable is ethnicity, Anglo, Black, and Mexican American. Home ownership, sex, and whether the respondent is a household head are obvious dichotomies. Presence of children under six in the household was divided into 0, 1, and 2 or more. Length of residence in the city was divided into 0 to 3 years, 4 to 9 years, 10 to 19 years, and 20 or more years. Occupational prestige and level of education were categorized in the manner suggested by Hollingshead (1957) except that those with no education were placed in a separate category.

The ethnic groups were compared by the mean number of voluntary associations belonged to by each group. Multiple classification analysis (cf. Andrews, et al., 1967) was used to examine the remaining differences in social participation among ethnic groups after statistically controlling for all other independent variables. This procedure treats each category of each independent variable as a dummy variable. Using additive multiple least-squares regression, it adjusts the mean of the dependent variable for each category of the independent variables by the amount of deviation from the total sample (grand) mean that is due to intercorrelation with other independent variables in the analysis. This method is equivalent to adjusting the means of the treatment group for intercorrelations with covariates in the analysis of covariance (cf. Cohen, 1968). In this instance, ethnicity is the treatment variable; and the other independent variables are the covariates.

During a preliminary examination of the data a statistically significant interaction ($P < .05$) between ethnicity and sex was discovered after covariance adjustment. Black and Mexican American women were found to have higher participation rates than males in their groups; whereas, Anglo men have a higher participation rate than Anglo women. Consequently, the final analysis was con-

ducted keeping the sex groups separate within the ethnic categories.

After controlling on the independent variables, an examination was made of the mean participation rates; and statistical tests of significance were made between ethnic groups within sex categories, e.g., Anglo males compared to Black males. The statistical significance of the difference in the adjusted means of the ethnic groups were estimated from F tests computed using the differences in explained variance of full and restricted forms of the multiple classification analysis (Bottenberg and Ward, 1965). The full model included all six ethnic-sex categories and all controls. In the restricted models, one ethnic contrast (e.g., Anglo males compared to Black males) was dropped from the equation and replaced by a single category designating both groups (e.g., one category including Anglo and Black males). The difference in the explained variance in the full model (which, for example, assumes that Black and Anglo males have different population means) and the restricted model (which, for example, assumes that Black and Anglo males have the same population mean) were tested with F ratios with 1 and 347 degrees of freedom.

FINDINGS

The mean participation rate in voluntary associations for all respondents is .897 with a standard deviation of 1.504. As can be seen from the class means (the actual rates of participation) presented in Table 1, Black men and women have higher rates of participation than their Anglo counterparts. Mexican Americans, regardless of sex, have lower rates than either Anglos or Blacks.

Presence of young children in the home, advanced age, short-term residence in the city, tenant status, limited education, and low occupational prestige are all factors which could inhibit social participation. These variables are somewhat differently distributed within the three ethnic samples and thus could have produced spurious associations between ethnicity and participation in voluntary associations. Furthermore, the Black sample was found to contain a somewhat larger proportion of female-headed households than the other two

⁴ The data were analyzed also using "belonging to one or more associations" and "not belonging to any associations." The findings are essentially the same.

Table 1. Mean Participation Rates in Voluntary Associations by Ethnicity and Sex before and after Covariance Adjustment^a

Ethnicity and Sex	Number	Class Mean	Adjusted Mean
Black American:			
Male	44	1.364	1.158
Female	75	2.013	1.955
Anglo American:			
Male	39	1.077	.547
Female	50	.560	.427
Mexican American:			
Male	80	.325	.441
Female	92	.370	.712

^aControl variables are age, education, occupational prestige, presence of young children in the household, length of residence in the community, home ownership, and whether the respondent is a household head.

groups. If being the head of a household is associated with active participation in organizations, then it was possible that the participation rate for Black females was enhanced by having a larger proportion of heads than the other two groups of women. Given these possibilities, the participation rates were adjusted by holding these variables constant. As can be seen from the adjusted means presented in Table 1, the participation rates for Blacks and Anglos are decreased slightly after controlling on the other variables; whereas, the Mexican American rates are increased. This indicates that the lower rates of participation among the Mexican Americans can be accounted for by the differential distribution of the other independent variables among the samples.

As mentioned, tests of significance were made between the mean participation rates among ethnic groups within sex categories. These tests were made after controlling on all other independent variables. As shown in Table 2, Black men and women have significantly higher rates of participation than the other two ethnic groups; whereas, there is no significant difference at the .05 level between Anglo and Mexican American men and between Anglo and Mexican American women.

Table 3 shows the relative contribution of each independent variable to variation in the mean participation rates both before and after controlling on all other independent variables. The square of eta for the ethnicity variable is .189 and beta-squared is .143. In

other words, before introducing the controls, ethnicity appeared to explain 18.9 percent of the variation; whereas, after controlling on these variables, ethnicity accounts for an estimated 14.3 percent of the variation in the dependent variable.⁵ Nevertheless, although beta-squared is slightly smaller than the square of eta, this introduction of controls provides greater assurance that ethnicity is an important variable in accounting for participation in voluntary associations. Inspection of the table's other findings shows that education is the only variable among those examined which equals ethnicity in importance. Taken together, the variables account for 38.3 percent of the variation in rates of participation.

INTERPRETATION AND FURTHER THEORETICAL CONSIDERATIONS

The above findings do not support isolation theory which suggests that minority peoples rarely participate in voluntary associations because they are set off from society. On the contrary, the findings show that Black people have a significantly higher rate of participation in associations than Anglos; this difference clearly is not due to education,

⁵ Andrews, et al. (1969:117-19) point out that the square of beta is not exactly interpretable as the per cent of explained variation in the dependent variable. This is why we have referred to beta-squared as an estimate. The important point for the present study is that this coefficient provides us with measures of the *relative* importance of the various predictors.

Table 2. Results of Tests of Significance between Mean Participation Rates in Voluntary Associations after Covariance Adjustment^a among Ethnic Groups within Sex Categories^b

Test Categories	Level of Significance ^c
Males:	
Anglo by Mexican American rates	n.s.
Anglo by Black American rates	.01
Black by Mexican American rates	.01
Females:	
Anglo by Mexican American rates	n.s.
Anglo by Black American rates	.01
Black by Mexican American rates	.01

^aControl variables are age, education, occupational prestige, presence of young children in the household, length of residence in the community, home ownership, and whether the respondent is a household head.

^bThe regression model used assumes values of the dependent variable are sampled independently of one another. As 147 of the households have both a husband and wife in the sample, this assumption is violated. Since there are only 233 independently sampled households, the degrees of freedom in the sample would be 233 rather than 380 if there were perfect congruence of husband-wife participation scores, and the efficiency of the significance tests would be reduced. While perfect congruence of husband-wife scores was not found in the sample, all F ratios were recomputed using the reduced degrees of freedom to yield a "conservative" test of the effect of violating the assumption of independence of dependent variable observations. All differences using 380 degrees of freedom significant beyond the .05 level were significant beyond the .05 level with 233 degrees of freedom, and all differences not significant at the .05 level using the full degrees of freedom also were not significant at the .05 level using the reduced degrees of freedom. The reduction in the efficiency introduced by this violation of a regression analysis assumption, therefore, does not affect the conclusions summarized in the table.

^cn.s. means not statistically significant beyond the .05 level and .01 means statistically significant beyond the .01 level.

occupation, length of residence in the city, presence of young children in the home, home ownership, age, or being head of a household.

Cultural inhibition theory, which for the most part has been applied to Mexican Americans, posits that minority-group culture contains values, norms, and beliefs which differ from the Anglo American majority and which tend to prevent social participation in voluntary organizations. The findings from this study indicate that Mexican Americans are not significantly different from Anglos in social participation once certain structural (not cultural) variables are taken into account.

Compensatory or ethnic community theory or both are supported by the findings. It is not clear whether compensatory theory is more salient than ethnic community theory in explaining participation or whether the two theories complement each other; a research design different from the one used

would be needed to disentangle these two theories. For example, information is needed on such things as the specific goals (formal and informal) of various organizations, the motivations of members for joining, the degree of ethnic identification among minority-group members. In other words, a useful study in this regard would focus on organizations and their members. The present study focused on the population at large and asked who belongs to associations and who does not. Nevertheless, it does offer leads for future research.

It is quite possible that a particular voluntary association can fulfill both compensatory and ethnic community needs. For example, an individual can gain a sense of importance or ego enhancement from participating in an association designed to combat racial discrimination. However, some organizations may be more clearly directed toward fulfilling compensatory needs (e.g., a social club);

Table 3. Relative Contribution to Variation in Mean Participation Rates by Eight Independent Variables before and after Covariance Adjustment^a

Variables	Eta-square	Beta-square
Ethnicity-sex ^b	.189	.143
Occupation	.045	.007
Education	.161	.177
Age	.024	.037
Home ownership	.035	.010
Length of residence in the city	.009	.005
Children under six in the household	.017	.004
Head of household	.003	.007

^a Covariance adjustment refers to controlling for all of the other variables listed in the table.

^b As mentioned, due to the statistically significant interaction between ethnicity and sex, sex categories within the ethnic groups were kept separate.

whereas others appear almost entirely directed toward changing aspects of the larger society (e.g., a pro-ethnic organization). With this in mind, participation by type of association was computed for each ethnic-sex category. The findings are presented in Table 4.⁶

The percentages of Black Americans belonging to different associations show no clear pattern of belonging primarily to "compensatory" or to "ethnic community" organizations. Social clubs, fraternal organizations, and recreational groups which might be thought of as emphasizing compensatory needs have about the same percentages belonging as do such possibly ethnic-community-oriented associations as pro-ethnic groups, neighborhood improvement organizations, and job-related associations. Church-related groups might be considered compensatory in nature; and it is true that such organizations can serve as a source of self-esteem, emotional expression, and respectability. However, numerous Black churches, especially those in urban areas, have operated as focal points for social action. Belonging to the P.T.A. (and in the present sample this

includes 88.9 percent of Black women with school-age children!) supports the compensatory explanation to the extent that education is perceived as a means toward upward mobility. It is plausible, however, that the observed interest in education also indicates a strong concern for the welfare of the ethnic community.

In the Southwest, Mexican Americans appear to be subject to discrimination similar to that which characterizes Blacks in all parts of the United States. On this basis, it seemed possible that Mexican Americans might react to discrimination similarly to Blacks and that the pattern of affiliating with groups might be parallel for the two groups. As shown, however, Blacks have a significantly higher rate of participation in associations; and, Table 4 shows that the pattern of affiliation differs as well. Unlike Black Americans, respondents of Mexican descent tend to concentrate in two types of organizations, church-related groups and the P.T.A. And, these two organizations are difficult to characterize as emphasizing either compensation or ethnic community to the exclusion of the other. Some scholars might argue that participation in church-related groups associated with the Catholic Church in the Southwest is almost exclusively compensatory. However, even if subsequent research bears them out, Mexican American membership in the P.T.A.

⁶ Table 4 gives the actual percentages belonging to each type of association. Since no controls are introduced for socioeconomic status or situation, comparison between ethnic groups should be made with caution.

Table 4. Ethnicity and Sex by Type of Organizational Membership

Type of Association	Ethnicity and Sex					
	Black American		Anglo American		Mexican American	
	% Male (N=44)	% Female (N=75)	% Male (N=39)	% Female (N=50)	% Male (N=80)	% Female (N=92)
Church-related	25.0	48.0	7.7	10.0	7.5	10.9
P. T. A.	6.8	26.7	12.8	12.0	12.5	18.5
Fraternal organizations	16.9	13.3	10.3	2.0	0.0	0.0
Social clubs	4.5	5.3	2.6	0.0	2.5	1.1
Recreational groups	9.1	2.7	10.3	2.0	1.3	1.1
Pro-ethnic organizations	11.4	4.0	0.0	0.0	0.0	0.0
Neighborhood improvement organizations	4.5	10.7	0.0	0.0	1.3	1.1
Job-related associations	6.8	10.7	20.5	2.0	3.8	0.0
Alumni organizations	6.8	8.0	7.7	2.0	0.0	0.0
Civic clubs	2.3	2.7	2.6	4.0	0.0	0.0
Veterans groups	4.5	1.3	2.6	0.0	0.0	1.1
Other	9.1	10.7	17.9	12.0	2.5	1.1

could express concern for the welfare of the Mexican American community as a whole.

CONCLUSIONS

From the present findings, it is reasonable to conclude that removing the socioeconomic disparity between Anglo and Mexican Americans would result in a Mexican American participation rate in voluntary associations equal to that of Anglos. Black Americans, on the other hand, have a significantly higher rate of social participation than Anglos. A major problem for future research will be to explain the different rates and patterns of affiliation between Black and Mexican American minorities. A possible key to this difference may be a difference in awareness of minority status, i.e., a difference in the extent to which minority individuals perceive themselves and their group as objects of collective discrimination. In a manner similar to the development of class consciousness as a requisite for class action, some Black Americans seem to have gained a sense of ethnic community, Black consciousness, which has led to action-oriented social participation.

It may be too late within the purview of the theories outlined above to study most European-linked minorities. For example, Greek and Italian Americans, apart from name identity, have already become largely assimilated and acculturated into the larger society. We can only speculate that at one time compensatory and ethnic community needs played a major role in hastening this process. However, compensatory and ethnic community theories lend themselves to test in the case of several visible minorities, apart from Blacks, existing in society today.

From an historical perspective and given our present state of knowledge of intergroup relations, it seems plausible that compensatory rather than ethnic community theory, until recently, provided a better explanation for Black participation. Although acculturated, Blacks were probably led by such factors as high visibility, subordinate status, and overt discrimination to fall back on each other and organize into groups to maintain their dignity and achieve personal fulfillment. Social integration, though viewed by many as an important goal, was not believed possible.

Black consciousness, surfaced with greater force through such successes as the Montgomery bus boycott organized by the S.C.L.C., lunch counter sit-ins by Black students, the development of CORE and the somewhat more recent emergence of militant groups like the Black Panthers. Together these have given greater force to ethnic identity and feelings of Black pride. If this process is occurring, and the evidence suggests that it is, then ethnic community theory should play an increasingly important role in helping us understand the participation of Black Americans in voluntary associations. We should expect a sense of ethnic community to become increasingly salient in fostering participation by other visible minorities, particularly American Indians, Mexican Americans, and United States residents from Puerto Rico and Cuba. These minorities are subject to practices of discrimination and prejudice comparable in some ways to those leveled against Blacks. All have begun to organize militant associations to express their identity and consciousness, e.g., the American Indian Movement, the La Raza Unida Party, the Young Lords. At present, these and similar organizations do not appear to have the broad base of support of such groups as the N.A.A.C.P., but these ethnic minorities appear to be on the threshold of changing their patterns of participation. The present study provides empirical evidence within a theoretical framework which might guide research on patterns of participation of such visible minorities and offers cues about what such research endeavors are likely to uncover.

REFERENCES

- Aberback, Joel D.
1969 "Alienation and political behavior." *American Political Science Review* 63 (March): 86-99.
- Almond, Gabriel and Sidney Verba
1963 *The Civic Culture*. Princeton, N.J.: Princeton University Press.
- Alvarez, Salvador
1971 "Mexican American community organizations." *El Grito* 4(Spring):91-100.
- Andrews, Frank, James N. Morgan and John A. Sonquist
1967 *Multiple Classification Analysis: A Report on a Computer Program for Multiple Regression Using Categorical Predictors*. Ann Arbor, Michigan: Institute for Social Research, The University of Michigan.
- Babchuk, Nicholas and Alan Booth
1969 "Voluntary association membership: a longitudinal analysis." *American Sociological Review* 34(February):31-45.
- Babchuk, Nicholas and Ralph V. Thompson
1962 "Voluntary associations of Negroes." *American Sociological Review* 27(October):647-55.
- Bottenberg, Robert A. and Joe H. Ward, Jr.
1963 *Applied Multiple Linear Regression*. Lackland Air Force Base, Texas: 6570th Personnel Research Laboratory, Aerospace Laboratory, Aerospace Medical Division, Air Force Systems Command.
- Briegel, Kaye
1970 "The development of Mexican-American organizations." Pp. 160-78 in Manuel P. Servin (ed.), *The Mexican-Americans: An Awakening Minority*. Beverly Hills, California: Glencoe Press.
- Cogen, Jacob
1968 "Multiple regression as a general data-analytic system." *Psychological Bulletin* 70 (December):426-43.
- Curtis, James
1971 "Voluntary association joining: a cross-national comparative note." *American Sociological Review* 36(October):872-80.
- Erbe, William
1964 "Social involvement and political activity: a replication and elaboration." *American Sociological Review* 29(April):198-215.
- Heller, Celia S.
1966 *Mexican - American Youth: Forgotten Youth at the Crossroads*. Cambridge: Schenkman Publishing Company.
- Hollingshead, August B.
1957 *Two Factor Index of Social Position*. New Haven: Mimeographed. Hyman, Herbert H. and Charles R. Wright
1971 "Trends in voluntary association membership of American adults: replication based on secondary analysis of national sample surveys." *American Sociological Review* 36 (April):191-206.
- Kardiner, Abram
1959 "Explorations in Negro personality." Pp. 413-23 in Marvin K. Opler (ed.), *Culture and Mental Health*. New York: Macmillan.
- Lane, Robert E.
1959 *Political Life: Why and How People Get Involved in Politics*. New York: The Free Press.
- Lipset, Seymour M., Martin Trow and James Coleman
1956 *Union Democracy*, Glencoe, Illinois: The Free Press.
- Lopata, Helena Z.
1964 "The function of voluntary associations in an ethnic community: 'Polonia'." Pp. 203-23 in Ernest W. Burgess and Donald J. Bogue (eds.), *Contributions to Urban*

- Sociology. Chicago: University of Chicago Press.
- Lyden, Fremont James and Jerry V. Thomas
1969 "Citizen participation in policy-making: a study of a community action program." *Social Science Quarterly* 50 (December): 631-42.
- Mayo, Selz C.
1950 "Age profiles of social participation in rural areas of Wake County, North Carolina." *Rural Sociology* 15(December):242-51.
- Montiel, Miguel
1970 "The social science myth of the Mexican-American family." *El Grito* 3(Summer): 56-63.
- Myrdal, Gunner, Richard Sterner and Arnold Rose
1944 *An American Dilemma*. New York: Harper and Brothers.
- Olsen, Marvin E.
1970 "Social and political participation of Blacks." *American Sociological Review* 35 (August):682-97.
- Orum, Anthony M.
1966 "A reappraisal of the social and political participation of Negroes." *American Journal of Sociology* 72(July):32-46.
- Romano, Octavio Ignacio
1968 "The anthropology and sociology of the Mexican-Americans: the distortion of Mexican-American history." *El Grito* 2 (Fall):13-25.
- Rose, Arnold
1967 *The Power Structure*. New York: Oxford University Press.
- Rubel, Arthur J.
1966 *Across the Tracks: Mexican-Americans in a Texas City*. Austin: The University of Texas Press.
- Sallach, David L., Nicholas Babchuk and Alan Booth
1972 "Social involvement and political activity: another view." *Social Science Quarterly* 52 (March):879-92.
- Vanecko, James J.
1969 "Community mobilization and institutional change: the influence of the community action program in large cities." *Social Science Quarterly* 50(December):609-30.
- Williams, J. Allen, Jr.
1964 "Interviewer-respondent interaction: a study of bias in the information interview." *Sociometry* 27(September):338-52.
- Williams, J. Allen, Jr.
1968 "Interviewer role performance: a further note on bias in the information interview." *Public Opinion Quarterly* 32(Summer): 287-94.
- Wright, Charles and Herbert H. Hyman
1958 "Voluntary association memberships of American adults: evidence from national sample surveys." *American Sociological Review* 23(June):284-94.
- Zurcher, Louis A.
1970 *Poverty Warriors*. Austin: The University of Texas Press.

ITEMS (Continued)

raphy of the black population of the United States and patterns of urban population distributions.

■ **William R. Morgan** is Assistant Professor of Sociology at Indiana University and Director of the 1972-73 Indianapolis Area Project (which, he tells me, was conducted in Louisville, Kentucky this year). He is interested in race and sex differences in aspirations and achievement, particularly as these relate to peer and parental influences. **Terry Nichols Clark** is Assistant Professor of Sociology and Director of the Comparative Study of Community Decision Making at the University of Chicago. He has divided his time for the past year between Harvard and Yale Universities and the Sorbonne. His research interests are reflected in the present article and, especially, in the broader aspects of community study made possible by the community decision-making program.

■ The lengthy list of co-authors for the "Attitude and Action" article results from its origin as a largely graduate student project attached to the 1969 Detroit Area Study. **Robert Brannon** is now Assistant Professor of Psychology, Brooklyn College, C.U.N.Y.; **Gary Cyphers** is Director of Evaluation and Research, Washtenaw County Community Mental Health Program; **Sharlene Hesse** is a doctoral candidate at the University of Michigan and presently a research fellow at the Ethnology Institute, University of Stockholm; **Susan Hesselbart** is Assistant Professor of Sociology, Florida State University; **Roberta Keane** is Instructor in Sociology at Assumption College, Worcester, Massachusetts; **Howard Schuman** is Professor of Sociology at the University of Michigan; **Thomas Viccaro** is Instructor in Social Work, University of New Hampshire; **Diana Pearce Wright** is completing a dissertation at the University of Michigan on real estate brokers and residential segregation.

■ **J. Allen Williams, Jr.** and **Nicholas Babchuk** are Professors and **David R. Johnson** is Assistant Professor in the Department of Sociology at the University of Nebraska-Lincoln. For the past several years Williams has been engaged in research pertaining to housing for low-income families. With **Harry J. Crockett, Jr.**, and **Clyde Z. Nunn**, he is beginning a national study of "Trends in Tolerance of Non-Conformity" sponsored by the National Health Foundation. Babchuk is engaged in research on social participation and primary relations in urban societies. He is also studying the relationship between membership on philanthropic boards and community power structure. Johnson is currently engaged in research on the dimensionality, validity, and reliability of Langner's Twenty-Two Item Scale of Mental Disorders. He is also investigating the relation of Catholicism, family structure, and fertility in eighteen Latin American nations.

■ December will feature Mirra Komarovsky's presidential address to the American Sociological Association. We look forward to an opportunity for more leisurely consideration of her thoughtful paper.

J.F.S.

 **SPRINGER**

Family Interaction

James L. Framo, *Chief, Family Therapy and Training Unit, Jefferson Community Mental Health Center, et al.* A "breakthrough" dialogue between 29 prominent family researchers and therapists. Included is a therapist's moving account of his attempt to treat his own family and free himself from the family's emotional system. 272 pp., 1972, \$9.50

Theories of Attraction and Love

Bernard I. Murstein, *Connecticut College, et al.* Substantive, scholarly papers by major authorities on why two people are drawn to each other. "Excellent picture of the state of knowledge and theory in the areas of love and attraction."—*Contemporary Psychology*. 192 pp., 1971, \$7.95

Personality Changes in Aging

Joseph H. Britton & Jean O. Britton, *Pennsylvania State University*. Pioneering nine-year study of community residents, aged 65 or more, that sheds light on why some persons decline with advancing years while others manage both to survive and to achieve a reasonably satisfying old age. 236 pp., 1972, \$7.95

Collective Education in the Kibbutz

Albert I. Rabin, *Michigan State University & Bertha Hazan, Sifriat Poalim (Tel Aviv)*. Comprehensive, authentic account of child-rearing, school practices, informal education and teacher training in Israeli kibbutzim, by ten educators who helped develop—and worked within—the system. 224 pp., 1973, \$6.95

Conflict Among Humans

Robert D. Nye, *SUNY, New Paltz*. Psychological and social factors that contribute to hostility among individuals and groups, drawing on the ideas of Skinner, Ardrey, Lorenz, and others, and illustrated in situations to which the reader can relate. 220 pp., 1973, \$7.50 (paper \$4.50)

Widely acclaimed and eminently readable:

THE PSYCHOLOGY OF DEATH

Robert Kastenbaum, *University of Massachusetts & Ruth Aisenberg*, *Children's Hospital Medical Center*. "As a critical view of findings and concepts in regard to death, this volume is without parallel."—*Am. Jnl. of Psychiatry*. "So well written . . . an intelligent reader picking up their book out of mere curiosity might easily read through—and finish the book the beneficiary of a deepening experience."—*Publishers' Weekly*. 509 pp., 1972, \$11.95

SPRINGER PUBLISHING COMPANY, Inc.
200 Park Ave. South, New York, N.Y. 10003
Send me:

- ☐ Framo, FAMILY INTERACTION, \$9.50
☐ Murstein, THEORIES OF ATTRACTION, \$7.95
☐ Brittons, PERSONALITY CHANGES, \$7.95
☐ Rabin & Hazan, COLLECTIVE EDUCATION, \$6.95
☐ Nye, CONFLICT, \$7.50 ☐ paper \$4.50
☐ Kastenbaum & Aisenberg, DEATH, \$11.95

Payment of \$— is enclosed. (Please add 35¢ for postage and handling. N.Y. residents: add sales tax.)

Name _____

Address _____

City, State _____ Zip _____

10ASR3

-7-

A SIGNIFICANT NUMBER IN SOCIOLOGY—

**If you are interested in compact,
readable accounts of original, basic research.**

*This series of 7 under the General Editorship of:
Albert J. Reiss, Jr., Yale University*

*Additional Monographs to be prepared under the General Editorship
of Sheldon Stryker, Indiana University.*

The ASA Rose Monograph Series now offers the following titles:

PATTERNS OF CONTACT WITH RELATIVES

Shella R. Klatzky, University of Wisconsin

ATTITUDES AND FACILITATION IN THE ATTAINMENT OF STATUS

Ruth M. Gasson, University of Cambridge
Archibald O. Haller, University of Wisconsin
William H. Sewell, University of Wisconsin

**BLACK STUDENTS IN PROTEST: A Study of the Origins of the Black Student
Movement**

Anthony M. Orum, University of Illinois

**LOOKING AHEAD: Self-Concepts, Race and Family as Determinants of
Adolescent Orientation to Achievement**

Chad Gordon, Rice University

BLACK AND WHITE SELF-ESTEEM: The Urban School Child

Morris Rosenberg, NIMH
Roberta G. Simmons, University of Minnesota

SOCIOECONOMIC BACKGROUND AND EDUCATIONAL PERFORMANCE

Robert M. Hauser, University of Wisconsin

DEVIANCE, SELVES AND OTHERS

Michael Schwartz, Florida Atlantic University
Sheldon Stryker, Indiana University

ORDER FROM:

**THE AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W., Washington, D.C. 20036**

Members, \$2.75 per title; Non-members, \$5.00 per title

Price for entire set of 7

Members, \$15.00; Non-members, \$30.00

American University Center.
U S I S-Calcutta.

FEB 12 1974

AMERICAN SOCIOLOGICAL REVIEW
Volume 38 Number 6

Komarovsky

Some Problems in Role Analysis

Maige
Maige

Politics of Birth Practices

Ball-Rokeach

Values and Violence

Hillips
Feldman

Social Integration and
Mortality

Freeman

Environment, Technology
and Administration

Form

Internal Stratification of the
Working Class

Hammond

Error in Ecological
Correlation

Sheingold

Social Networks and Voting

Klemmack et al

Non-Random Exogenous
Variables

McPhail
Miller

The Assembling Process

Comments

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit three copies, and retain a copy for your own files.* Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—doublespaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–4) that ..."] Incorporate within parentheses any brief phrase associated within reference. ["... have claimed that this is so (but see Jones, 1952:99 for conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semi-colons. ["... as many have noted (Johnson, 1942; Perry, 1947; Linquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

- Davis K.
1963a "The theory of change and response in modern demographic history." *Population Index* 29 (October):345–66.
1963b "Social demography." Pp. 204–21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.
- Goode, W. J.
1967 "The protection of the inept." *American Sociological Review* 32 (February):5–19.
- Moore, Wilbert E. and Arnold S. Feldman
1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt (ed.)
1962 *The American College*. New York: Wiley.

* Manuscripts will not be returned unless accompanied by a self-addressed, stamped envelope.

AMERICAN SOCIOLOGICAL REVIEW

DECEMBER, 1973

VOLUME 38, NO. 6

PRESIDENTIAL ADDRESS: SOME PROBLEMS IN ROLE ANALYSIS*

Mirra Komarovsky

Barnard College

American Sociological Review 1973, Vol. 38 (December):649-62

The history of Linton's and Parsons' role analysis exemplifies a familiar pattern in the intellectual development of humanistic sciences (Linton, 1936; Parsons, 1949, 1951; Parsons and Shils, 1951). A line of theory and investigation becomes widely adopted, eventually induces criticism and, in the course of the polemic, undergoes further development.

This paper attempts to trace several strands in this historical pattern of role analysis, first from a general viewpoint and then in relation to my recent study of masculine role strains.

I use the phrase "role analysis" instead of "role theory," advisedly. Role theory is likely to develop only with regard to the formal, Simmelian aspects of roles. Indeed, we have the beginnings of such theory in socialization into roles, role conformity, and deviance, and conditions moderating or intensifying role strain. As contrasted with these formal aspects, the obligations and rights that constitute the substantive content of roles would appear to span much of the subject matter of sociology. For example, the American president and the French prime minister, the factory manager in the United States and in the Soviet Union, kinship obligations of an adult male in a patri- or matri-lineage — any theoretical propositions concerning the normative content of these roles would hardly be distinguishable from the general fields of political, economic, or family sociology.

One other preliminary caveat. To limit the scope of this paper to manageable proportions, I shall deal only with institutionalized

roles, linked to recognized social statuses. Excluded, then, are many "regularities in interpersonal relationships" (Newcomb, 1966) or forms of interaction like "the family scapegoat," "the big wheel," or "the rebel," lacking the normative content of institutionalized roles (Popitz, 1972). Moreover, the emphasis will be primarily on social structural analysis rather than on symbolic interactionism of the descendants of Cooley and Mead. The latter have been concerned with such processes as the variable capacity for role-taking, the acquisition of roles, the emergence of informal roles, and the like (Rose, 1962). Another example of an interactionist approach to roles is Erving Goffman's *The Presentation of Self in Everyday Life*. Institutionalized roles are implicit in Goffman's analysis. His purpose is to reveal the processes in social encounters between role partners, seen from the vantage point of "... impression management, of contingencies which arise in fostering an impression, and of the techniques for meeting these contingencies" (Goffman, 1959:80). By contrast, for Gross, Mason, and McEachern, for Preiss and Ehrlich, Merton, or Goode, the interest lies in institutionalized roles.

The theoretical developments in role analysis selected for consideration have come from two sources. Some were caused by forces endogenous to the field. Others, more dramatic, reflected shifting emphases and polemics in the discipline as a whole. We shall consider them in turn.

Endogenous Sources of Change in Role Analysis

As long as the literature on social roles consisted of highly abstract essays, the con-

*Presidential Address delivered at the Annual Meeting of The American Sociological Association in New York City, August 27, 1973. The preparation of this paper was aided by NIMH Grant MH14618.

cept of role could remain imprecise and inclusive. The growth of empirical research and the need to operationalize the concept were bound to lead to conceptual clarification. This is not the place to review the rich vocabulary for role analysis proposed by various studies (Biddle and Thomas, 1966; Gross, Mason and McEachern, 1958; Preiss and Ehrlich, 1966). But this conceptual specification raised new theoretical problems. For example, Gross, Mason and McEachern challenge the postulate of role consensus and maintain that the degree of consensus about a given role in a social group is itself a variable (Gross, et al., 1958:43). Viewing consensus as a variable raises a series of theoretical questions concerning the causes and effects of varying degrees of consensus for the actors or social system. Similarly, I shall presently illustrate the theoretical yield of distinguishing between the objective fact of social disorganization, on the one hand, and the experience of role strain, on the other.

Other endogenous sources of change stemmed less from empirical research than theoretical continuities. For example, the very emphasis on the pervasiveness of role conflict may have led Robert Merton to observe that a degree of social orderliness, nevertheless, does obtain and, hence, to seek to identify the "social mechanisms" that minimize conflict (Merton, 1957).

The exogenous sources of change raise more polemical issues.

Exogenous Sources of Change in Role Analysis

Intellectual currents in the general field of sociology produced repercussions in role analysis. Several writers in the late 1950's and in the 1960's challenged, what they alleged to be, some untenable premises of Parsonian theory. Rolf Dahrendorf, C. Wright Mills, Alvin Gouldner, Dennis Wrong, Judith Blake, Kingsley Davis, and William J. Goode have all agreed on two issues. Using Wrong's phraseology (1961), Parsons' theory in the opinion of these critics, presented an overintegrated view of society and an oversocialized view of man.

These currents led a small minority of writers to urge abandoning the concept of role as not merely redundant but a distorting

dramaturgical analogy. In the words of one such radical critic:

The concept of role with its reliance on a view of man as a role conformer and of society as integrated system is a distortion. It is time that these inadequacies were recognized and the concept of role was abandoned by sociologists. *Without it* we are able to examine the relationships between expectations which members of different groups hold of the incumbents of a particular position in a more flexible and dynamic way . . . (Coulson, 1972:119).

Another states:

. . .one has but to omit the word "role" or the phrase "the role of" from passages selected at random from social science literature to discover that this often changes the meaning not at all, and on occasion clarifies it (Dewey, 1969).

I do not find this position persuasive. Indeed, once a critic states that members of groups hold certain "expectations" of the "incumbents of a particular position" it does not matter how we designate this phenomenon, provided we pursue the scientific quest it entails.

Such "expectations" (of "incumbents") raise a set of theoretical problems concerning variations from group to group and from time to time, their interrelations, the extent to which incumbents of particular positions actually conform to or deviate from them, and the like. The task of unraveling these relationships is central to the concept of role, a task well nigh unmanageable without it.

The great majority of recent critics, however, do not reject the concept of role altogether. They direct three major criticisms at social structural role analysis. I shall identify all three, though I have time to assess only the first two.

Three Criticisms of Social Structural Role Analysis

To begin with, role analysis is said to obscure and neglect the importance of individuality. "Is there no man behind the mask?" ask the critics. "Are men so programmed and passive that sociologists can afford to neglect the intrusion of self into the role or the

individual innovations which may inaugurate social changes?" (Bradbury et al., 1972). And again, "Are [roles] not played parts in a play being written in the act of being played? Do they not contain marks of individuality?" (Naegele, 1966).

The second criticism alleges that role conformity and stability have been over-emphasized and, conversely, that deviation, malintegration and social change have been minimized or neglected.

The third criticism focuses less on the extent of role conformity than on explaining such conformity as does exist. In the phrase of Blake and Davis (1964), traditional role analysis is characterized by the "fallacy of normative determinism," an assumption that the major explanation of conformity to roles, and, indeed, of the existence of social order, is to be found in the internalization of social norms. "Societies as we know them," write Blake and Davis,

are . . . filled with conflict, striving, deceit, cunning. Behavior in a given situation tends . . . to be strongly affected by individual interests, to be unpredictable from a knowledge of the norms alone.

Far from being fully determinant, the norms themselves tend to be the product of constant interaction involving the interplay of interests, changing conditions, power, dominance, force, fraud, ignorance, and knowledge (1964:464).

If some criticized this overemphasis on normative consensus, others questioned identifying such consensus with social integration and, the reverse implication, that normative dissensus was a major source of social disorganization. A study of stable working-class married couples with a high degree of value consensus was cited as a case in point. In that group, not anomie or dissensus but precisely the rigidity of role conformity in a period of social change, was judged to be a major cause of family disorganization (Komarovsky, 1967:335-8).

Similarly, Desmond P. Ellis cites several sources to support his contention that "shared values may lead to disorder and fragmentation" (1973:697).

Time will not permit the assessment of this third current of criticism save to note the swings in theoretical emphases that it reflects. The issue underlying role conformity is the

problem of order. Beginning with *The Structure of Social Action*, Talcott Parsons challenged the assumption that enlightened self interest, contract, and exchange were enough to ensure social order (1937:89-102). Instead, he stressed internalized, shared values as the cement of society. The recent criticism of this normative solution of the problem of order left a theoretical vacuum being filled, in part, by a return to a more sophisticated theory of exchange (Homans, 1961; Blau, 1967).¹ It would almost appear that the discipline develops in a series of discontinuous approximations. A particular theoretical orientation offers a useful, if one-sided analysis. It elicits criticism, and is superseded by an over-emphasis on the previously neglected variables. With regard to exchange, we may have come full circle when a reviewer of a book on the theory of exchange cautions sociologists not to exaggerate the role of calculated exchange in human behavior. Such exchange, the reviewer asserts, could be dominant only

in a wholly uninstitutionalized society in which all social relations are conducted ab initio and without established norms The institutionalization of roles into statuses, of power into authority or precedent into norm reduced the role of calculated exchange (Bierstedt, 1965).

William Goode, in his article, "A Theory of Role Strain" attempts to combine the two approaches to role conformity (1960). He questions whether normative commitment of individuals would ensure conformity in a complex, urban society. Goode proposes that a role relationship be viewed as a transaction or "bargain" in which the individual allocates his scarce resources among his various role obligations in light of the rewards or penalties he anticipates from his role partners. Goode does not rule out norm commitment but combines it with the theory of exchange by considering role obligations, demands, rewards, and penalties as the currency of exchange.

So much for an overview of the three main critical attacks on structural role analysis. I shall begin by assessing the first, the contention that role analysis neglects psychological variables. This allegation expresses an age-old issue in American sociology, reminiscent of

¹ Force, the third explanation of the social order, has also received renewed attention (Goode, 1972).

Floyd Allport's spirited attack in 1927 on what he termed, the "institutional fallacy," and George Homans' recent presidential address, "Bringing Men Back In" (1964). I have no illusions that the proposal I am about to make will put this persistent issue forever behind us.

The Interplay of Psychology and Sociology in Role Analysis – General Remarks

Let me begin by describing the place of psychology in Merton's structural and functional model of role analysis, as revealed in such works as "The Role Set: Problems in Sociological Theory" (1957) or "Sociological Ambivalence" (his paper with Elinor Barber, 1963).

Psychological variables enter Merton's model first, as epiphenomena, or the "mental side" of sociological facts. There is no trace of reification in the essays cited – it is the individual and not the role-set who acts, feels, or suffers. Merton and Barber, the authors of "Sociological Ambivalence," explicitly refer to the psychological experience of being pulled in opposite directions. They are concerned, however, with those characteristic psychological conflicts that are socially induced; and their purpose is to reveal "...the ways in which ambivalence comes to be built into the structure of social statuses and roles." In that effort Merton and Barber make a variety of psychological assumptions, e.g., that the helping professional, by virtue of his authority over his client, becomes an agent of frustration or that a state of anxiety in which a client seeks the help of a professional, makes him sensitive to the latter's behavior.

So far the psychological variables entered this structural model in the Durkheimian tradition of intervening variables, linking social facts, or as patterned psychic responses to social facts. Let us lay aside for the present George Homans' view that sociological propositions probably derive from the more general psychological theories. I would make a more modest and pragmatic case for psychology. More specifically, sociologists should try to make explicit the psychological assumptions implied in linking social facts. Let me illustrate the heuristic value of doing so. A relationship between two social phenomena may involve the psychological assumption that frustration induces aggression. Recogniz-

ing this proposition would reveal its problematic or contingent character. Frustration may, after all, result not in aggression but resignation or retreat. Such recognition, in turn, will stimulate the search for social determinants of various reactions to frustration and hence lead to a refinement of the original sociological generalization.

Neil J. Smelser has correctly stated that a sociological generalization invariably contains an "almost interminable list of assumptions about aspects ... outside the accounting model that do *not* vary" (1968). Far from suggesting so impossible a procedure as the questioning of all such assumptions, I propose the scrutiny of only the most relevant and proximate psychological propositions assumed to link social facts.

Psychological variables, however, enter into role analysis more actively, as additional independent variables increasing the explanatory power of the model to account for observed social behavior. I thus distinguish between psychological factors as intervening and independent variables.² Among the numerous studies actually combining sociological and psychological independent variables, I select William T. Smelser's laboratory investigation of problem-solving interaction. Smelser used personality tests to divide the participants into dominant and submissive types. The experiment was designed to include dominant and subordinate role assignments. The dependent variable was the problem-solving ability of the group. "The most productive group" concludes Smelser,

was composed of pairs in which the dominant subject (as rated on the personality test) was assigned the dominant role and the submissive role.... It was concluded that congruence of role and personality pattern within subject and complementarity of patterns as between subjects were major determining variables in cooperative achievement (1961:541).

For our purposes the significant fact was the combined use of independent psychological and sociological factors. Used together, these explained more of the variance in the dependent variable, i.e., group productivity, than either could have explained alone.

² Neil J. Smelser has made a similar distinction between intervening and, what he termed "primary" or "operative" variables (1968).

The potential of psychological independent variables to raise the explanatory power of an austere Durkheimian model carries considerable risk. It may prematurely halt a structural explanation. Take, as an illustration Rose Coser's analysis of Erving Goffman's concept of "role distance," designed to avoid that tendency (Coser, 1966). Goffman's concept originally referred to "actions which effectively convey some disdainful detachment of the performer from a role he is performing" (Goffman, 1961:110). Coser has persuasively used ideas of social structure to reinterpret an instance of role distance cited by Goffman, the use of humor by the surgeon in charge of an operation. The surgeon faces, she alleges, a sociologically ambivalent situation. He must control subordinates, while helping them maintain their poise. Far from manifesting detachment from his role, argues Coser, the surgeon's use of humor enables him to conform to his role even more effectively. Put more generally, in situations of sociological ambivalence, using humor can increase the effectiveness of the person in command and in effect reflect commitment to, rather than his detachment from, his role. Similarly, Coser questions whether the clowning of the eight-year-old boy on the merry-go-round illustrates defiance of role expectations. The normative features of this behavior become manifest when this situation is seen as involving a succession of statuses. The eight-year-old must demonstrate that he has outgrown kid stuff. Yet to refuse a ride on the merry-go-round would transform him into "a big shot." "Clowning" or "role distance" is his way out of the ambivalent situation. The boy on the merry-go-round is taking "role distance" from a status he aspires to but hasn't quite the right to claim (especially before others who possess a legitimate claim to it).

In taking issue with Coser's structural analysis, Robert A. Stebbins fails to make the necessary distinction between the two uses of psychological variables: as intervening variable and additional independent variable (1967). Stebbins, I would urge, rightly insists on spelling out the psychological assumptions linking sociological ambivalence and the "role distance" of the boy, clowning on the merry-go-round: "people do not like to make fools of themselves" or "people are concerned about the image they present to others or about their own self-conceptions." But he

adopts quite another theoretical position when he continues: "We cannot study *actual role behavior* [emphasis mine] very effectively from a structural and institutional point of view as it seems Coser is suggesting with respect to role distance" (1967:249). Here he argues for including psychological variables not as intervening links but as additional independent variables. Obviously, attitudes towards a given role differ among individuals. The incorporation of the actor's own interpretation of his role, built into the design of the study will, equally obviously, increase our power to account for variance in "actual [i.e. concrete] role behavior." Its increased power to account for variance is precisely the distinguishing feature of an independent as contrasted with an intervening psychological variable. The latter supplies a psychological explanation of the association between social facts. Thus, for example, the eight-year-old, finding himself in a sociologically ambivalent situation, clowns, because "people don't want to make fools of themselves." The intervening variable may explain but does not, in itself, increase the predictive value of a sociological generalization, as does introducing relevant psychological independent variables. To repeat, however, my earlier caution, the benefit of psychological independent variables carries the risk of premature "psychologizing." I shall presently illustrate from my own research how this temptation to stop at a plausible psychological explanation had to be fought. But, first, one final general observation on the interplay of sociology and psychology.

In his presidential address, "Bringing Men Back In," George Homans states: "I now suspect that there are no general sociological propositions, propositions that hold good of all societies or social groups as such, and that the only general propositions of sociology are in fact psychological" (1964:817). This being the case, Homans maintains, explaining sociological findings would entail deriving them from psychological propositions (1964:815).

Homans' critics generally base their opposition on the extreme complexity or downright impossibility of such a derivation (see, for example, Murray Webster, Jr., 1973; Peter M. Blau, 1970). But suppose we grant Homans' maximum position that "sociological propositions . . . can in principle be derived from, reduced to, propositions about the behavior of individuals" (1970:325). Even were this

thesis valid, it may not be crucial to theoretical sociology. Our sociological task is to account for aggregate phenomena on as general a level as we can. Homans makes no reference to the fact, though he would perhaps concede it, that without sociological concepts the generalizations about aggregates could hardly have been anticipated from psychological premises. I refer to propositions such as "the increasing size of organizations promotes structural differentiation along various dimensions at decelerating rates" (Blau and Schoenherr, 1971), or "the less cohesive a society the higher the rate of egoistic suicide." To take still another example, it is no accident that the traditional psychologist, using the model of the individual as a stimulating and stimulated organism, studied speech acquisition but left it to others to explore speech as a device in socialization (Newcomb, 1954).

But, once formulated, can these sociological propositions be deduced, as Homans surmises, from the more general psychological constants? The main issue, it would seem to me, is the utility of such derivation for sociology. The dominant sociological concern lies in comparative explanations (why social phenomenon A, rather than B). This being the case, it is as a rule, not the invariant and constant principles, though they may apply, but precisely the special ones that will illuminate our sociological interest in the difference between aggregates. I question, therefore, the utility of directing the process of explanation towards those invariant and constant psychological propositions. An explanation of an empirical finding is, after all, not an absolute, single act of derivation but a relative matter. Some investigators experience a sense of closure only when the process of derivation reaches a psychological proposition, but this may be a subjective reaction. Such relativity of explanation is, I take it, the import of P. W. Bridgman's words in *The Logic of Modern Physics*: "The essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests" (1927).

I said at the outset that I will report some actual research proings of the first two polemical issues. By way of summarizing the preceding discussion and anticipating the forthcoming illustration — this, in brief, is my answer to the critics who claim that role

analysis neglects "the man behind the mask." This danger is not intrinsic to the concept of role. Quite the contrary, role analysis allows us all the more clearly to identify "the intrusion of self into the role" because it encourages a full exploration of social structure and prevents premature "psychologizing." My answer goes beyond the familiar contention that social structural and psychological perspectives are complementary and, used together, can account more fully for concrete behavior than either perspective alone. I urged that a clearer distinction be made between intervening and independent psychological variables and that even a Durkheimian role analyst would do well to specify his usually latent assumptions about the most proximate or relevant psychological variables (or propositions) linking related social facts. It may be contested that the latter proposal returns me to Homans' position. The intervening psychological proposition, linking two social phenomena may in fact constitute a psychological explanation of their relationship. But my proposal, unlike Homans', does not stem from the thrust towards increasing the level of theoretical generality. Mine is a limited and pragmatic strategy aimed less at explaining than refining sociological generalization. Behind this strategy is the hunch that the psychological assumptions generally made by sociologists, represent not psychological constants, but contingent propositions. Hence, making the psychological assumptions explicit would stimulate the refinement of sociological propositions in the manner illustrated in the "frustration-aggression" case.

Turning to my research, I shall use its materials to illustrate two issues: the interplay of role analysis and psychological factors and the problem of malintegration and social change. With regard to the latter, I should like to show that role analysis, far from obscuring malintegration and social change, can lead to a direct analytical confrontation with these phenomena.

The Interplay of Psychology and Sociology in Role Analysis: The Study of Masculine Role Strains

The study I shall draw on was undertaken to ascertain the nature and extent of masculine role strains. The emphasis was on the distinctively male strains, those that men experience in a given social milieu, at a certain

stage of the life cycle, precisely because they are men and not women. The data consisted of elaborate case studies of a small sample of sixty-two men, randomly chosen from the senior class of an Ivy League college. In each case the search for strains covered a variety of statuses: student, son, sibling, male to male peers, male to females and others.

Role strain was defined, modifying William J. Goode's (1960) definition in two respects, as latent or felt difficulties in fulfilling role obligations or a sense of insufficient rewards for role conformity. The search for such role strains proceeded through several steps. We began by inquiring into the typical behavior of each person in a given sector, e.g. initiative on meeting with women friends at various stages of the relationship, and the range of variation in this behavior. We assumed that the actual behavior of each individual, even if patterned, is the complex result of multiple factors. We tried to determine only a few: his normative expectations in the given area, his actual preferences, his cognitive beliefs, whenever relevant, his perception of the normative expectations of his female friend and of her preferences, and his perception of the attitudes of "significant others."

In addition to interviews and schedules, each senior was given two psychological tests, the California Personality Inventory and the Gough Adjective Check List for "my ideal man" and "my real self."

The finding I have selected for this presentation permits us to examine the interplay of sociological and psychological variables. It concerns only one of several strains experienced by male seniors in relationships with women friends. Nearly one-half of the sample (45 per cent) expressed mild to acute anxiety over their failure in relationships with women, to live up to the traditional ideal of superior masculine assertiveness, determination, decisiveness, courage, independence, aggressiveness and stability in the face of stress. This cluster of "manly" virtues has been variously termed "ascendancy," "competence," leadership and the like. It would be a mistake to assume that the half of the sample who did not express anxiety on this score was composed solely of men who in fact exemplified those virtues. Some did; but other types included among the adjusted were, for example, men who enjoyed satisfactory relationships with stronger and supportive women.

Let us view the nature of the strain more closely. The troubled seniors were so classified, first on the basis of feelings of inadequacy admitted to in their detailed descriptions of relationships with women friends. This sense of inadequacy may have permeated the total relationship or surfaced in specific subroles, such as, in initiating contacts, in decision-making on dates, and in sexual behavior. Occasionally, their weakness originated in other statuses (an excessive dependence on parents, or low occupational ambition), but disrupted their male-female ties.

The troubled men not only felt inadequate but felt that they violated their own or their partners' normative expectations. It is generally believed that the ideal of masculinity has been changing among undergraduates. The emergence of the counter-culture with its disdain for competitive aggression and for "machismo" is one sign of the change. Studies including my own, show that the male ideal of masculinity now includes some qualities hitherto largely defined as feminine, such as sensitivity, patience and artistic appreciation (John P. McKee and Alex C. Sherriffs, 1959; Inge Broverman, et al., 1972). Nevertheless, the comparison of the Gough Adjective Check Lists, filled out by the seniors, for "my ideal" man and "my real self," reveals that the "feminine" virtues have not so much replaced as have been added to the familiar masculine stereotype. For most of these seniors the ideal man was still "assertive," "strong," "courageous," "aggressive," and "masculine." Of the traits men wished they could have but lacked (those attributed to the "ideal man" but most often missing from the description of "my real self") 40 per cent fell into the cluster of the "manly" traits similar to those enumerated.

That the role strain in question was not merely the result of an unfulfilled desire for greater power in interpersonal relationships was especially clear when the pressure for the traditional masculine behavior came from women friends. "She likes to be dominated," remarked one man about his current friend: "And she wants me to be more decisive. When I become pushy she does yield. But I believe in more equalitarian relationships and I would prefer one in which neither party had to hassle." "One thing that bothers me," declared another youth, "is the way they always picture men as having to be dominant and

strong. That puts a lot of strain on a man. I'd like to share things and you cannot dominate and share at the same time. But girls like a hard exterior in a man."

A small minority of the troubled men yearned to play the traditional role even though they were intellectually committed to an equalitarian ideology. These seniors experienced a double strain: low self-confidence vis-a-vis women and guilt over their psychological need to dominate them. One such senior explained perceptively: "Despite my egalitarian proclamations, tugging at my psychic strings is the thought that I am really most comfortable when I maintain a margin of dominance over a woman. My basic insecurity conflicts with my liberated consciousness making me feel like a double-talking hypocrite."

The use of psychological tests enables us to compare men troubled about their lack of assertiveness, with others at ease on this score. The "troubled" men had lower scores on aggressiveness, self-confidence, dominance, and several other traits in the ascendancy cluster. The full data will be published elsewhere, but a few comparisons are cited here by way of illustration. On the Adjective Check List for "my real self," only 14 per cent of the "troubled" men, as against 74 per cent of the "adjusted," scored above the mean for our sample on Self-Confidence. The scores on Self-Abasement show a similar contrast. Seventy-three per cent of "troubled" as compared with only 32 per cent of the "adjusted" fell above the mean of the total sample on Self-Abasement ratings. The California Personality Inventory shows the "troubled" group to be low on Dominance, with 52 per cent scoring below the mean for the sample on this trait. By contrast, only 14 per cent of "adjusted" men scored below the mean on Dominance.

Conceivably the lower self-confidence of the troubled men manifested in these tests, may have been the result rather than the cause of their failure to attain the masculine ideal. Granting such circular causation, one finding suggests that these psychological characteristics were rooted in childhood experiences and thus antedated feelings of inadequacy in heterosexual relationships. This telling finding pertains to parent-child ties. Relationships with each parent were classified into three categories: unsatisfactory, average, and good.

Men troubled on the score of assertiveness, in comparison with the adjusted men, reported a higher proportion of unsatisfactory relationships with their mothers and fathers. For example, 48 per cent of the "troubled" as against only 18 per cent of the "adjusted" men had unsatisfactory relationships with their mothers.³

The mode of role strain of these "troubled" men could be defined as a malfit between the idiosyncratic personality and social role. But again the temptation to stop with this plausible psychological explanation had to be resisted. If nothing else, the very extent of the strain, involving as it did nearly one-half the sample, raised the probability of social determinants.

I should like to propose the hypothesis that the seniors suffered from another mode of role strain, a socially structured scarcity of resources for living up to the norm of male ascendancy. Let me identify this type of strain before giving evidence of its relevance to the problem. Some social roles are difficult to fulfill, quite apart from scarcity of time or energy or from, what Goode termed, the general "overload" of role obligations. A clear case in point would be the problem of a doctor called on to treat a disease for which contemporary medicine has no cure. His failure to fulfill his role is caused neither by the competing claims of other roles, nor by personal inadequacy, nor yet by his low position in the social hierarchy. It might be instructive to compare various social roles in a given society with regard to their "utopian" components. Given the state of technical skills, the inherent risks, and other scarcities of facilities, some social roles present wider gaps between prescribed goals and available means than others.

In Merton's classical essay, *Social Structure and Anomie*, the distinction is posited between widespread aspirations in a given society, and differences in access to legitimate means of realizing them at various levels of

³The psychological profiles drawn by a clinical psychologist on the basis of the two psychological tests, suggest numerous hypotheses about the etiology of this strain and other psychodynamic processes characterizing the troubled as against the adjusted men. These suggestive results deal precisely with the "man behind the mask" and with the "intrusion of self into the role."

social stratification. In our case, the normative aspirations are those linked to a given role, with difficulties tending to be experienced by a sizeable proportion of actors in that role.

As for our seniors, the social advantages males still enjoy may, at first blush, argue against the hypothesis that social resources for exercising masculine assertiveness were lacking. Quite apart from whatever genetic sex differences may exist in assertiveness, the sexes are still socialized to maximize ascendancy traits in boys and mute them in girls. Certain masculine privileges remove decisions from contest and cede advantages automatically to the male. For example, the majority of young women in our study granted priority to their mates' careers and, in case of conflict, were prepared to scale down their own occupational aspirations. Again, the self-esteem of young women is probably still more dependent on their popularity with men than the reverse. The prerogative of initiating contacts, though it exposes the man to the risk of rejection, nevertheless implies and carries a degree of power. The woman's bargaining power declines more precipitously with age.

Given these masculine advantages, we might have expected a deviant minority but not nearly half the men to feel anxious about their inability to play the masculine role. But though the foregoing inventory of male advantages is no doubt accurate, this story has another side.

The men gave abundant illustrations of perceived inadequacies in intellectual and emotional relationships with women. After all, these women were generally also college students, if anything more rigorously selected in terms of their high school performance and of nearly the same age (*Princeton Alumni Weekly*, 1971). The trend towards earlier cross-sex interaction has increased the dependence of the young man on the emotional support of his female friend as against the male clique. Our study of self-disclosure revealed that for all aspects of the self, especially in the most sensitive area, the closest female friend was the preferred confidante over closest male friends, siblings of either sex, or parents. The ability to grant or withhold this expressive function gives some advantage to the female. Moreover, the increase in pre-marital sexual experience of female undergraduates creates stress because it may challenge the still-dominant expectation

that the male be the more sexually experienced partner (Kaatz and Davis, 1970). Finally, the women's liberation movement leads an increasing number of college women to challenge traditional male privileges, which are no longer ceded as a matter of course but must be contested and won by personal strength.

These challenges confront the male at a vulnerable stage of his life. He is still economically dependent on his family. Neither his role as student, nor as part-time worker can bolster his sense of "manhood" in a culture that anchors it so largely in economic independence and occupational success. Superior physical strength is not an effective resource in a milieu that censures its use with women. In time these men may in fact acquire superior power and status since present family life generally restricts the access of married women to independent sources of accomplishment, status, and economic power. These socially-rooted advantages and privileges, later in life, will probably give many of these men the desired edge. But during college years, at the modal age of twenty-one, the ideal of masculine leadership was not attainable for a large proportion of the sample.

So far the difficulty of conforming to the ideal of masculinity has been considered in relation, first, to the special handicap of some personality types and, second to a socially-structured scarcity of facilities for role fulfillment. But the presence or absence of such strains is affected also by other structural features of the social environment, not directly derivable from psychological theory. These are features that make it more or less difficult to live up to role requirements or make the failure to do so more or less traumatic. The latter depends on the visibility of role performance to "significant others" and on availability of cultural alternatives, i.e., other esteemed roles for those not successful in heterosexual relationships. To cite a few illustrations, interviews with students who transferred from other colleges suggested that of the structural variables affecting the extent of stress, the ratio of males to females may be a dominant factor, either raising or lowering the bargaining position of each sex. Opportunities for relatively informal contacts with women is another factor. Even a timid youth may bolster the courage to invite a coed in his class for a cup of coffee; whereas, a more

formal dating system puts greater demands on self-assurance in initial contacts. A large and impersonal campus may shield a timid youth from surveillance of his strategy with women or conceal his failures from his peers. The size of the campus has other implications in interplay with psychological factors. The more specialized the psychic needs of the individual, the larger must be the pool of eligibles to increase the chances of finding congenial partners; and social environments vary in size and access to eligible mates.

I have used my study to illustrate the interplay of sociological and psychological factors in a particular case of role strain. The second and final theoretical problem to be examined concerns malintegration and social change. The remaining time is too short to deal with this issue in a comprehensive and orderly manner but a few examples from the work in progress will suggest the links between role strain and social change.

Role Strain and Social Change – The Study of Masculine Role Strains

That role strain may be a source of social change has been recognized by many sociologists (e.g. Parsons, 1951:280-3). However, the overriding interest of writers on role conflict has been in mechanisms that hold conflict in check. There is no escaping the familiar lament that the problem of social change has been neglected.

As in other areas of role analysis, here also some distinctions will open the way for productive investigations. The first such distinction is between the existence of social disorganization, and the strain experienced by the actor. We have been too ready to identify the objective condition of malintegration (e.g. status discrepancy versus status crystallization, conflicting obligations, ambivalence) with felt strain (Treiman, 1966). The distinction gives rise to the question: What will determine whether a given instance of disorganization will be experienced as a stressful situation? Vulnerability to identical forms of disorganization may vary with the actors. This becomes, then, a special case of the well-recognized problem of differing reactions to similar objective stimuli. The concept of relative deprivation and reference group theory, applied to this area may prove a fruitful source of a whole range of hypotheses.

An illustration of differing reactions to an identical role conflict is found in Wallin's replication at a western university of research done earlier at an eastern college (Wallin, 1950; Komarovsky, 1946). In both studies an identical proportion of women reported that norms for academic and occupational success conflicted with norms for the traditional feminine role. The interview materials led Wallin to conclude that the conflict was not as stressful on the western campus where the respondents were primarily oriented to the traditional role, as it was in the eastern sample which contained a higher proportion of career-minded students.

But if similar forms of disorganization may be differently experienced, it is also true that forms of disorganization vary in their potential for stress. For example, dissensus over norms regulating the interaction of role partners would presumably be more disruptive, and hence more stressful, than similar dissensus in imagery peripheral to such interaction. It is not surprising that Preiss and Ehrlich found relatively low consensus in such images held by policemen about their occupational role as advancement opportunities, freedom to express feelings, and the like. The same research revealed higher consensus on instrumental than on expressive role expectations (1966:170).

Similarly, the normative components of most roles are ordered in importance from mandatory to discretionary. Insofar as the former, the core elements, tend to be more deeply internalized or more severely sanctioned than the latter, difficulties in conforming to them will be more stressful.⁴

So much for a few illustrative hypotheses suggested by the distinction between the existence of social disorganization and the actor's experience of stress.

Given the experience of role strain, what are its implications for social change? What kinds of strain, and under what conditions, will dissolve traditional attitudes and catalyze change?

Weber's idea of elective affinity is consistent with our data. Weber maintained that attitudes tend to be adopted by groups whose interests they serve (Gerth and Mills,

⁴In addition to such socially-mandated priorities, there are, of course, individual and idiosyncratic hierarchies (Ralph Turner, 1968).

1965:62-4). This elective affinity was observed in the endorsement by young men of some particular element in the new ideology of sex roles that served their interests, and the rejection of those new attitudes that threatened their power or self-conception. To illustrate, our society is moving towards less sharply differentiated and more symmetrical or partnership conceptions of masculine and feminine roles. The seniors, facing expensive and extended professional training and desiring to marry, were apparently able to accept some elements of the new partnership roles. Two-thirds of the seniors expressed no objection to being supported through graduate school by a working wife. They discarded the traditional view that economic dependence on a wage-earning wife violated the masculine role.

Significantly, seniors who were engaged (or committed to marry their current mates) sanctioned such a pattern more often than others still "playing the field." The latter, remote from the economic problems of early marriage, claimed that economic dependence on one's wife would give marriage a bad start and undermine their self-respect.

Whereas some two-thirds of the sample saw no objection to temporary economic dependence on one's working wife, the attitude that the husband must be the superior achiever in the occupational world was upheld by an overwhelming majority. Only seven per cent favored a marriage in which husbands and wives symmetrically shared economic and family obligations. Thus, changes in attitudes that serve one's interests are clearly more readily accepted than others entailing a greater sacrifice of power or more deeply identified with self-esteem.

The mode of strain is another factor helping or hindering social change. The study distinguished five modes of role strain,⁵ and each may have its distinctive relationship to social change. For example, sociological ambivalence, a subtype of role conflict, appeared to have a conservative influence, supporting the status quo. This effect seemingly confirms

the paralyzing influence of "cross-pressures" observed in other spheres.

The study suggested other hypotheses concerning the potential of role strain to effect change. Some seniors were attracted ideologically to more egalitarian sex roles which would have been functionally appropriate at their stage of life and in their milieu. However, male superiority in the occupational, political, and cultural institutions in the total society continued to form their masculine self-image and hindered attitudinal change. In general a new and appropriate adaptation in one institutional sector, or at one stage of life, might be readily accepted were it not for the fact that the traditional role is still rooted in more strategic institutions.

Whether or not an experience of stress will induce a change in norms or ideologies is affected by the actor's "definition of the situation." The tendency to place blame for role strain on personal inadequacy has, no doubt, a conservative effect. On the other hand, deflecting the blame from self to some feature of the social order activates the potential for change. In its macrosociological aspects, this is, of course, a basic problem of Marxian theory, i.e. the conditions hindering or facilitating proletarian class consciousness. Political sociologists address this general problem when they probe conditions under which economic or status frustrations lead to radical voting behavior or to social movements (Portes, 1971).

The final theoretical problem to be raised deals with the discrepancy between professed values and actual behavior in relation to social change. That words and deeds are not always in accord has been recognized throughout history. In American sociology, the interest in this discrepancy first surfaced in a methodological debate over the validity of questionnaires, especially in the area of race prejudice. Richard LaPiere (1934) questioned the usefulness of opinion and attitude surveys in predicting actual behavior. Other sociologists, most particularly C. Wright Mills, Robert Merton and Irwin Deutscher, identified this disjunction between verbal expressions and actual behavior, as a theoretical problem in need of systematic investigation. Mills stated that we needed to know "how much and in what direction disparities between talk and action will probably go" (1940). Merton speculated that "Northerners treat Negroes

⁵ The five modes are conflict, anomie or ambiguity, malfit between idiosyncratic personality and role requirements, low rewards for role conformity, and "socially structured insufficiency of resources for role fulfillment (other than those caused by scarcity of time or energy).

less 'favorably' than they talk about them and that Southerners talk about Negroes less 'favorably' than they treat them." He went on to inquire: "May we assume the amount and direction of spread between opinion and action to be relatively constant for members of different groups? . . . To my knowledge no systematic research on the problem has been carried out" (1940). Deutcher, still referring to the race problem and concerned with strategies of change, raised the question of "behavioral consequences of attitude change" and "attitudinal consequences of behavioral change," the latter as revealed, for example, in deliberately integrated housing (1970). To formulate the problem more explicitly and more generally, the disjunction between words and actions must be studied not merely in a cross sectional perspective but in relation to social change. In a period of change, verbal expressions of new ideologies, attitudes, or values sometimes anticipate and sometimes lag behind, corresponding changes in behavior. What are the manifold determinants of the direction of this disjunction? Phrased still more broadly, when do social structural changes precede and when do they follow related cultural changes? In the study of masculine roles, some seniors exemplified a familiar disjunction. They learned to pay lip service to modern sex role ideology, all the while remaining traditional in actual behavior. On a liberal campus, with sanctions against "old-fashioned" attitudes, this discrepancy was expected. Apposite is Erik Erikson's statement, made in another connection, that "it takes a much longer time to emancipate what goes on deep down inside us— that is, whatever . . . [has] become part of our impulse life and our identity formation—than the time it takes to re-define professed values . . ." (1965). Presumably those deeper layers of identity affect our actual behavior even as we learn to mouth the fashionable new beliefs.

That much was to be expected. But we encountered also the reverse pattern, that is, men who were egalitarian in behavior but traditional in ideology. A few of the latter admittedly surrendered to the superior power of their mates. But others in this category made a variety of novel egalitarian adjustments in behavior, seemingly unaware that they had departed from their professed traditional beliefs. The emotionally-charged stereo-

type of masculinity endured in the face of their contrary new adaptations.⁶ It is likely that in our case this discrepant combination of "modern" behavior and traditional ideology presupposed a satisfactory relationship with the woman. The reason is two-fold. A satisfactory relationship both increases the motivation to serve the interests of the mate and tends to conceal the full significance of these accommodations. A man adhering to traditional ideology is likely to view the egalitarian male as emasculated and defeated. Hence, the very self-esteem that the traditional man enjoys in a satisfactory relationship helps maintain the fiction that his egalitarian adjustments are minor changes, dictated by practical exigencies, which in no way violate masculine leadership. Erikson's insight about the resistant feelings of identity must, hence, be qualified by the human capacity to re-define situations and to maintain fictions. One is reminded of Sumner's insight that mores tend to be changed by ritual, by small variations in behavior and habit and, eventually, by changes in attitudes. Suggestive as these insights may be, the task of studying the disjunction between professed beliefs and actual behavior *in the perspective of social change* is clearly still ahead of us.

This paper singled out two issues of contemporary polemics, which, having originated in the general field of sociology, were reflected in the area of role analysis. Stated in the form of criticisms of existing orientations, the first referred to the alleged neglect of individuality in role analysis, and, the second, to a similar neglect of malintegration and social change.

I attempted to show that these dangers are not inherent in role analysis. Partly, perhaps, as a constructive result of recent criticisms, we can see that role analysis does not at all require us to neglect the "man behind the mask." If anything, it allows us to explore the social structure fully and thus specify more precisely the interplay of psychological and structural variables. Secondly, if role analysis

⁶This phenomenon is well recognized in studies of prejudice. Unfavorable stereotypes of minority groups or of older workers have been known to persist despite favorable encounters with members of these groups, who were defined as exceptions to the rule. But again, our emphasis is on social change and, more specifically, the differing rates of change in expressed attitudes, as against actual behavior.

begins with the search for the routine social patterns, it does not end there. Far from obscuring conflict, deviation, malintegration and social change, role analysis leads directly to the exploration of these phenomena. These broad theoretical issues were discussed on a general plane, and as reflected in my current research.

REFERENCES

- Biddle, Bruce J. and Edwin J. Thomas (eds.)
1966 *Role Theory*. New York: John Wiley and Sons, Inc.
- Bierstedt, Robert
1965 Review in the *American Sociological Review* 30(October): 789-90.
- Blake, Judith and Kingsley Davis
1964 "Norms, values, and sanctions." P.464 in R.E.L. Faris (ed.), *Handbook of Modern Sociology*. Chicago: Rand-McNally.
- Blau, Peter M.
1967 *Exchange and Power in Social Life*. New York: Wiley.
1970 "Comment." Pp. 329-39 in R. Borger and F. Cioffi (eds.), *Explanation in Behavioural Sciences*. Cambridge University Press.
- Blau, Peter M. and Richard A. Schoenherr
1971 *The Structure of Organizations*. New York: Basic Books.
- Bradbury, Malcolm, et al.
1972 "The man and the mask: a discussion of role theory." Pp. 41-64 in J. A. Jackson (ed.), *Role*. Cambridge University Press.
- Bridgman, P. W.
1927 *The Logic of Modern Physics*. New York: Macmillan and Co. Cited in Robert K. Merton, "Recent French sociology." *Social Forces* XII (May) 1934:544.
- Broverman, Inge, et al.
1972 "Sex-role stereotypes: a current appraisal." *The Journal of Social Issues* 28(2):68.
- Coser, Rose Laub
1966 "Role distance, sociological ambivalence and transitional status systems." *American Journal of Sociology* 72(September): 173-87.
- Coulson, Margaret A.
1972 "Role: A redundant concept in sociology?" P. 119 in J. A. Jackson (ed.), *Role*. Cambridge University Press.
- Dewey, Richard
1969 "The theatrical analogy reconsidered." *The American Sociologist* 4 (November): 309.
- Deutscher, Irwin
1970 "Words and deeds, social science and social policy." Pp. 28-31 in William J. Filstead (ed.), *Qualitative Methodology*. Chicago: Markham Publishing Co.
- Ellis, Desmond P.
1971 "The Hobbesian problem of order: a critical appraisal of the normative solution." *American Sociological Review* 36(August):697, 692-703.
- Erikson, Erik
1965 "Comment." Pp. 232-45 in J. Mattfield and C. Van Aken (eds.), *Women in the Scientific Profession*. Cambridge: MIT Press.
- Gerth, Desmond P. and C. Wright Mills
1958 *From Max Weber*. New York: Galaxy Book.
- Goffman, Erving
1959 *The Presentation of Self in Everyday Life*. Doubleday Anchor.
1961 *Encounters*. Indianapolis: Bobbs-Merrill Co.
- Goode, William J.
1960 "A theory of role strain." *American Sociological Review* 25(August):246-58.
1972 Presidential address: "The place of force in human society." *American Sociological Review* 37 (October):507-19.
- Gross, Neal, Ward S. Mason and Alexander W. McEachern
1958 *Explorations in Role Analysis: Studies of the School Superintendency Role*. New York: John Wiley and Sons, Inc.
- Homans, George C.
1961 *Social Behavior, Its Elementary Forms*. New York: Harcourt, Brace and World.
1964 "Bringing men back in." *American Sociological Review* 29(December):817.
1970 "The relevance of psychology to the explanation of social phenomena." Pp. 313-29 in R. Borger and F. Cioffi (eds.), *Explanation in the Behavioural Sciences*, Cambridge University Press.
- Jackson, John A. (ed.)
1972 *Role*. Cambridge University Press.
- Kaatz, Gilbert R. and Keith E. Davis
1970 "The dynamics of sexual behavior of college students." *Journal of Marriage and the Family* 32(August):390-9.
- Komarovsky, Mirra
1946 "Cultural contradictions and sex roles." *American Journal of Sociology* 52 (November):182-9.
1967 *Blue-Collar Marriage*. New York: Vintage Books.
- LaPiere, Richard
1934 "Attitudes vs. actions." *Social Forces* 13(March):230-7.
- Linton, Ralph
1936 *The Study of Man*. New York: Appleton-Century.
1946 *The Cultural Background of Personality*. London: Routledge and Kegan Paul.
- Merton, Robert K.
1940 "Fact and factitiousness in ethnic opinionnaires." *American Sociological Review* 5(February):13-28.
1957 "The role-set: problems in sociological theory." *British Journal of Sociology* 8(June):133ff.
- Merton, Robert K. and Elinor Barber
1963 "Sociological ambivalence." Pp. 91-120 in E. A. Tiryakian (ed.), *Sociological*

- Theory, Values and Sociocultural Change. New York: The Free Press of Glencoe.
- McKee, John P. and Alex C. Sherriffs
1959 "Men's and women's beliefs, ideals, and self-concepts." *American Journal of Sociology* 64(4):356-63.
- Mills, C. Wright
1940 "Methodological consequences of the sociology of knowledge." *American Journal of Sociology* 46(November):316-30.
- Naegele, K. I.
1966 Cited on p. 167 in Jack J. Preiss and Howard J. Ehrlich, *An Examination of Role Theory: The Case of the State Police*. Lincoln: University of Nebraska Press.
- Newcomb, Theodore M.
1954 "Sociology and psychology," Pp. 234-5 in John Gillin (ed.), *For a Science of Social Man*. New York: The Macmillan Co.
1966 Cited p. VI in Bruce J. Biddle and Edwin Thomas (eds.), *Role Theory*. New York: John Wiley and Sons, Inc.
- Parsons, Talcott
1937 *The Structure of Social Action*. McGraw-Hill.
1951 *The Social System*. Glencoe, Illinois: The Free Press.
- Parsons, Talcott and E. A. Shils, (eds.).
1951 *Towards a General Theory of Action*. Cambridge: Harvard University Press.
- Popitz, Heinrich
1972 "The concept of social role as an element in sociological theory." P. 11-39 in J. A. Jackson (ed.), *Role*. Cambridge University Press.
- Portes, Alezander
1971 "On the interpretation of class consciousness." *American Journal of Sociology* 77(September): see bibliography, 228ff.
- Preiss, Jack J. and Howard J. Ehrlich
1966 *An Examination of Role Theory: The Case of the State Police*. Lincoln: University of Nebraska Press.
- Princeton Alumni Weekly
1971 February 23:7
- Rose, Arnold M. (ed.)
1962 *Human Behavior and Social Processes: An Interactionist Approach*.
- Smelser, Neil J.
1968 "Sociological theory and psychological theory." Paper delivered at the annual meetings of the American Sociological Association, Boston, Massachusetts.
- Smelser, William T.
1961 "Dominance as a factor in achievement and perception in cooperative problem solving interactions." *Journal of Abnormal and Social Psychology* 62:535-41.
- Stebbins, Robert A.
1967 "A note on the concept of role distance." *American Journal of Sociology* 73(September):247-50.
- Treiman, Donald J.
1966 "Status discrepancy and prejudice." *The American Journal of Sociology* LXXI (May):651-64. See also included bibliography.
- Turner, Ralph
1968 "Role." *International Encyclopedia of the Social Sciences* 13:556.
- Wallin, Paul
1950 "Cultural contradictions and sex roles: a repeat study." *American Sociological Review* 15(April):288-93.
- Webster, Jr., Murray
1973 "Psychological reductionism, methodological individualism, and large scale problems." *American Sociological Review* 38(April):258-73.
- Wrong, Dennis
1961 "The oversocialized conception of man in modern sociology." *American Sociological Review* 26(April):188-92.

THE POLITICS OF BIRTH PRACTICES: A STRATEGIC ANALYSIS*

by

Karen E. Paige

University of California, Davis

and

Jeffery M. Paige

University of California, Berkeley

American Sociological Review 1973, Vol. 38 (December):663-76

A theory is proposed to account for cross-cultural variation in the customary birth practices of women and men. We suggest that the restriction of women during childbirth and the husband's ritual involvement in birth are both strategies for asserting or defending paternity rights. When paternity rights are established by agreements based on property transfers and enforced by organized kin groups, women will be restricted to insure that nothing upsets the agreements. When such agreements cannot be made and enforced, paternity claims will be asserted by the husband's ritual involvement in the birth. These hypotheses were tested in a sample of 114 societies based on Murdock and White's Standard Cross-Cultural Sample. The data confirm the hypotheses for the birth practices of both sexes. We suggest that birth practices represent a special case of bargaining mechanisms in societies without centralized authority.

INTRODUCTION

The customary practices and beliefs associated with childbearing have been the subject of much detailed ethnographic description and speculative analysis (cf. Sumner, 1934; Crawley, 1902; Frazier, 1922; Webster, 1942; Ford, 1964). Description and theory have both focused on two widespread birth customs—the restriction and segregation of women during pregnancy and childbirth and the observance of couvade by men during their wife's post-delivery confinement. Some customary restrictions on women such as sexual abstinence for specified periods during

the pregnancy or the avoidance or prohibition of particular foods or articles of clothing are almost universally observed. However, some societies restrict a woman's normal social contacts by instituting special rules of avoidance, particularly the avoidance of men, ostensibly to protect the society against the contamination and uncleanness associated with feminine reproductive activities. In some cases women are strictly segregated during the birth process and may be confined in special huts, restricted to their own residence, or removed to another community. The most widely discussed birth custom involving men is the couvade which usually includes customary dietary and occupational observances and post-partum seclusion of the husband. In some societies the husband observes all the restrictions observed by his pregnant wife; and when she goes into delivery, he may seclude himself in his residence and fast for a week or more. While in some cases the restrictions for the husband are even more extensive than the

*This study was supported by funds granted to Karen Paige by the Dept. of Psychology, University of California, Davis. We are particularly indebted to Linda Fuller, who supervised data collection, and to the coders—Kathy Barchak, Christi Bengard, Hart Guenther, and Setha Low. We also wish to thank the Survey Research Center, University of California, Berkeley, for providing services and facilities.

wife's, in most societies he performs such minimal ceremonial duties as cutting the umbilical cord after delivery, preparing a birth feast for relatives, performing sacrifices, or helping his wife with her daily chores. While these minor practices are generally not classified as *couvade*, they are similar in form if not in intensity.

Birth practices have been examined from several theoretical perspectives including sociological functionalism (Norbeck, 1961; Van Gennep, 1961), psychoanalytic personality theory (Bettelheim, 1954; Deveraux, 1950), and the social psychological theory of ritual (Radcliffe-Brown, 1952a). While these theories have been the focus of much cross-cultural study and theoretical debate (Homans, 1941; Ayres, 1954; Burton and Whiting, 1961; Young, 1963), relatively less attention has been paid to the earliest theory of birth practices first proposed by Tylor in 1889. Tylor claimed that *couvade* was most often practiced in societies in which paternity rights were customarily held by the mother's kinsmen rather than by her husband and concluded that the *couvade* represented a pre-legal method by which husbands attempted to establish paternity rights over their biological offspring. Tylor's theory has been handicapped by his inadequate sample of societies and by its association with discredited 19th century theories of the evolution of descent systems. Malinowski (1931), however, suggested a similar explanation of *couvade* in a discussion of social paternity. He argued that paternity rights must be socially legitimated and that performance of rituals during the birth process served that purpose. "The function of *couvade*," he argued, "is the establishment of social paternity by the symbolic assimilation of the father to the mother...and is an integral part of the institution of the family" (Malinowski, 1931:631). Neither of these theories is described in sufficient detail to permit prediction of cross-cultural variation in birth practices; and neither, of course, is concerned with the birth practices of women. Nevertheless the idea of social paternity is central to understanding the determinants of birth practices. This paper extends the argument of Tylor and Malinowski to account for both cross-cultural variation in birth practices and for the practices of women as well as men.

THEORY

Were social paternity an automatic consequence of biological parenthood, there would be no need to engage in birth practices to assert claims to offspring. But as Malinowski (1927) demonstrated, paternity rights are established through social consensus or contractual agreement and not through the biological process of conception, pregnancy, and childbirth. The birth of a child not only affirms biological parenthood but also allows an opportunity for interested parties to lay claim to the child's allegiance. These claimants are not limited to the biological parents. In fact the individual with the greatest biological involvement in the birth—the mother—is rarely given authority over her own children. Jural rights to control a child's political allegiance, economic activities, property, or inheritance usually rest with males, though not necessarily with the child's biological father. These rights define the role of sociological father, or *pater*, who is customarily distinguished from the biological father or *genitor*. It is not uncommon for the two roles to be played by different individuals. Paternity rights may be claimed by an individual or group of individuals who provided property used as brideprice regardless of their gender or kinship relation to the *genitor*. The role of sociological father may be played by the maternal brother, a lineage head, ceremonial kinsmen, state agencies, women, or even decedents. In most societies, of course, the biological father has jural as well as personal authority over his offspring. Even in this case, however, paternity rights are established by the marriage contract; and the competing claims of the wife's kinsmen may be relinquished only in exchange for property or personal service on the part of the husband.

Since paternity rights depend on social consensus or contractual agreements, individuals or groups can attempt to influence the consensus or renegotiate the agreement to their advantage. Paternity rights are subject to dispute in all societies, but the problem is acute in simpler societies where kinship is the major determinant of social position, economic resources, and political power. The greater the importance of such ties in determining social structure, the greater the significance of paternity rights in controlling the political and

economic activities of others. Gaining paternity rights means gaining a contributor to the communal economy, a new supporter of a political faction, and an additional ally in a feud. With such important issues at stake, it is not surprising that the birth of a child is the focus of paternity conflict in many societies.

While legitimate rights to offspring may have been theoretically established at marriage, the birth of a child gives them practical significance. It requires that nominal rights be recognized, that ambiguities in customary rights be clarified and that conflicting claims be resolved. Childbirth represents an opportunity for all potential claimants to reopen negotiations, accuse other claimants of bad faith, to demand compensation for real or imagined malfeasance. No matter who is recognized as the sociological father or how his rights have been established, claims can always be challenged and rights ignored.

In societies in which the wife's kinsmen hold jural authority over her children, the husband may try to use his personal authority to claim his children's allegiance for his own kin group. In societies where the husband's and wife's kinsmen share jural authority or where authority is shared by members of a corporate lineage, there may be continual competition for children's primary allegiance. Even in societies where the wife's kinsmen relinquish paternity rights at marriage, they may try to reclaim these rights after a child is born.

Competition over paternity is most apparent in divorce proceedings when the husband may have to relinquish his paternity rights or choose between yielding parental authority or forfeiting wealth or property transferred to the wife's family at marriage. In some cases, the husband may retain authority over male children but yield it over the female children. Whatever the social arrangements concerning paternity, a claimant can always find some grounds for questioning the rights of the sociological father.

In complex societies paternity disputes are the subject of litigation, and a formal judicial apparatus is available to settle them and enforce claims. But those societies in which kinship is most important in determining social structure are the least likely to have such formal mechanisms. This does not mean, of course, that agreements cannot be reached

or claims successfully defended; but it does suggest that the tactics of paternity disputes should take a different form in simple societies. Paternity rights are often the subject of bargains negotiated between groups of kinsmen. These bargains may involve substantial amounts of property and can be enforced by the organized military power of the respective kin groups. While such agreements are not legally enforceable, their terms cannot be violated without serious financial or political consequences. In such a situation the main interest of both parties is to insure that nothing upsets their agreement. Ceremonial attempts by males of either faction to assert additional paternity rights would only threaten the agreement. Where explicit, enforceable bargains exist, male birth rituals are neither necessary nor desirable. Both parties to the bargain have an interest in making sure that the contract is fulfilled by the birth of a healthy offspring with unambiguous paternity. Their main focus during pregnancy and birth should be to insure that nothing happens to the biological mother that could threaten either the health of the child or their claims to its allegiance. Given the biological uncertainties of childbearing and the benefits of claiming paternity rights, this may be a formidable task. The restriction or segregation of women during pregnancy and birth can be interpreted as a method of protecting the established rights of claimants against these dangers.

This interpretation suggests that maternal restrictions during pregnancy and birth and the couvade are alternate strategies to establish or defend paternity rights. The birth practices of both men and women depend on the nature of the bargains over paternity rights in a given society. Since the nature of the bargains which lead to restrictions on women should differ from those which lead to ritual involvement by men, each will be considered separately.

Maternal Restrictions

If the restriction of women during the birth process represents an attempt to insure the fulfillment of a previously negotiated contract, then a potential claimants' interest in the proceedings should vary with the importance of the bargain. When breach of contract leads to great financial loss or to the threat of violent retaliation from other inter-

ested factions, concern with a successful birth and efforts to monitor the birth process should increase. The importance of the bargain and the subsequent interest in the birth process should depend on two major factors: 1) the amount of property involved in the negotiations and 2) the claimants' power to enforce the agreement. These considerations, of course, are not limited to disputes over paternity and stated more generally simply indicate that the significance of a bargain depends on the size of the payoff and the finality of the terms.

Payoffs.—The payoffs in a paternity bargain always involve the relative wealth and power of individuals and kin groups, but the payoffs may be increased by direct property exchanges either at marriage or after a successful birth. While paternity rights do not always depend on the exchange of wealth and may be established by the marriage itself, substantial payments usually confer rights to the woman's offspring. The institution of brideprice in particular is usually interpreted as a direct payment for the reproductive capacity of the wife (cf. Mair, 1971; Fortes, 1962; Radcliffe-Brown, 1952b, 1950). The close association between brideprice and paternity rights is indicated by the fact that payments may be made only after the birth of an offspring or may be made in installments contingent on the wife's continued fertility. The brideprice represents compensation to the wife's kinsmen for the loss of potential offspring to their lineage. As Mair has observed the sum paid in brideprice may be equivalent to the sum paid in compensation for homicide, an indication of the close association between the loss of living and potential kinsmen.

The payment of brideprice or any other exchange of wealth does not assure that paternity rights will be realized. Accidents of reproduction may make the contract meaningless. Given the high rate of fetal and infant mortality in most pre-industrial societies, the possibility is strong that the terms of the paternity agreement will not be fulfilled. Failure to produce an offspring may result from such purely medical problems as barrenness, fetal death or infant disease; but it may also result from infanticide or abortion. Whatever the cause, the potential claimant may demand compensation from the wife's kinsmen. If the wife is barren, her husband may demand that her father give him an additional

wife, a child of his own, or some of his property or animals. If a wife is guilty of abortion, her kinsmen may be compelled to pay for the fetus. Any irregularity in the birth process may also occasion additional demands for compensation. The potential claimant can use a breach birth, a birth mark, or even suspected witchcraft to reopen negotiations. However the wife's kinsmen may be in a poor position to meet demands for compensation. Property received for paternity rights is not saved but dispersed among kinsmen, used to buy wives, or pay blood debts and other outstanding obligations. Failure to produce offspring may therefore disrupt an elaborate pattern of financial and kinship obligations. It will certainly lead to renewed haggling and mutual recrimination and may prompt sorcery or outright violence. Clearly, both sets of kinsmen have a vested interest in fulfilling the original contract. The husband's kinsmen must assure that offspring are born in return for wealth already expended and the wife's kinsmen are interested in avoiding the return of wealth they have already spent. The main interest of both parties during pregnancy and birth should be in insuring that nothing happens to upset the contract.

There are a number of ways maternal birth practices might function to protect a bargain. Rules requiring social isolation, avoidance of men, confinement in birth huts or removal to another community all facilitate the surveillance and control of both the mother and potential claimants. Restricting and confining the mother limits the possibility of disrupting the birth through abortion, infanticide, or kidnapping. Segregation from men limits the possibility that other male claimants, such as an unsuccessful suitor or an adulterer, can present their claims. Confinement and isolation also mean that contact with the mother can be limited to the agents of the kin groups with a legitimate interest in the birth. Often, agents of both interested parties will be present at delivery which enables them not only to monitor the mother but also one another. Often it is only after some indication that delivery is successful and the infant's sex determined that additional negotiations and transfer of brideprice occur. If some biological accident disrupts the birth, the limited contacts of the mother and the presence of kin group agents insure that conflict over compensation can be limited and that blame will not

be unjustly assigned. In a society with no recourse to police or judiciary, kin groups must monitor other claimants and their own members to insure that contracts will be respected.

Enforcement Power. — The importance of a paternity bargain depends not only on the amount of property exchanged but also on the enforcement power of the interested parties. There would be little point in carefully monitoring the birth process were there no way to apply sanctions to those who disrupted it. Similarly there would be little point in investing substantial amounts of property in rights to future offspring were there no way to defend these rights or demand compensation for default. In simple societies, enforcement power depends on the organized military and political power of groups of kinsmen. A man without kin backing has only his own strength and personality to defend his rights. Enforcing paternity bargains requires that the males of a kin group act together and use force if necessary to defend their interests. Such groups have been termed *fraternal interest groups* by Van Velzen and Van Wetering (1960). They demonstrate that fraternal interest groups are indeed associated with the use of force to defend kin group interests. Societies with such groups were significantly more likely to engage in violent retaliation over adultery, personal injury, and murder. Otterbein (1968), Otterbein and Otterbein (1965), and Ember and Ember (1971) have similarly demonstrated that internal warfare and blood feuds are more likely in societies containing fraternal interest groups. Paige (in press) has demonstrated that factional conflicts between fraternal interest groups are reflected in the form of the sovereign decision making structure in the society. The political power of a fraternal interest group gives kinsmen the ability to enforce the restrictions on maternal behavior and maintain the mother's isolation from other claimants.

The existence of fraternal interest groups depends on a society's dominant residence pattern. Their formation is facilitated by patrilocal and avunculocal residence and inhibited by matrilocal, bilocal and neolocal residence. In both patrilocal and avunculocal residence consanguinally related males live together in the same community. In patrilocal residence sons live with their fathers after marriage, and in avunculocal residence neph-

ews live with their maternal uncles. In matrilocal, bilocal and neolocal residence kin are dispersed rather than localized. In matrilocal residence sons live with their wives' families, in neolocal residence sons live separately, and when residence is bilocal sons may or may not live with their father. Patrilocal or avunculocal residence has therefore been generally used to indicate the presence of fraternal interest groups. While patrilocal or avunculocal residence produces communities in which residence and kinship reinforce one another, other forms of residence produce conflicting patterns of allegiance. An individual owes allegiance both to his own kinsmen, many of whom live elsewhere, and to the members of his local community. In patrilocal and avunculocal societies, groups of related males with similar interest in a paternity dispute will be able to act as a unit. In societies with other residence patterns, communities will likely contain individuals whose loyalties lie with competing claimants, and concerted action will be difficult.

Residence patterns which facilitate the formation of fraternal interest groups should make both contractual agreements over paternity and restrictions on women more likely. Clearly, however, the effects of fraternal interest groups on maternal restrictions are not independent of the effects of wealth exchange and demands for compensation. Compensation cannot be successfully demanded if sanctions are not available to enforce the demand, and wealth will not be risked when no agreement can be enforced. Wealth exchange and demands for compensation should, therefore, both be associated with the presence of fraternal interest groups. Since wealth exchange, demands for compensation, and the presence of fraternal interest groups can each be considered a measure of the presence of explicit paternity bargains, each should be positively associated with the presence of maternal restrictions. While these zero-order relationships are clearly implied by the argument associating maternal restrictions with paternity bargains, the effect of each variable controlling for the others is less clear. Specifying the complete causal model associating the characteristics of paternity bargains with maternal restrictions requires empirically investigating the indicators' interrelationships. If our theory is incorrect, however, even the zero-order relationships will not hold, so

that it seems advisable to examine these predictions empirically before examining the entire causal model in more detail.

Husband Ritual Involvement

The ritual involvement of husbands in the birth process through dietary observances or post-delivery seclusion can be viewed as an attempt to assert paternity rights in the absence of more potent sources of influence. While such observances can be used to influence public opinion in any society they are most effective in societies where enforceable paternity agreements do not exist. No amount of ritual activity is likely to alter paternity agreements based on the expenditure of several years earnings or the power of an important lineage. In fact when paternity agreements are carefully monitored unwarranted ritual claims could be viewed as a threat to the agreement and lead to attempts at retaliation. When the backing of courts or organized kinsmen is available, ritual is a poor substitute for legal or political action. A potential claimant would be ill advised to spend two weeks in a hammock or avoid turtle meat if he could claim his child by hiring a lawyer or organizing a war party. If such enforcement power is unavailable, however, a husband loses little by making ritual claims since none of the other claimants are in any position to stop him except by engaging in ritual themselves. In particular, the husband's ritual involvement helps offset the implicit claims of his wife's kinsmen dramatized by the wife's undeniable role in the birth. The husband's involvement in the birth can then be viewed as a form of psychological warfare useful when opportunities for more direct forms of conflict are restricted.

Ritual conflict is not limited to paternity disputes and, as Service (1966) and Gluckman (1965) have pointed out, is a common method of adjudicating quarrels in societies which lack organized enforcement power. Such ritual conflicts as the Eskimo song duel or the Tiwi spear throwing contest provide alternatives to litigation or private vengeance. As Service suggests, these contests allow each party to state his grievances publicly and attempt to influence community opinion in his favor. They also allow other community members to decide which side they favor. Eventually the contests make clear where the

majority opinion lies, and the losing party will usually not press his claims further. Ritual conflict is particularly likely when disputes involve kinsmen or members of the same community or when societies lack the organization necessary for military activity. Gluckman suggests that ritual may also be used if individual and group interests are in conflict or if cross-cutting allegiance patterns inhibit more direct forms of conflict.

If the husband's ritual involvement in the birth occurs in the absence of enforcement power, then husband involvement and maternal restriction represent alternate strategies in paternity disputes. Maternal restrictions depend on the presence of enforceable bargains, while husband involvement depends on their absence. This inverse relationship between husband involvement and maternal restrictions depends fundamentally on the unequal role of men and women in the birth process. While women bear children, paternity rights are almost invariably controlled by men. Thus women are objects not actors in most paternity negotiations. Both the ritual involvement of husbands in the birth and the restriction of women are primarily tactics used by male claimants to protect rights over offspring being produced by women. When males are effectively organized they can use this power to control the birth process to their own advantage. When they lack such power, they must find alternative strategies to assert their claims.

The interrelationships between fraternal interest groups, wealth exchange, compensation, and birth practices suggested by the above analysis can be summarized in the following set of hypotheses:

1. The presence of fraternal interest groups should be positively associated with the presence of maternal restrictions and negatively associated with the presence of husband involvement.
2. The presence of wealth exchange should be positively associated with the presence of maternal restrictions and negatively associated with the presence of husband ritual involvement.
3. The presence of demands for compensation should be positively associated with the presence of maternal restrictions and negatively associated with the presence of husband involvement.

4. Fraternal interest groups, wealth exchange, and demands for compensation should all be positively associated with one another.
5. Maternal restrictions should be negatively associated with husband ritual involvement during the birth process.

METHOD

The Sample

The hypotheses stated above were tested in a sub-sample of 114 societies selected from the Standard Cross-Cultural Sample (SCCS) recently developed by Murdock and White (1969). The SCCS is a stratified sample of world societies designed to minimize the effects of historical diffusion by including only one society from each of 186 distinct sampling provinces (Murdock, 1968). Each province consists of a cluster of societies with similar culture, language, and location. Sub-samples from the SCCS are not limited to 186 societies listed by Murdock and White, and other societies from the same sampling province may be substituted for those on the original list. The present sub-sample was selected by first taking every second society on the SCCS list. In cases where ethnographic materials were more readily available on an adjacent society than on the society selected by this rule, the better described society was substituted. Societies with matrilineal descent were over-sampled by adding any matrilineal society which was adjacent to a non-matrilineal society selected by the every other case rule. Thirteen societies on which data had already been collected for a pilot study were added to the initial sample. In no case, however, was more than one society included from the same sampling province. The complete sub-sample of societies and the sampling provinces they represent are listed in Appendix 1.

Table 1 compares the distribution of societies by world region for the SCCS and the current sub-sample. The sub-sample is reasonably representative of the SCCS except for the underrepresentation of societies in the Circum-Mediterranean. This is a result of oversampling matrilineal societies which are rare in this region. Matrilineal societies make up 21 percent of the sub-sample but only 14 per cent of the SCCS. Of the twenty-four

matrilineal societies appearing on the SCCS list, twenty-three were included in the present sub-sample. matrilineal descent was over-sampled because of its association with avunculocal and matrilineal residence and the significance of these residence patterns in the argument concerning fraternal interest groups. A single random sample would contain too few of these residence patterns to examine the effects of fraternal interest groups in different residence configurations.

Measures of Birth Practices

The customary birth practices of both men and women were coded from ethnographic sources on each society. All sources were coded independently by three graduate students each of whom had training in coding ethnographic materials and was fluent in at least one foreign language. All sources cited by Murdock and White in the SCCS were coded in the original ethnographic language when that language was English, French, Spanish, or German. Supplementary sources in Russian, Danish, and Arabic were not consulted. All additional ethnographic materials available as of 1972 in the University of California library and the Human Relations Area File microfilm collection were also consulted. Thus the coded data should be more representative of the true distribution of world cultures than is the usual cross-cultural analysis based on HRAF or English language sources only.

The presence or absence of each custom was determined by using the score obtained by two out of three coders when the coding

Table 1. Regional Distribution of Societies

World Region	Subsample (N=114)	SCCS (N=186)
Africa	16%	15%
Circum-Mediterranean	11	15
East Eurasia	17	18
Insular Pacific	19	17
North America	19	18
South America	18	17

decision was not unanimous. Customs were coded for the time period and locale specified in Murdock's Ethnographic Atlas (1967) and Murdock and White's SCCS (1969). The original codes for birth practices in this study therefore refer to the same period and place as the codes in Murdock's Atlas. This makes it possible to include Murdock's codes for wealth exchange and residence in the analysis

of birth practices. Only observable changes in behavior during the birth process were coded. Beliefs and myths about childbearing were not used as indicators of actual practice. The customary behavior of both men and women could be ordered into Guttman-type scales. The ordering of each set of customs and their cumulative frequencies are presented in Table 2.

Table 2. Measures of Birth Practices

Custom Category	Item	Cumulative Frequency	Custom Category Description
Maternal Restrictions Scale^a			
Social (High):			
Structural Seclusion	5	24.3%	Confined to dwelling during pregnancy at least 2 weeks prior to delivery; secluded in special hut; moved to other community during birth process.
Social Avoidance	4	48.6	Contact with people, especially men, is restricted during pregnancy. Pregnant women avoided and believed to be unclean and dangerous, evil.
Personal (Low):			
Sex Taboo	3	63.1	Sexual relations with husband restricted for at least two months before delivery.
Food Taboo	2	82.9	Eating certain foods during either pregnancy or post-partum is restricted.
Minor	1	100.0	Restrictions on looking at ugly objects, wearing certain clothing, working too hard, etc.
Husband Involvement Scale^b			
Couvade (High):			
Seclusion	5	16.2%	Secluded in dwelling during pregnancy or post-partum with or without mother and child. May also be considered unclean. Avoids others.
Post-partum Work Taboo	4	29.7	Refrains from performing normal tasks during post-partum period. Must remain close to home; contact with others minimized.
Food Taboo	3	44.1	Refrains from eating certain foods during pregnancy or post-partum.
Minor (Low):			
Minor Observances	2	64.0	Minor ritual observances, such as seeking a vision, performs birth-related sacrifices. May help wife with daily chores.
Informal	1	100.0	Residual category: no changes in normal behavior. No ritual observances.

a Coef. of reproducibility = .93. (Missing cases: Basque, Rhade, Amahuaca)

b Coef. of reproducibility = .96. (Missing cases: Suku, Rhade, Awaikoma)

The Maternal Restrictions Scale measures the degree of constraint on women's behavior during the birth process. The practices making up this scale range from minor restrictions on work or apparel to confinement in specially built dwellings or removal from the community. The five-item scale in Table 2 has a coefficient of reproducibility of .93 (see White and Saltz, 1967, for computation procedures). While all the items in the scale represent some restriction of women, not all are necessary to monitor the birth and protect previously negotiated contracts. Only the two highest categories, social avoidance and structural seclusion, permit close monitoring and control of the birth process. For purposes of analysis, therefore, these two categories were collapsed into a single category indicating the presence of significant maternal restrictions. The remaining three categories were correspondingly collapsed into a low restriction category.

The Husband Involvement Scale measures the degree to which husbands change their behavior in ways which indicate an active ritual involvement in the birth process. The lowest scale category indicates the absence of any changes in husband behavior and the highest indicates confinement and seclusion. The coefficient of reproducibility for the five-item scale is .96. Only the three highest categories—seclusion, post-partum work and food taboos—include behaviors which could be called *couvade*, and for purposes of analysis these three categories were collapsed into one. Minor behavioral changes such as helping wives with chores were grouped with the absence of any behavioral change in a low involvement category. The scale as it appears in the analysis, therefore, indicates simply the presence or absence of *couvade* in a particular society.

Measures of Bargaining

The index of the presence or absence of fraternal interest groups was simply the dominant residence pattern as coded by Murdock and Wilson (1972:261,Col.9) and Murdock (1967:156,Col.16). Fraternal interest groups were considered present if residence was patrilocal or avunculocal and absent if residence was matriloc, bilocal, or neolocal.

The index of wealth exchange was based on Murdock's (1967:155,Col.12) code for

mode of marriage which included various types of marital transactions. Wealth exchange was considered present if property was transferred either in the form of brideprice, substantial gifts, or sister exchange as part of the marital bargain. Wealth exchange was considered absent when no substantial amount of property was exchanged. Bride service, therefore, was combined with the code indicating the absence of any substantial consideration at marriage. Dowries were also excluded from the wealth exchange category since property five-item scale is .96. Only the three highest categories—seclusion, post-partum work and food taboos—include behaviors which could be called *couvade*, and for purposes of analysis these three categories were collapsed into one. is not exchanged but settled on a daughter and remains her property regardless of the outcome of the marriage.

The index of compensation demands was developed from the original ethnographic sources. Compensation demands were coded present when the husband or his kinsmen demanded payment of gifts or return of some or all of the bridewealth in cases of infanticide, abortion, or barrenness. They were also coded present when barrenness, infanticide, or abortion were grounds for divorce and divorce led to compensation demands.

RESULTS

The zero-order relationships between each of the three measures of paternity bargains and each of the two measures of birth practices are presented in percentage form in Tables 3, 4, and 5. The same set of relationships also appears in the correlation matrix of Table 6. Tables 3, 4, and 5 present the data necessary to test Hypotheses 1, 2, and 3 respectively. The data in Table 3 present the associations between fraternal interest groups and birth practices specified by Hypothesis 1. The data support the predictions for both maternal restrictions and husband ritual involvement in the birth process. The presence of fraternal interest groups is positively associated with the presence of maternal restrictions ($r=.31$) and negatively associated with husband involvement ($r = -.36$). The distribution of birth practices by residence type in Table 3 indicated that the effect of fraternal interest groups is not a consequence of a particular form of residence.

Table 3. Birth Practices and Fraternal Interest Groups

Extensiveness of Birth Customs	Dominant Residence Pattern			
	Fraternal Interest Groups Present		Fraternal Interest Groups Absent	
	Patrilocal	Avunculocal	Neolocal & Bilocal	Matrilocal
Maternal Restrictions ^a				
High (Social)	60.0 (39)	71.4 (5)	35.7 (5)	24.0 (6)
Low (Personal)	40.0 (26)	28.6 (2)	64.3 (9)	76.0 (19)
Total	100.0 (65)	100.0 (7)	100.0 (14)	100.0 (25)
Husband Involvement ^b				
High (Couvade)	33.3 (22)	14.3 (1)	76.9 (10)	64.0 (16)
Low (Minor)	66.7 (44)	85.7 (6)	23.1 (3)	36.0 (9)
Total	100.0 (66)	100.0 (7)	100.0 (13)	100.0 (25)

a χ^2 (FIG present vs. absent) = 10.958, $p < .001$, $\phi^2 = .099$

b χ^2 (FIG present vs. absent) = 14.225, $p < .001$, $\phi^2 = .127$

Residence rules that localize groups of kinsmen facilitate maternal restrictions but inhibit husband involvement. Rules that disperse kinsmen have the opposite effect. Thus both avunculocal and patrilocal residence have the same effect on birth customs even though avunculocality concentrates maternal uncles and nephews and patrilocality concentrates fathers and sons. Similarly, the effects of matrilocal, bilocal, and neolocal residence are the opposite of avunculocal and patrilocal residence even though males are dispersed among their wives' kinsmen in one case and scattered throughout the society in the other two cases. Residence rules, therefore, exert their effect on birth practices by determining the ability of kinsmen to organize fraternal interest groups to defend paternity rights.

The effects of wealth exchange and demands for compensation are similar to those of fraternal interest groups. Hypotheses 2 and 3 suggested that both indicators should be positively associated with maternal restrictions and negatively associated with hus-

band involvement. The data in Table 4 confirm the predictions for the effect of wealth exchange on both sets of birth practices. The correlation between the presence of wealth exchange and the presence of maternal restrictions is .29, while the corresponding correlation for husband involvement is -.25. The predictions for the effects of demands for compensation were, however, supported only for the case of maternal restrictions (Table 5). Demands for compensation for barrenness, infanticide, or abortion are significantly associated with maternal restrictions ($r = .42$); but the relationship with husband involvement, while in the expected direction, is not statistically significant ($r = -.15$).

The remaining correlations in Table 6 present data necessary for testing Hypotheses 4 and 5. Hypothesis 4 suggested that since fraternal interest groups, wealth exchange, and demands for compensation all indicated the presence of binding agreements over paternity, all three indices should be correlated with one another. As might be expected the

Table 4. Birth Practices and Wealth Exchange

Extensiveness of Birth Customs	Wealth Exchange	
	Present	Absent
Maternal Restrictions ^a		
High (Social)	64.7 (33)	35.6 (21)
Low (Personal)	35.3 (18)	64.4 (38)
Total	100.0 (51)	100.0 (59)
Husband Involvement ^b		
High (Couvade)	31.4 (16)	55.9 (33)
Low (Minor)	68.6 (35)	44.1 (26)
Total	100.0 (51)	100.0 (59)

$$a \chi^2 = 9.277, p < .005, \phi^2 = .084$$

$$b \chi^2 = 6.679, p < .01, \phi^2 = .061$$

strongest relationship is between wealth exchange and demands for compensation ($r=.67$). Clearly, claimants are in a much better position to demand their money back if they have paid something in the first place. Wealth exchange is also positively associated with the presence of fraternal interest groups ($r=.38$) suggesting that explicit payments for paternity are more likely when contracts can be enforced by organized groups of kinsmen. Similarly demands for compensation are more likely in the presence of fraternal interest groups ($r=.37$) suggesting that fraternal interest groups may provide the power necessary to extract compensation. As the theoretical analysis implied, the various factors of a paternity bargain are interdependent.

Hypothesis 5 suggested that maternal restrictions and husband involvement should be alternate strategies for asserting paternity claims. This hypothesis is only weakly supported by the data ($r = -.24$). It is clear that a number of societies combine both practices and a number of societies lack both.

While the pattern of zero-order re-

Table 5. Birth Practices and Compensation Demands

Extensiveness of Birth Customs	Compensation Demands	
	Present	Absent
Maternal Restrictions ^a		
High (Social)	71.4 (30)	29.5 (13)
Low (Personal)	28.6 (12)	70.5 (31)
Total	100.0 (42)	100.0 (44)
Husband Involvement ^b		
High (Couvade)	38.1 (16)	53.5 (23)
Low (Minor)	61.9 (26)	46.5 (20)
Total	100.0 (42)	100.0 (43)

$$a \chi^2 = 15.078, p < .0005, \phi^2 = .175$$

$$b \chi^2 = 2.028, p = ns, \phi^2 = .024$$

lationships generally supports the theoretical analysis, the various effects are obviously not independent of one another. The path diagrams in Figure 1 represent the effects of each variable controlling for the others, and permit a more detailed examination of the causal sequence linking paternity bargains and birth practices. The notation in Figure 1 is based on Duncan's (1966) description of path analysis. Both models in Figure 1 assume that fraternal interest groups and wealth exchange are pre-determined variables and that compensation demands depend on both. Both models also assume that birth practices in turn depend on compensation demands, wealth exchange and fraternal interest groups. The model accounts for 47 per cent of the variance of compensation demands, but it is clear that most of this effect is contributed by the direct path from wealth exchange to compensation demands. In fact the direct path from fraternal interest groups to compensation demands is not significantly different from zero. The indirect effects of fraternal interest groups through its correlation with wealth exchange

Table 6. Zero-Order Correlations of Indicators of Bargaining and Birth Practices

	Wealth Exchange	Compensation Demands	Husband Involvement	Maternal Restrictions
Fraternal Interest Groups	.38***	.37***	-.36***	.31***
Wealth Exchange		.67***	-.25**	.29**
Compensation Demands			-.15	.42***
Husband Involvement				-.24*

*Indicates $p < .01$ **Indicates $p < .005$ ***Indicates $p < .001$

is greater than its direct effects. In this case the path diagrams simply emphasize the importance of wealth exchange as the principal determinant of compensation demands.

The complete causal model accounts for 20 per cent of the variance in maternal restrictions and 15 per cent of the variance in husband involvement. The relative contributions of the three bargaining variables, however, distinctly differ in the two cases. Only compensation demands have a statistically significant direct effect on maternal restrictions. In fact almost the entire correlation between wealth exchange and maternal restrictions is a result of the intervening effect of compensation demands. Wealth exchange alone has almost no direct effect on maternal restrictions. The effect of fraternal interest groups is slightly larger, but still insignificant. The path diagram suggests a revision of the original theoretical analysis. The strong effect of compensation demands and the relative absence of any direct effects of wealth exchange or fraternal interest groups indicates that attempts to monitor the birth will occur only when payoffs are directly contingent on a successful delivery. If payments for paternity rights are not refundable, there would be less point in monitoring the birth process. In such a situation the property is lost in any event and no amount of surveillance will protect the investment. Similarly, when no property is involved the threat of retaliation represented by a fraternal interest group is not sufficient to induce claimants to protect their agreement. This result could suggest that fraternal interest groups represent potential enforcement power. The decision to use the

organized power of kinsmen may rest on the importance of the paternity bargain, and only those bargains which involve refundable property may be important enough to lead to actual retaliation. If this were the case, fraternal interest groups would exert their effect only because of their association with demands for compensation. This in fact seems to be the conclusion suggested by the path diagram for maternal restrictions.

While demands for compensation are the only significant direct determinant of maternal restrictions, they have almost no direct effect on husband involvement. Only the absence of fraternal interest groups has any statistically significant direct effect on husband involvement. Neither the amount of property involved nor the possibility that it may be demanded back seem to inhibit ritual claims. Husbands who cannot count on the support of organized groups of kinsmen apparently assert ritual claims even if disruption of the birth can sometimes lead to compensation demands. Ritual conflict over paternity therefore seems to depend on residence rules which establish cross-cutting patterns of allegiance and inhibit direct conflict.

CONCLUSIONS

The pattern of results in the two path diagrams indicates that of the variables examined, compensation demands are the most important determinant of maternal restrictions, while fraternal interest groups are the most important determinant of husband involvement. Paternity agreements are monitored only when their disruption has immediate economic consequences. Paternity rights may be claimed through ritual in-

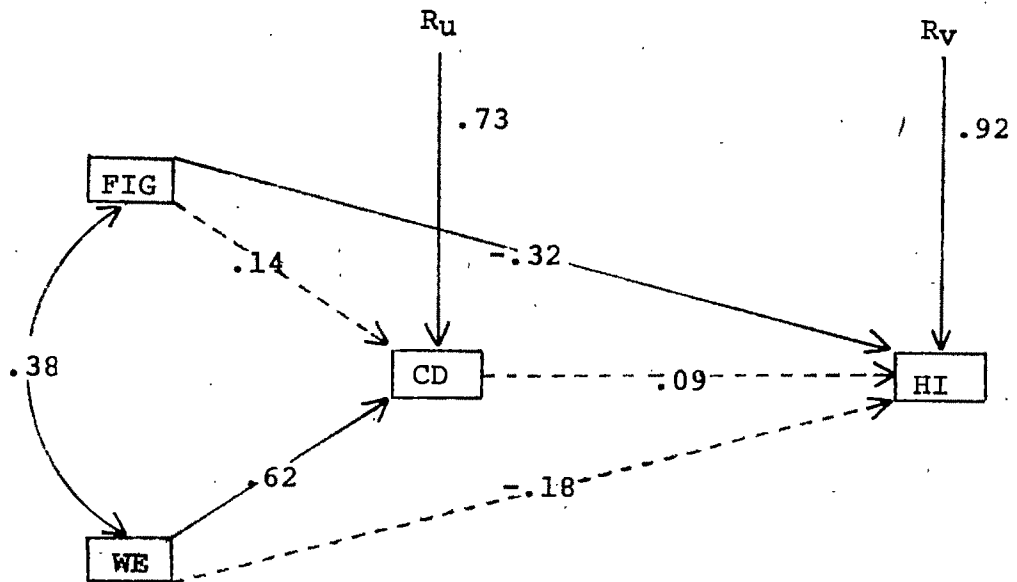
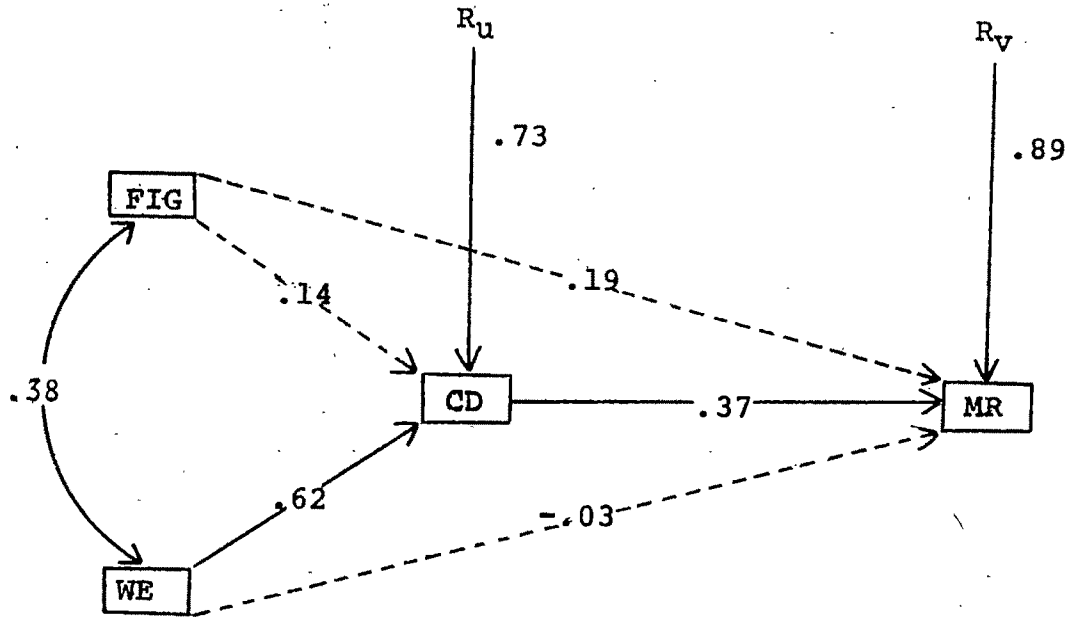


Figure 1. Path Diagrams for Maternal Restrictions (MR) and Husband Involvement (HI).

vovement even when they are established by explicit bargains, as long as direct conflict is inhibited by the absence of fraternal interest groups. Both these findings support the general hypothesis that birth practices represent tactics in negotiations over paternity.

While this hypothesis is concerned with customary behavior at birth, the theory itself is not limited to conflicts over paternity. Birth can be seen as one of a number of critical social transitions involving the transfer or exchange of important rights and privileges. The consequences of the size of payoffs and the effects of enforcement power in such situations should be similar whatever the issue. If ritual conflict over paternity is more likely because cross-cutting allegiance patterns inhibit direct conflict, then the absence of fraternal interest groups should lead to increased ritual conflict over other issues. Similarly male interests in restricting the behavior of mothers during pregnancy and childbirth is part of a larger pattern of male control over women's activities. The analysis of paternity bargains should provide concepts useful in understanding other conflicts over the control and subordination of women by men. The politics of birth practices therefore represent a special case of bargaining tactics in the absence of centralized authority.

REFERENCES

- Ayres, Barbara
1954 A Cross-Cultural Study of Factors Relating to Pregnancy Taboos. Unpublished Ph.D. Dissertation, Harvard University.
- Bettelheim, Bruno
1954 Symbolic Wounds. Glencoe, Ill: Free Press.
- Burton, R. and J. Whiting
1961 "The absent father and cross-sex identity." *Merrill-Palmer Quarterly* 7 (Winter): 85-95.
- Crawley, Ernest
1902 *The Mystic Rose*. London: Macmillan.
- Duncan, Otis
1966 "Path analysis: sociological examples." *American Journal of Sociology* 72 (July): 1-16.
- Deveraux, George
1950 "The psychology of feminine genital bleeding." *International Journal of Psychoanalysis* 31 (Winter): 237-57.
- Ember, Melvin and Carol Ember
1971 "The conditions favoring matrilineal versus patrilineal residence." *American Anthropologist* 73 (June): 571-94.
- Ford, Clellan
1964 A Comparative Study of Human Reproduction. Yale University Publication in Anthropology Number 32. New Haven: Human Relations Area File Press.
- Fortes, Meyer
1962 "Introduction." In Meyer Fortes (ed.), *Marriage in Tribal Societies*. Cambridge: Cambridge University Press.
- Frazier, J. G.
1922 *The Golden Bough*. London: Macmillan.
- Gluckman, Max
1965 *Politics, Law, and Ritual in Tribal Society*. Chicago: Aldine.
- Homans, George
1941 "Anxiety and ritual: the theories of Malinowski and Radcliffe-Brown." *American Anthropologist* 43 (April): 164-72.
- Mair, Lucy
1971 *Marriage*. London: Penguin.
- Mallinowski, Bronislaw
1927 *Sex and Regression in Savage Society*. New York: Harcourt, Brace.
- 1931 "Culture." In Seligman, E. (ed.) *Encyclopedia of the Social Sciences*. London: Macmillan.
- Murdock, George
1967 "Ethnographic atlas: a summary." *Ethnology* 6 (April): 109-236.
- 1968 "World sampling provinces." *Ethnology* 7 (July): 305-26.
- Murdock, George and Douglas White
1969 "Standard cross-cultural sample." *Ethnology* 8 (October): 329-69.
- Murdock, George and Suzanne Wilson
1972 "Settlement patterns and community organization: cross-cultural codes 3." *Ethnology* 11 (July): 254-95.
- Norbeck, Edward
1961 *Religion in Primitive Society*. New York: Harper Brothers.
- Otterbein, Keith
1968 "Internal war: a cross-cultural study." *American Anthropologist* 70 (April): 277-89.
- Paige, Jeffery
Forth- "Kinship and polity in stateless societies." coming *American Journal of Sociology*.
- Radcliffe-Brown, A.
1950 "Introduction." Pp. 1-85 in A. Radcliffe-Brown and D. Forde (eds.), *African Systems of Kinship and Marriage*. London: Oxford University Press.
- 1952a "Taboo." Pp. 133-52 in A. Radcliffe-Brown, *Structure and Function in Primitive Society*. London: Oxford University Press.
- 1952b "Patrilineal and matrilineal succession." Pp. 32-48 in A. Radcliffe-Brown, *Structure and Function in Primitive Society*. London: Oxford University Press.
- Service, Elman
1966 *The Hunters*. Englewood Cliffs, N.J.: Prentice-Hall.
- Sumner, William
1934 *Folkways*. Boston: Ginn.
- Tylor, Edward
1889 "On the method of investigating the development of institutions; applied to laws of marriage and descent." *Journal of the*

- Royal Anthropological Institute 18: Webster, H.
245-69. 1942 Taboo: A Sociological Study. Stanford:
Stanford University Press.
- Van Gennep, Arnold White, B. and E. Saltz
1961 The Rites of Passage. Chicago: University
of Chicago Press. 1967 "The measurement of reproducibility."
Pp. 241-57 in D. Jackson and S. Messick,
Problems in Human Assessment, New York:
McGraw Hill.
- Van Velzen, H. and W. Van Wetering
1960 "Residence, power groups and intra-
societal aggression...." International Ar-
chives of Ethnography 49: 169-200.
- Young, Frank
1965 Initiation Rites. New York: Bobbs-Merrill.

Appendix 1. Sample Societies and Sampling Provinces (SP), Husband Involvement Scores (H), and Maternal Restrictions Scores (M).

SP	Society	H	M	SP	Society	H	M	SP	Society	H	M
AFRICA				EAST EURASIA (cont'd)				NORTH AMERICA (cont'd)			
1	Nama	2	5	78	Ainu	5	3	140	Twana	4	4
2	Kung	1	4	80	Korea	1	5	141	Hupa	4	2
3	Thonga	3	4	81	Tungus	1	4	142	Klamath	2	5
6	Suku	1	1	85	Lepcha	4	2	144	Yokut	4	2
7	Lamba	1	1	87	Garó	4	1	146	Palute	3	2
9	Nyakyusa	3	4	88	Lakher	2	1	149	Sanpoil	3	5
11	Luguru	1	2	90	Lamet	4	2	150	Gros Ventre	2	4
13	Ganda	3	5	91	Thai	1	1	152	Micmac	2	4
14	Mongo	1	5	92	Andaman	3	2	153	Ojibwa	2	4
15	Mbuti	3	2	96	Rhade	.	.	154	Hidatsa	1	3
19	Dahomey	2	4	8	Tanala	1	3	157	Creek	4	3
20	Ashanti	2	5					159	Pawnee	4	1
24	Bambara	2	5	INSULAR PACIFIC				161	Apache	1	3
26	Tallensi	3	4	101	Iban	4	2	162	Navaho	3	2
29	Tiv	2	5	104	Bali	2	4	163	Papago	4	5
33	Azande	3	2	107	Alor	4	3	165	Tarasco	2	3
37	Shilluk	1	5	108	Murngin	2	3	166	Zapotec	1	1
38	Masai	2	5	109	Arunta	1	5				
CIRCUM MEDITERRANEAN				112	Kiwait	5	4	SOUTH AMERICA			
28	Hausa	1	5	113	Wogeo	5	5	168	Miskito	4	5
32	Fur	5	5	114	Kapauku	2	2	169	Bribri	3	4
41	Somali	1	4	115	Palauan	1	5	170	Cuna	5	2
42	Amhara	1	5	116	Yap	3	5	172	Goajiro	2	4
46	Tuareg	1	5	117	Ifaluk	3	3	173	Callinago	5	3
47	Rif	1	2	118	Marshall	2	4	175	Warrau	5	1
51	Hebrew	1	4	120	Manu	1	5	178	Carib	5	3
52	Rwala	1	4	121	Lesu	1	4	179	Saramacca	1	2
55	Gheg	1	2	122	Trobriand	2	5	180	Mundurucu	5	1
56	Basque	1	.	123	Kurtatchi	5	2	182	Witoto	5	3
58	Lapp	1	4	124	Ontong Java	1	1	183	Jivaro	5	3
62	Kurd	1	1	125	Tikopia	2	4	184	Amahuaca	1	.
EAST EURASIA				128	Fiji	3	5	186	Aymara	1	1
63	Basseri	1	1	129	Samoa	1	2	187	Siriono	5	2
66	Vedda	1	1	130	Maori	2	4	188	Nambicuara	5	2
67	Chenchu	2	1	131	Marquesa	5	5	190	Timbira	5	3
68	Baiga	1	3	NORTH AMERICA				191	Tupinamba	5	1
70	Burusho	2	5	132	Aleut	1	4	194	Aweikoma	.	1
72	Kazak	1	1	133	Copper Eskimo	4	2	196	Lengua	3	2
73	Yurak Samoyed	1	4	136	Eyak	4	5	198	Mapuche	1	5
76	Chukchee	1	4	138	Kaska	3	3	200	Yahgan	5	4
				139	Kwakiutl	3	4				

A DIP IN DEATHS BEFORE CEREMONIAL OCCASIONS: SOME NEW RELATIONSHIPS BETWEEN SOCIAL INTEGRATION AND MORTALITY*

David P. Phillips and Kenneth A. Feldman
State University of New York at Stony Brook

American Sociological Review 1973, Vol. 38 (December):678-696

This paper presents evidence that there are fewer deaths than expected before three ceremonial occasions: the birthday, Presidential elections, and the Jewish Day of Atonement. The investigation of mortality before the birthday was based on biographical information on 1,333 famous persons; official vital statistics tables were used to study mortality before the other two occasions. Alternative explanations of our findings are examined; the evidence suggests that the dip in deaths before ceremonies results from some persons' postponement of death. These results are interpreted in terms of Durkheim's discussion of social integration and ceremonies.

INTRODUCTION

Durkheim suggested that an individual who is integrated into his society must be involved with its ceremonies, because a society is expressed and affirmed through its ceremonies.

There can be no society which does not feel the need of upholding and reaffirming at regular intervals the collective sentiments and collective ideas which make its unity and personality. Now this moral remaking cannot be achieved except by means of reunions, assemblies and meetings where the individuals, being closely united to one another, reaffirm in common their common sentiments; hence come [religious and political] ceremonies. . .

(Durkheim, 1961:475)

*We would like to thank the following persons for their helpful comments and suggestions: Stephen Cole, Rose Coser, Norman Goodman, Kurt Lang, David Street, Gerald Suttles, and Eugene Weinstein. We are grateful to Andrew Calia for collecting some of the information used in this paper. The preparation of this paper was made possible in part by a grant from the Russell Sage Foundation to the senior author. A nontechnical discussion of some of the material on mortality before the birthday was published earlier in Phillips (1972). Much of the material in this paper appears in an unpublished doctoral dissertation, "Dying as a form of social behavior" by the senior author Phillips (1970).

An individual, in his turn, if he is strongly attached to the society of which he is a member, feels that he is morally held to participating in its sorrows and joys; not to be interested in them would be equivalent to breaking the bonds uniting him to the group. . .

(Durkheim, 1961:446, our emphasis)

Here Durkheim is equating an individual's integration into society with his felt obligation to participate in the ceremonies of that society. The strength of this integration and the strength of this obligation may vary between two extremes. At one extreme, are persons unintegrated into their society and uninvolved with its ceremonies. According to Durkheim, these persons are so detached from society that they die prematurely by committing suicide. At the other extreme are persons highly integrated into their society and involved with its ceremonies. Extending Durkheim's argument, one might speculate that such people postpone death in order to participate in social ceremonies. Because they are so attached to society, they die postmaturely.

If some deaths are in fact postponed in this way, then there should be a dip in deaths before important ceremonies. In this paper, we will show that such "deathdips" do in fact

occur. People die less often than expected before their birthdays, Presidential elections, and the Jewish day of Atonement (examples of domestic, political, and religious ceremonies, respectively).

The notion that there is a dip in deaths before important ceremonies will be called "the deathdip hypothesis" and our explanation of it will be called "the deathdip interpretation." If this explanation is correct, then the deathdip is related theoretically to social integration and cultural ceremonies and thus it can be used as an indicator of some important social and cultural processes. The use of the deathdip to explore these processes will be discussed after the data are presented.

Mortality Before the Birthday

In seeking to establish the presence of a death-dip before the birthday, we examined the deaths of famous people only. There were two reasons for this. First, birth and death dates can be collected more easily for famous people than for ordinary people. Unfortunately, it would be difficult to collect this information for a large sample of ordinary people because an individual's birth and death dates are not available from conventional vital statistics tables. However, these dates are readily available for famous persons because their biographies are easily accessible. Secondly, the famous may be more likely than the nonfamous to produce a death-dip. A famous person's birthday is often publicly celebrated, and he may receive many gifts, much attention, and other tokens of respect. In contrast, an ordinary person receives much less attention on his birthday and may have less reason to look forward to it. Thus, if the desire to experience a birthday produces a death-dip before it, this dip should be most evident in a sample of famous persons.

Because we examined the birth and death dates of more than 1,300 people, we found it convenient to classify these events by month rather than by day. Thus, in our analysis we consider the relationship between the month of birth and month of death, rather than the relationship between day of birth and day of death. A person is said to have died on his birthmonth if the month of his death is the same as the month of his birth. Accordingly, if a person was born on March 1, 1797, and died on March 31, 1850, he died on his

birthmonth; but if he had died on February 28 instead, then he would have died in the month before his birthmonth. When one classifies events by month rather than by day, one gains convenience at the expense of precision; if there is a dip in deaths in the month before the birthmonth, one cannot determine whether the dip extends over the

Is There a Death-dip Before the Birthday?

Table 1 shows the month of birth and month of death of people listed in *Four Hundred Notable Americans* (1965). This book, originally a biographical appendix to *The Encyclopedia of American History* (1965), lists famous persons from all periods of American history. Using the data in Table 1, we can compare the number of deaths observed before the birthmonth with the number expected under the null hypothesis of independence between month of birth and month of death. There is a dip in deaths before the birthmonth, if the observed number of deaths is significantly less than the number expected.

Sixteen deaths are observed in the month before the birthmonth (the sum of the numbers in the starred cells of Table 1). Given the null hypothesis, the number of deaths expected in this period can be calculated with standard contingency table techniques. If the null hypothesis is correct, the deathmonth is independent of the birthmonth, and the deaths of those born in any given month should be distributed throughout the year in the same way as the deaths of those born in any other month. Accordingly, because 6.32% of all deaths in Table 1 fall in December (22/348), 6.32% of those born in January ($.0632 \times 38 = 2.40$) should die in December, 6.32% of those born in February ($.0632 \times 32 = 2.02$) should die in December, and so on.

The number of deaths expected in each of the starred cells was calculated by this technique, and the sum of these numbers (28.3) is the total number of deaths expected in the month before the birthmonth, given the null hypothesis of independence. The number of deaths observed before the birthmonth (16) is significantly less than the number expected under the null hypothesis (28.3); hence there is a statistically significant death-dip in the month before the birthmonth (see Appendix I).

Table 1. Number of Deaths by Month of Birth and Month of Death (Sample 1)

Month of Death	Month of Birth												Row Total
	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	
Jan	1	1*	2	1	2	2	4	3	1	4	2	4	27
Feb	2	3	1*	3	1	0	2	1	2	2	6	4	27
Mar	5	6	5	3*	1	0	5	1	2	5	3	1	37
Apr	7	6	3	2	1*	3	3	1	3	2	4	4	39
May	4	4	2	2	1	2*	4	1	3	2	1	5	31
June	4	0	4	5	1	1	1*	2	1	2	4	0	25
July	4	0	3	4	3	3	4	1*	6	4	2	5	39
Aug	4	4	4	4	2	2	3	3	1*	1	2	0	30
Sept	2	2	1	0	2	0	2	4	2	0*	5	2	22
Oct	4	2	2	3	2	2	2	3	3	1	4*	5	33
Nov	0	2	0	2	1	1	0	3	3	3	1	0*	16
Dec	1*	2	2	1	2	1	4	1	4	0	2	2	22
Column Total	38	32	29	30	19	17	34	24	31	26	36	32	348 ^a
Total													

Source: R. B. Morris, ed., *Four Hundred Notable Americans* (New York: Harper and Row, 1965).

*Deaths corresponding to month preceding birth month.

^aThe total number of deaths is less than 400 because (1) some of those in the source volume have not yet died, (2) the month of birth and/or death is not known for some of those in the volume.

Is There a Death-peak After the Birthday?

A death-rise after the birthday is not necessarily implied by the death-dip interpretation, our proposed explanation of the death-dip. If our interpretation is correct, then twelve or so persons (28.3 - 16) postponed death until after their birthdays; but depending on the manner in which their deaths were postponed, one may or may not expect a rise in deaths immediately after the birthday. For example, given the death-dip interpretation, the death-dip could arise because some people who were hovering between life and death unexpectedly recovered; in this case, it might be years before these people die, and no rise in deaths would be expected immediately after the birthday. Alternatively, the death-dip could appear because some do not die just before their

birthdays but survive a few days or weeks longer than expected; in this case, a peak in deaths would be expected soon after the birthday.

Although the death-dip interpretation does not necessarily imply a death-rise immediately after the birthday, such a rise is suggested by an exploratory, nonsystematic survey of famous Englishmen (listed in the British edition of *Who Was Who*, 1951-1960). In our exploratory survey, we found a rise in deaths in a four month period (the birthmonth and the three months thereafter), and because of this finding, we predicted a death-rise in the same period for our American sample.

Table 2 gives the observed number of deaths before, during, and after the birthmonth for the sample of famous Americans. It is evident that there is a rise in deaths in the period consisting of the birthmonth and the

three months thereafter. In this four month period 140 deaths occurred, but only 116.2 deaths are expected when contingency table methods are used. This death-rise on and after the birthmonth (140 - 116.2) is statistically significant. In sum, both a statistically significant death-dip and a statistically significant death-peak occur in our sample of famous Americans. Such a death-dip and death-peak would occur in the same sample less than twenty-five times in ten thousand, given the null hypothesis of independence between birthmonth and deathmonth, and given the procedures used to test this hypothesis (see Appendix I).

Replication of the Findings in Four Other Samples

Despite the statistical significance of our findings, the results in Table 2 could have occurred by chance (twenty-five times in ten thousand). One would be more confident that the death-dip and death-peak are not chance fluctuations if these phenomena could be replicated in other samples. Table 3 presents the number of deaths before, during, and after the birthmonth for four additional samples. Each of these samples consists of famous persons who were selected so that no sample overlaps with any other or with the first sample (examined in Tables 1 and 2). Sample 2 includes those listed as buried in Westminster Abbey in the article "Westminster Abbey" in the *Encyclopedia Americana* and *Chambers Encyclopedia*.² The other three

samples in Table 3 consist of persons who are listed in three American editions of *Who Was Who* and have surnames listed in "Foremost Families of the U.S.A." (an appendix to *Royalty, Peerage, and Aristocracy of the World* 1967). These three samples were designed to select for persons who had high *achieved* status and were likely to have high *ascribed* status also.³

In deciding whether there are fewer deaths than expected before the birthmonth in each of these samples, we could once again calculate the expected number by contingency table techniques. However, this expected number can be more easily calculated by a simpler (and nearly as accurate) method: we estimate that 1/12 of all deaths in the sample should fall in each of the months before, during, or after the birthmonth. For example, 1/12 of all sample deaths should fall in the month before the birthmonth, and 4/12 of all deaths in the birthmonth and in the three months thereafter. (Had we used this simpler estimation technique on sample 1, we would have expected 29 [348/12] deaths in the month before the birthmonth, rather than the 28.3 calculated by contingency table techniques; and we would have expected 116 [348 x 4/12] deaths on the birthmonth and in the three months thereafter, instead of the 116.2 previously calculated.)

When we compare the observed number of deaths before and after the birthmonth with the expected number (as estimated by the simpler technique), we find a death-dip and death-peak in each of the samples in Table 3. The dips and peaks are very small for samples 3, 4, and 5 but are noticeably larger for the sample of famous Englishmen (sample 2). The bottom row of Table 3 (and Figure 1) gives the number of deaths before, during, and after the birthmonth for *all five samples combined*. The dip in deaths before the birthmonth and the peak in deaths immediately thereafter are both evident. Just before the birthmonth,

¹ Of special relevance to our findings are psychophysiological studies showing differential physiological responses to pleasant and unpleasant psychological stimuli (Handlon, 1962; Malmö et al., 1957; DiMascio et al., 1957; Kissel, 1965; Sokolov, 1963; Hess and Polt, 1960) as well as studies showing differential physiological responses to various "sets" or expectations induced in the subject (as reviewed in Sternbach, 1966). For general reviews of the psychophysiological literature, see Roessler and Greenfield, 1962; Leiderman and Shapiro, 1964; Sternbach, 1966; Shapiro and Crider, 1969; Black, 1970. Some psychosomatic studies are also relevant to our findings (reviewed in Phillips, 1970). Hinton (1967) and Rees and Lutkins (1967) note that persons tend to die more often than expected soon after the deaths of their spouses. Studies on the dying patient have been reviewed by Crane (1970).

² In their articles on Westminster Abbey, none of the other popular encyclopedias (*World Book*, *International Encyclopedia*, or the *Encyclopedia Britannica*) lists persons who are buried there. A

complete list of those buried in the Abbey is apparently not available (personal communication, Westminster Abbey staff).

³ Our sampling procedure selects persons whose ascribed status is likely to be high but is not definitely so, because some persons with a distinguished surname do not come from the family that made that surname famous. A fuller description of the selection procedures used in samples 3, 4, 5 can be found in Phillips (1972).

Table 2. Number of Deaths before, during, and after the Birth Month (Sample 1)

	Months Before						The Birth Month	Months After				
	6	5	4	3	2	1		1	2	3	4	5
Number of deaths	24	31	20	23	34	16	26	36	37	41	26	34

n = 348
n/12 = 29.0

Source: Table 1

Table 3. Number of Deaths before, during, and after the Birth Month (Samples 2, 3, 4, 5 and all 5 Samples Combined)

Number of Deaths	Months Before						The Birth Month	Months After					Total	Total 12
	6	5	4	3	2	1		1	2	3	4	5		
Sample 2 ^a	9	5	9	7	4	4	5	8	13	9	4	5	82	6.8
Sample 3 ^b	17	23	26	27	28	28	42	32	31	34	36	30	354	29.5
Sample 4 ^c	10	14	12	11	8	12	15	15	15	13	20	13	158	13.2
Sample 5 ^d	39	32	29	35	31	30	36	35	38	26	31	29	391	32.6
All 5 samples combined	99	105	96	103	105	90	124	126	134	123	117	111	1333	111.1

^aSample 2 includes those listed as buried in Westminster Abbey in the article, "Westminster Abbey," in *Encyclopedia Americana* and in *Chambers Encyclopedia*.

^bSample 3 excludes those listed in *Who Was Who in America 1951-1960* who died outside of that period and those listed in *Four Hundred Notable Americans*.

^cSample 4 excludes those listed in *Who Was Who in America 1943-1950* who died outside of that period or during World War II and those listed in *Four Hundred Notable Americans*.

^dSample 5 excludes those listed in *Who Was Who in America 1897-1942* who died outside of that period or during both World Wars and those listed in *Four Hundred Notable Americans*.

21:1 (111.1-90) fewer people die than would be expected under the null hypothesis, a deficit of 19% [21.1/111.1]. On and soon after the birthmonth, 62.6 (507-444.4) more people die than would be expected under the null hypothesis, an excess of 14.1%.

Relationship Between Fame and Size of Death-dip and Death-peak

Earlier we supposed that a group of famous people is expected to produce a larger death-dip than a group of ordinary people. Elaborating this supposition, we now hypothesize that the more famous a group of people, the larger the death-dip it produces. Similarly, we hypothesize that the more fa-

mous a group the larger the death-peak it produces. In order to test these two hypotheses, we need measures of the death-dip and death-peak that are independent of each other under the null hypothesis of no relation between birth and death month. The use of independent measures enables us to test separately the hypothesis relating degree of fame to size of the death-dip and the hypothesis relating degree of fame to size of the death-peak.

Our measures can be described most concisely by the following notation:

period a = the month before the birth-month

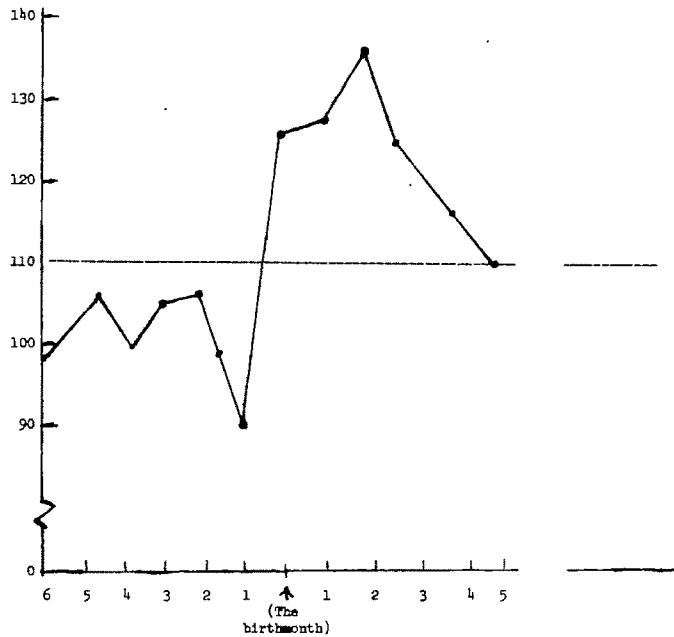


Figure 1. Number of Deaths Before, During, and After Birthmonth (All Five Samples Combined).

period b = the four month period consisting of the birthmonth and the three months thereafter

period c = the eleven month period consisting of all months *except* the month before the birthmonth

E_a = expected proportion of all deaths that should fall in the month before the birthmonth, given the null hypothesis

As we noted earlier, E_a is almost exactly $1/12$; for convenience, we will assume that E_a is *exactly* equal to $1/12$.

O_a = observed proportion of all deaths in the month before the birthmonth

The difference between the two statistics ($O_a - E_a$) is our measure of the size of the death-dip in the month before the birthmonth.

Somewhat analogous statistics can be used to measure the death-peak:

E_b = expected proportion of all deaths in period c which should fall in period b , given the null hypothesis

In the eleven months of period c , $4/11$ of all deaths should fall in the birthmonth and in the three months thereafter, given the null

hypothesis; consequently, $E_b = 4/11$.

O_b = observed proportion of all deaths in period c which fall in period b

The difference, $O_b - E_b$, measures the size of the death-peak on and just after the birthmonth.

Given the null hypothesis of no relation between birth and death month, the measure of the death-dip ($O_a - E_a$) is independent of the measure of the death-peak ($O_b - E_b$);⁴ knowing the value of ($O_a - E_a$) does not help to predict the value of ($O_b - E_b$). After classifying the members of the four hundred into groups of differing fame, we can use these statistics to measure the death-dip and death-peak for each of these groups. If our hypotheses are correct, the more famous the

⁴Given H_0 , these measures would not be independent if O_b and E_b were defined in a manner that might seem at first to be more sensible than the one adopted here:

O_b = observed proportion of *all* deaths in period b

E_b = expected proportion of *all* deaths in period b , given the null hypothesis.

If these definitions of O_b and E_b were used, the value of ($O_b - E_b$) would not be independent of the value of ($O_a - E_a$); if ($O_a - E_a$) is a large negative number, then ($O_b - E_b$) is likelier to be a large positive number.

group the larger the death-dip and the death-peak it produces.

There are obviously many ways to classify groups by fame. Our method is convenient and seems plausible. The best-known members of "the four hundred" are those whose names are "common knowledge," which may be said to consist of the information shared by almost all members of a society. To find the members of the four hundred who are commonly known, we must locate a set of people who have learned only what is common knowledge. Of all the people in a society, children come closest to meeting this description. If a child has heard of someone in the four hundred, he must be very famous indeed. Thus the members of the four hundred who appear in children's biographies may be judged to be better known than members who do not.

Two series of children's biographies were examined: Dodd, Mead's (1966) and Bobbs-Merrill's (1966). The criterion of coverage or non-coverage in these series can be used to classify members of the four hundred into groups of differing fame. Three different subgroups were formed from the original four hundred. Arranged in descending order of fame, they are:

Group 1: Those of the four hundred whose names are found in both of the children's biography series. For example, George Washington, Thomas Jefferson, Benjamin Franklin, Mark Twain, and Thomas Edison are in this group.

Group 2: Those of the four hundred whose names are in only one of the series. For example, John Quincy Adams, John Hancock, Jefferson Davis, Edgar Allen Poe,

and Alexander Graham Bell are in this group.

Group 3: Those of the four hundred whose names are in neither series. For example, Samuel Adams, Millard Fillmore, Rutherford B. Hayes, H. L. Mencken, and Nikola Tesla are in this group.

Table 4 gives the size of the death-dip and death-peak for each of these groups. As predicted, the more famous the group the larger its death-dip. This finding would occur 1/6 of the time, given the null hypothesis of no relationship between degree of fame and size of death-dip. It is also clear from this table that the more famous the group the larger its death-peak, a finding that would also occur 1/6 of the time under the null hypothesis. Given the null hypothesis, the probability that both findings would occur together in the same sample is .028 ($1/6 \times 1/6$).⁵ Additional support for these findings is evident from a comparison of the samples in Tables 1, 2, and 3; the samples containing the most famous people (samples 1 and 2) also produce the largest death-dips and death-peaks.

A DEATH-DIP BEFORE TWO OTHER OCCASIONS

Interest in the death-dip would be more fully justified if this phenomenon appeared before other important occasions in addition to the birthdays of the famous. Having shown

⁵The probability of the joint finding is the product of the probabilities of each separate finding because the size of the death-peak has been defined so as to be independent of the size of the death-dip.

Table 4. The Size of the Death-dip and Death-rise for Groups of Differing Fame

Groups of Differing Fame	Total Number in Group	Number of Deaths in the Month Before the Birth Month	Number of Deaths in Birth Month, and 1,2,3 Months Thereafter	Size of the Death-dip ($O_a - E_a$)	Size of the Death-rise ($O_b - E_b$)
1	55	1	29	-.0651	.1734
2	129	4	53	-.0523	.0604
3	164	11	53	-.0162	-.0172

Note: See text for description of groups and for definition of measures of death-dip and death-rise.

a death-dip before a domestic event, we will now demonstrate that this phenomenon exists before a political occasion and before a religious occasion as well. In this demonstration, we will use official statistical reports on *all* deaths, not just the deaths of famous persons. Thus, in our analysis of U.S. mortality before elections, we will be examining millions of deaths, not just hundreds.

A Death-dip Before U. S. Presidential Elections (1904-1968)

There is a death-dip in September and October of an election year if the mortality level in this period is less than the average mortality level in September and October of adjacent years.⁶ The following statistics are used to measure the mortality level in election years and in non-election years:

$${}_1E_i = \frac{\text{no. of U.S. deaths in Sept. and Oct. of the } i\text{th election year}}{\text{total no. of U.S. deaths in the } i\text{th election year}}$$

This is the proportion of all U.S. deaths in the *i*th election year falling in September and October of that year. In our series, 1904 is the first election year examined; hence ${}_1E_1$ is the proportion of all U.S. deaths in 1904 that fall in September and October of that year.

$$E_i = \frac{\text{no. of U.S. deaths in September and October of the year before the } i\text{th election year}}{\text{total no. of U.S. deaths in the year before the } i\text{th election year}}$$

This proportion measures the U.S. mortality level in September and October in the year *before* the *i*th election year. For example, ${}_1E_1$ is the proportion of all U.S. deaths in 1903 in September and October of that year.

⁶ Because the election campaign generates interest in the election well before Election Day, we assumed that a pre-election death-dip would be spread over a two-month period. We cannot calculate U.S. death-dips for the elections prior to 1904 or after 1968, because the necessary data are lacking. Although we found a death-peak after the birthday, we do not find a peak in mortality after elections or after the Day of Atonement.

$${}_2E_i = \frac{\text{no. of U.S. deaths in Sept. and Oct. of the year after the } i\text{th election year}}{\text{total no. of U.S. deaths in the year after the } i\text{th election year}}$$

This proportion measures the U.S. mortality level in September and October of the year *after* the *i*th election year.

If $E_i < ({}_1E_i + {}_2E_i)/2$, then there is a death-dip before the *i*th election, because the U.S. mortality level in September and October of the election year is less than the average U.S. mortality level in September and October of the adjacent (nonelection) years. The death-dip hypothesis states that there will generally be a death-dip in September and October of election years. More formally, H_1 states:

Probability($E_i > ({}_1E_i + {}_2E_i)/2$) > Probability($E_i < ({}_1E_i + {}_2E_i)/2$). This hypothesis can be opposed by a null hypothesis stating that there is no tendency for the mortality level in September and October of election years to be lower or higher than expected. That is, H_0 states:

Probability($E_i > ({}_1E_i + {}_2E_i)/2$) = Probability($E_i < ({}_1E_i + {}_2E_i)/2$) = .5.

If we let $D_i = ({}_1E_i + {}_2E_i)/2 - E_i$, we can rephrase H_0 :

Probability($D_i > 0$) = Probability($D_i < 0$) = .5.

That is, the probability that there is a pre-election death-dip equals the probability that there is none.

Table 5 gives the values of E_i , $({}_1E_i + {}_2E_i)/2$, and D_i for the seventeen presidential elections 1904-1968. The mortality level is lower than expected before thirteen out of seventeen elections; that is, $E_i > ({}_1E_i + {}_2E_i)/2$ for thirteen elections.⁷ We will reject the null hypothesis in favor of the death-dip hypothesis if, under the null hypothesis, the probability of at least thirteen death-dips out of seventeen is less than .05. This probability can easily be calculated, if we assume that the size of the death-dip before any one election (D_i) is independent of the size of the death-dip before any other. Given this assumption, and the null hypothesis, the probability of thirteen or more death-dips (out of 17) is

⁷Two of these death-dips are very small. It might be thought that one should be conservative and not consider them as dips at all. Phillips (1970:41) provides additional evidence, justifying their classification as dips.

Table 5. U.S. Mortality in September and October of Election Years Compared with U.S. Mortality in the Same Months of Control Years, 1904-1968

(1) Election Year	(2) Mortality Level in September and October of Election Year (E_i) ^a	(3) Average Mortality Level in September and October of Control Years [$(E_1 + E_2)/2$]	(4) Difference between Mortality Level in Control and in Election Years [(3)-(2)]
1904	.1473	.1525	+.0052
1908	.1578	.1579	+.0001
1912	.1575	.1528	-.0047
1916 ^b	.1529	.1521	-.0008
1920	.1380	.1469	+.0089
1924	.1486	.1497	+.0011
1928	.1485	.1503	+.0018
1932	.1480	.1526	+.0046
1936	.1472	.1507	+.0035
1940 ^c	.1509	.1515	+.0006
1944 ^c	.1532	.1533	+.0001
1948	.1553	.1557	+.0004
1952	.1571	.1559	-.0012
1956	.1592	.1623	+.0031
1960	.1576	.1595	+.0019
1964	.1613	.1576	-.0037
1968	.1560	.1609	+.0049

Sources: U.S., Department of Commerce, Bureau of the Census, *Mortality Statistics*, yearly volumes, 1903-1936. U.S., Department of Health, Education, and Welfare, Public Health Service, annual report, *Vital Statistics of the United States*, 1937-1968, and *Monthly Vital Statistics Report*, 1969.

Note: Deaths are classified by month and place of occurrence. From 1903-1932, data are for U.S. Death Registration Area; after 1932, data are for the entire U. S.

^aMortality levels for election years have been normed for a year of 365 days.

^bData for the control year, 1917, exclude deaths of the Armed Forces.

^cData for 1940-1945 exclude deaths of Armed Forces overseas.

.025.⁸ Consequently, we reject H_0 in favor of the death-dip hypothesis, and conclude that there is a statistically significant dip in U.S. mortality before U.S. presidential elections (1904-1968).

A Death-dip Before the Jewish Day of Atonement (New York City, 1921-1969)

On the Day of Atonement, the holiest day of the Jewish year, the observant Jew asks

⁸This probability is calculated from the binomial distributions ($n = 17$, $p = .5$, $x \geq 13$). Like many other tests, the binomial requires the assumption of independence among the observations (in this case, among the D_i). It would be convenient to make this assumption, but unwise to do so, if there is evidence against it. Phillips (1970) tested for dependence among the D_i and found none: consequently we assume independence among the D_i and apply the binomial test to them. For a more detailed discussion and for the application of this argument to the other data examined, see Phillips (1970).

and receives forgiveness for his misdeeds of the previous year. On this day, the Jew is most free from sin, and there is a legend that all Jews who die on this day will be admitted to Heaven. In our examination of mortality before the Day of Atonement (Yom Kippur), we would have preferred to examine Jewish deaths only, but this is not possible with U.S. vital statistics tables. Instead, we examined mortality in New York City, a city whose population was approximately 28% Jewish from 1921 to 1969.⁹

In the search for a death-dip before Yom Kippur, one must use techniques different from those used in the investigation of pre-election mortality. Because Presidential elec-

⁹Phillips (1970) documents the size of the Jewish population from 1921-1969. At the beginning of this investigation it was decided arbitrarily to begin the series of observations with the last year (1921) in which there was large-scale Jewish immigration into the U.S. In this year, the first immigration quota bill was introduced.

tions do not occur every year, one can use non-election years as "controls" to determine whether pre-election mortality is unusually low. This procedure cannot be employed for yearly events like Yom Kippur and other holidays. For most such annual occasions, there is no way to separate the presumed effect of the occasion on mortality from the effect of the season in which the occasion occurs. Thus, for example, any death-dip found before Christmas might be due to this holiday, but on the other hand, it might be merely a seasonal fluctuation in mortality.

However, it is possible to separate the effect of the occasion from the effect of the season for those annual occasions which occur at a different date each year. If the death-dip before an occasion like Yom Kippur "moves around" the calendar in a way that corresponds to the "movement" of Yom Kippur, then the death-dip is associated with Yom Kippur and not with the season in which this holiday occurs. This point is evident from Figure 2, which displays hypothetical daily mortality before Yom Kippur, on the assumption that there is a death-dip extending over a one month period before this occasion.¹⁰

Inspection of Figure 2 reveals that the September mortality level should be lowest for those years in which Yom Kippur falls around September 30, say September 28-October 3, inclusive. Yom Kippur fell on one of these days in 1922, 1925, 1930, 1933, 1941, 1949, 1952, 1960, 1963, 1968, during our observation period. These years will be called "Y-years." The following statistics are used to measure the September mortality level in a Y-year and in the years adjacent to it:

$$Y_1 = \frac{\text{no. of N.Y.C. deaths in Sept. of the } i\text{th Y-year}}{\text{total no. of N.Y.C. deaths in the } i\text{th Y-year}}$$

This proportion measures the N.Y.C. September mortality level in a Y-year. For example, Y_1 is the proportion of all N.Y.C. mortality in the first Y-year (1922) falling in September of that year; similarly, Y_2 is the proportion of all N.Y.C. mortality in September of the second Y-year, 1925.

¹⁰Informal observation suggests that Yom Kippur, unlike the presidential election, arouses little interest two months before its arrival. Consequently, it was assumed before the analysis that if there were a dip before Yom Kippur, it would extend over a one month period only.

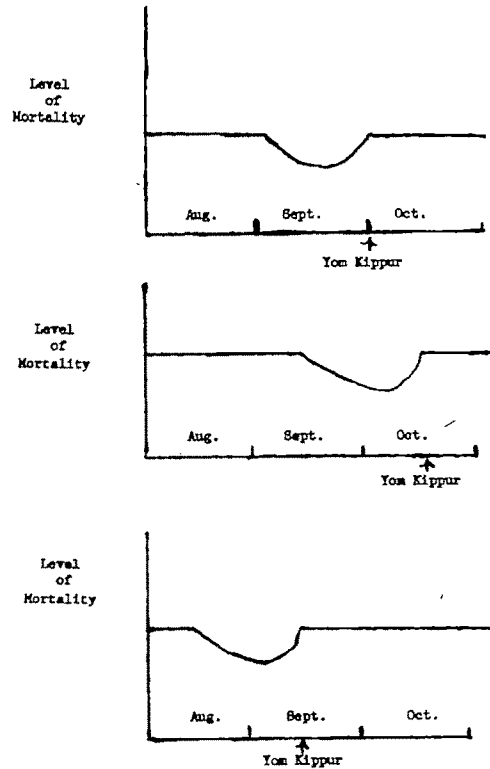


Figure 2. Assumed Relationship Between the Death-dip and Yom Kippur, for Different Dates of this Occasion.

$${}_1Y_i = \frac{\text{no. of N.Y.C. deaths in Sept. of the year before the } i\text{th Y-year}}{\text{total no. of N.Y.C. deaths in the year before the } i\text{th Y-year}}$$

This proportion measures N.Y.C. September mortality level in the year before the i th Y-year. For example, ${}_1Y_i$ is the proportion of all N.Y.C. deaths in 1921 falling in September of that year.

$${}_2Y_i = \frac{\text{no. of N.Y.C. deaths in Sept. of the year after the } i\text{th Y-year}}{\text{total no. of N.Y.C. deaths in the year after the } i\text{th Y-year}}$$

This proportion measures the N.Y.C. September mortality level for the year after the i th Y-year.

The size of the N.Y.C. death-dip in September of a Y-year is measured by the difference between the N.Y.C. September mortality level in the Y-year and the average N.Y.C. September mortality level in the adjacent years:

$$({}_1Y_i + {}_2Y_i)/2 - Y_i = ND_i$$

We found it convenient to define equivalent statistics of U.S. mortality in these periods. This can be done by substituting a "U" for the "Y" in the above statistics, and by substituting "U.S." for "N.Y.C." For example, the size of the U.S. death-dip in September of the i th Y-year is measured by:

$$({}_1U_i + {}_2U_i)/2 - U_i = UD_i$$

This is the difference between the U.S. mortality level in September of the Y-year and the average U.S. mortality level in September of the adjacent years.

In terms of these statistics, the death-dip hypothesis states:

H_1 : Probability ($ND_i > 0$) > Probability ($ND_i < 0$)

The null hypothesis opposed to this states that the probability of a N.Y.C. death-dip equals the probability of no New York City death-dip:

H_0 : Probability ($ND_i > 0$) = Probability ($ND_i < 0$) = .5

The first column of Table 6 gives the values of ND_i for the ten Y-years in the period 1921-1969. $ND_i > 0$ for eight of these years.

It is difficult to decide whether this evidence justifies rejection of H_0 , because it is difficult to evaluate the probability under H_0 of eight or more death-dips. The results of our investigation of pre-Yom Kippur mortality are in part predictable from the results of our investigation of pre-election mortality; knowing the size of the U.S. death-dip in September and October of an election year (D_i) helps one to predict the size of the N.Y.C. death-dip in September of a Y-year (ND_i).¹¹

This difficulty may be circumvented by rephrasing the death-dip hypothesis (H_1) and the null hypothesis (H_0) so that testing the null hypothesis is unaffected by the statistical dependence of ND_i and D_i . If H_1 is true, then H_1' should be true also. H_1' states that the size of the N.Y.C. death-dip in September of a Y-year (ND_i) should generally be bigger than the size of the U.S. death-dip in September of that year (UD_i). H_1' states: Probability ($ND_i > UD_i$) > Probability ($ND_i < UD_i$). On the other hand, if H_0 is correct (there is no

death-dip associated with Y-year Yom Kippur), then H_0' should be true also. H_0' states:

Probability ($ND_i > UD_i$) = Probability ($ND_i < UD_i$) = .5

That is, the New York City death-dip in September of a Y-year has no tendency to be smaller or larger than the U. S. death-dip in the same period. Given H_0' , knowing the size of U.S. pre-election death-dips (D_i) does not help one to predict the difference in size between N.Y.C. and U.S. death-dips ($ND_i - UD_i$) in September of a Y-year. Accordingly, we can test H_0' even though we have already examined U.S. pre-election mortality.

Table 6 gives the size of the N.Y.C. death-dip in September of a Y-year (ND_i), the size of the U.S. death-dip in that period (UD_i) and the difference in size of the two death-dips ($ND_i - UD_i = Q_i$). The N.Y.C. death-dip is larger than the U.S. death-dip nine times out of ten. Given H_0' , the probability that $ND_i > UD_i$ at least nine out of ten times is .011. We reject H_0' in favor of the death-dip hypothesis that, in Y-years, the N.Y.C. death-dip before Yom Kippur is larger than the U.S. death-dip in that period.

Replication of Pre-Yom Kippur Death-dip (Budapest, 1875-1915)

Despite the statistical significance of our findings for New York City, one would be more confident that a death-dip exists before Yom Kippur if a death-dip before Yom Kippur appeared in an additional population. There are few other American cities in which Jews constitute a large fraction of the population. However, at least one European city, Budapest, had a large Jewish population at one time.¹² During the period 1875-1915, approximately 22% of Budapest's population was Jewish (Budapest statistische Amt, 1925).

Between 1875 and 1915, Y-years occurred on 1876, 1881, 1884, 1887, 1892, 1895, 1900, 1903, 1906, 1911, 1914. Given the death-dip hypothesis, the September mortality level in each Y-year should be lower than the average mortality level in September of adjacent years. Our measures of Budapest mortality levels are analogous to our measures for New York City:

$$B_i = \frac{\text{no. of Budapest deaths in September of the } i\text{th Y-year}}{\text{total no. of Budapest deaths in the } i\text{th year}}$$

¹¹ We need mention only one reason for the dependence between ND_i and D_i . Some of the data used to calculate ND_i are also used to calculate D_i . For example, ND_7 is calculated from New York City mortality in 1951, 1952, 1953, and in September of these years. D_7 is calculated from U. S. mortality in 1951, 1952, 1953, and in September and October of these years.

¹² We would like to thank Dr. Elvira S. Rapaport for mentioning this fact.

Table 6. Comparison of N.Y.C. and U.S. Death-dips before "Y-Year"
Yom Kippurs, 1921-1969

"Y-Year"	The Size of the N.Y.C. Death-dip Before Yom Kippur (N^D_i)	The Size of the U.S. Death-dip Before Yom Kippur (U^D_i)	Difference in Size of N.Y.C. and U.S. Death-dips ($N^D_i - U^D_i$)
1922	+ .0068	+ .0022	+ .0046
1925	- .0019	- .0027	+ .0008
1930	+ .0008	- .0009	+ .0017
1933	+ .0001	- .0023	+ .0024
1941	+ .0015	+ .0014	+ .0001
1949	- .0032	- .0018	- .0014
1952	+ .0029	+ .0014	+ .0015
1960	+ .0032	+ .0019	+ .0013
1963	+ .0034	+ .0013	+ .0021
1968	+ .0036	+ .0024	+ .0012

Sources: New York City data--data for 1959, 1962, 1963, 1964 are from New York State, Department of Health, *Monthly Vital Statistics Review*; data for 1967, 1969 are from New York City, Department of Health, annual report, *Summary of Vital Statistics* and *Monthly Vital Statistics*; data for 1968 are from New York City, Department of Health, *Service and Vital Statistics Tables (1968) for Health Center Districts, New York City*; data for all other years are from State of New York, Office of Biostatistics, *Annual Statistical Report of the Department of Health*. U.S. data--U.S., Department of Commerce, Bureau of the Census, *Mortality Statistics*, yearly volumes, 1903-1936. U.S., Department of Health, Education, and Welfare, Public Health Service, annual report, *Vital Statistics of the United States*, 1937-1968, and *Monthly Vital Statistics Report*, 1969.

Note: Prior to 1930, N.Y.C. deaths are by place of occurrence. For 1930 and later, N.Y.C. deaths are by place of residence, but decedents whose usual place of residence is New York City are "credited" to that city only if they die within New York State. Calculations for all leap years have been normed for a year of 365 days.

$${}_1B_i = \frac{\text{no. of Budapest deaths in Sept. of the year before the } i\text{th Y-year}}{\text{total no. of Budapest deaths in the year before the } i\text{th Y-year}}$$

$$({}_1B_i + {}_2B_i)/2 - B_i = BD_i$$

$${}_2B_i = \frac{\text{no. of Budapest deaths in Sept. of the year after the } i\text{th Y-year}}{\text{total no. of Budapest deaths in the year after the } i\text{th Y-year}}$$

The death-dip hypothesis can be phrased in terms of these statistics:

Probability ($BD_i > 0$) > Probability ($BD_i < 0$)

The null hypothesis states:

Probability ($BD_i > 0$) = Probability ($BD_i < 0$)

The first statistic measures the Budapest mortality level in September of the i th Y-year. For instance, B_1 measures the Budapest September mortality level in the first Y-year (1876). The last two statistics measure September mortality levels in the years before and after the Y-year. The size of the Budapest death-dip in September of a Y-year is measured by the difference between the September mortality level in the Y-year and the average mortality level in September of the adjacent years:

Table 7 gives the values of B_i , $({}_1B_i + {}_2B_i)/2$, and BD_i for 1875 to 1915. There is a death-dip ($BD_i > 0$) for nine of the eleven Y-years in this period. We will reject H_0 in favor of the death-dip hypothesis if, under the null hypothesis, the probability of at least nine death-dips out of eleven is less than .05. This probability is .032; accordingly we reject H_0 in favor of the hypothesis that, in the Y-years 1875-1915, there is generally a dip in Budapest mortality in the month before Yom Kippur. When taken together, our Budapest and New York City findings show a death-dip before Yom Kippur over a ninety year observation period (1875-1915, 1921-1969).

Table 7. Budapest Mortality in September of "Y-Years" Compared with Budapest Mortality in September of Control Years, 1875-1915

(1) Y-Year	(2) Mortality Level In September of Y-Year (B_i)	(3) Average Mortality Level in September of Control Years [$(B_1 + B_2)/2$]	(4) Difference between Mortality Level in Control Years and in Y-Year [(3)-(2)]
1876	.0672	.0715	+.0043
1881	.0751	.0693	-.0058
1884	.0624	.0688	+.0064
1887	.0728	.0811	+.0083
1892	.0656	.0725	+.0069
1895 ^a	.0749	.0796	+.0047
1900	.0655	.0687	+.0032
1903 ^a	.0709	.0781	+.0072
1906 ^a	.0680	.0781	+.0101
1911 ^a	.0809	.0699	-.0111
1914 ^a	.0710	.0728	+.0018

Sources: Except for the years 1894-1898 and 1902-1915, all data come from Budapest, *Die Fünfzigjährige Entwicklung Budapests, 1873-1923*, Publicationen des Statistischen Amtes der Haupt- und Residenzstadt Budapest, Budapest, 1925. Data for 1894-1898, 1902-1904, 1908-1912 are from Budapest, *Statistisch-Administratives Jahrbuch der Haupt- und Residenzstadt Budapest*, Communal Statistisches Bureau, Budapest. This source also provides Jewish annual mortality for 1905-1907, 1913-1915, but Jewish September mortality for those years are taken from Budapest, *Monatshefte der Budapester Communal-statistischen Bureau*.

Note: Deaths are classified by place of residence. Calculations for leap years have been normed for a year of 365 days.

^aThe death-dips for these years have been calculated for Jewish mortality only. Information on monthly Jewish mortality does not seem to exist for 1893 and earlier, or for 1899-1901.

Variation in the Size of the Death-dip by the Religion of Those Dying

If the desire to experience Yom Kippur helps a person to survive beyond that occasion, then Jews should produce a larger death-dip before Yom Kippur than non-Jews.¹³ Because the appropriate data are lacking, the death-dips of different religious groups cannot be compared in New York City, but they can be compared in Budapest, where mortality statistics are classified by religion for at least some of the years we have examined. In all, Jewish and non-Jewish death-dips can be calculated for five Y-years: 1895, 1903, 1906, 1911, 1914. Table 8 presents the size of these death-dips: For all five Y-years, the Jewish death-dip is larger than the non-Jewish death-dip. The probability of this result is .031, given the null hypothesis that the Jewish death-dip has no tendency to be larger or smaller than the non-Jewish death-dip. Jews produce a larger

death-dip before Yom Kippur than do non-Jews.

Alternative Interpretations of the Death-dip Phenomenon

There is good evidence for the existence of the death-dip because statistically significant death-dips have been found in a variety of locations and time.¹⁴ Nevertheless, our explanation of this phenomenon must remain tentative. The death-dip interpretation correctly predicted the occurrence of death-dips; still, one might prefer a more conventional interpretation of the death-dip phenomenon. Four such interpretations will be discussed here (for further details, see Phillips, 1970).

1) *The Effect of Misregistration.* It is possible that an impending social event is important enough to disrupt the activities of medical and legal officials, who postpone the registration of deaths until after the event, thus creating a death-dip before it. This

¹³We have not hypothesized that Jews alone should produce a death-dip. Persons labeled "non-Jews" may produce a small one before Yom Kippur if some of them converted from Judaism but retained parts of their old religious identity.

¹⁴In addition, there is some evidence of a death-dip before the Olympic Games (in the cities that hold them), and before Thanksgiving (Phillips, unpublished analyses). A death-dip before 20th century British coronations was sought but not found.

Table 8. Jewish and Non-Jewish Death-dips before "Y-Year" Yom Kippur, Budapest, 1895-1914

"Y-Year"	The Size of the Jewish Death-dip	The Size of the Non-Jewish Death-dip
1895	+ .0047	- .0033
1903	+ .0072	+ .0021
1906	+ .0101	+ .0011
1911	- .01108	- .01113
1914	+ .0018	- .0024

Note: See sources cited in Table 7.

explanation cannot account for the death-dip before the birthdays of famous persons, because their deaths are a matter of historical record and are hardly subject to registration error. It is plausible to suppose that any disruption of registration activities before Yom Kippur would be most evident just prior to that event. If this is so, the death-dip before Yom Kippur should be concentrated in the few days before that occasion. However, using daily and weekly mortality records, Phillips (1970) has shown that the death-dip is not concentrated in this fashion but instead extends over the entire month before Yom Kippur. This evidence seems to be inconsistent with the misregistration explanation. It is conceivable that registration activities are disrupted during the two months prior to the Presidential election; even if this is so however, the death-dip before elections does not result from late registration of deaths, because the U.S. mortality we have analysed is classified by date of occurrence, not by date of registration.

2) *The Effect of the Seasons on Mortality.* We have previously noted that the procedure used to find a death-dip before Yom Kippur corrects for the effects of the seasons on mortality. We controlled similarly for seasonal effects on pre-election mortality by comparing deaths in September and October of election years with deaths in the same period of non-election years. The death-dip before the birthdays of famous Americans was demonstrated by showing that the observed number of deaths before the birthday is less than the number expected. Because this expected number was calculated by contingency table techniques, which take into account the seasonal distribution of births and deaths, any death-dip found before the birthday cannot be explained as a seasonal fluctuation in births or deaths.

3) *The Effects of Unusual Excitement.*

The excitement of a birthday celebration might overstrain an old person and lead to his death. If this is so, one might find a peak in deaths just after the birthday. If there are more deaths than expected just after the birthday, then it is likely that there will be fewer deaths than expected in any other period, for example, just before the birthday. In other words, the existence of a death-peak after the birthday increases the chances of a death-dip just before it. This might be a plausible interpretation of the death-peak after the birthday, but it is not an adequate explanation of the death-dip just before it. The "excitement interpretation" implies a generalized dip in deaths spread evenly over all months except the birthmonth and the three months thereafter. This sort of death-dip is not observed. Instead, we find a more localized death-dip which seems to be concentrated in the month before the birthday, and this type of death-dip is not explainable by the "excitement interpretation." In addition, this interpretation cannot explain the death-dips before the elections or before the Day of Atonement, because a death-peak is not observed after these ceremonies.

4) *The Effects of Medical Care.* Physicians may work unusually hard to protect the lives of patients who are anxious to experience an important event. If this is true, then any death-dip produced by this medical process should be very much smaller in the early years of U.S. history, when medical care was much less effective than it is today. The evidence is not consistent with this expectation. If we classify the four hundred into those who die before the median year of death (1894) and those who die thereafter, we find that the death-dip produced by the early group is about the same size as the death-dip produced by the later one. Of the early group 4.65%

(8/172) die in the month before the birth-month; while 4.62% (8/173) of the later group die in this period. The sample of famous Englishmen also provides data inconsistent with the medical care explanation. The median year of death for the British sample is 1,778; 2.5% (1/40) of the early group die in the month before the birth-month, while 7.32% (3/41) of the later group die in this period. A more detailed analysis of the death-dip produced at different times may yet uncover support for the medical care interpretation; but at present, such support is lacking.

In the future, new tests of the death-dip interpretation may render it invalid and new interpretations of the death-dip may be more consistent with the data. At present, however, the death-dip interpretation appears to be the best of those suggested here.

Discussion

The death-dip, like suicide, is a minor demographic phenomenon¹⁵ and its sociological interest lies mainly in the light it sheds on social and cultural processes. The size of the death-dip before a ceremony appears to reflect the degree to which people are involved within that ceremony, and consequently the degree to which they are attached to that segment of the culture which is symbolized by the ceremony. If this is so, the death-dip can be used as a technique to investigate a variety of social problems. By comparing the size of the death-dip before the different ceremonies of a culture, we can estimate the relative importance of different segments of that culture. Comparison of the size of the death-dip before the same ceremony in different cultures should reveal similarities and differences between those cultures. By examining temporal changes in the size of the death-dip before a ceremony, we can learn about the fluctuating importance of that ceremony and of the cultural segment it symbolizes. The use of the death-dip technique to investigate these

and other problems will be discussed in more detail below.

The death-dip phenomenon can be used to investigate the detachment of a suicide from his culture. It is probable that this detachment proceeds gradually, first from one aspect of the culture, then from another, until finally the suicide is so much separated from society that he separates himself from life. It is possible that each suicide becomes socially detached in an idiosyncratic fashion: one loses interest in religion but remains involved in politics, while another does the opposite. However, it is more likely that suicides conform to a pattern of detachment; perhaps they lose interest in the peripheral aspects of their culture before the core of their culture ceases to concern them. Because monthly suicide statistics are available, we can easily compare the suicide's response to a particular ceremony with the response of those who die from other causes. If suicides generally lose interest in some ceremonies but maintain interest in others, they should produce death-dips before the latter ceremonies, but not before the former. Furthermore, because suicides are less attached to society than ordinary people, they should produce death-dips before a smaller range of ceremonies than ordinary people do. By comparing a list of ceremonies before which suicides produce death-dips with a list of ceremonies before which ordinary people produce death-dips, we can learn how much suicides have detached themselves socially, and in what way.

Emigrants, like suicides, detach themselves from a culture, but, unlike suicides, they acquire a new one. The death-dip phenomenon can be used to investigate the ways in which emigrants lose one culture and gain another.¹⁶ How long does it take before the ceremonies of the mother culture cease to interest the emigrants? Which segments of the mother culture are given up first, and which are relinquished last of all? Is this "core culture" of the emigrant similar to the "core culture" of the suicide, or do suicides and emigrants detach themselves from their cultures in different ways? How long does it take before the ceremonies of the host culture begin to interest the emigrants? During the assimilation of the emigrant group, there may be a time when they are attached to the

¹⁵In the United States, suicides constitute about 1% of all recorded deaths (see *Vital Statistics of the United States*, yearly volumes). On the average, about 1% of those expected to die before Presidential elections do not do so. The corresponding figure for the New York City death-dip before Yom Kippur is 2%. About 3% of Budapest Jews expected to die before Yom Kippur do not do so. In our pooled sample of famous people, about 17% of those expected to die before their birthdays do not do so. For a more detailed analysis of the size of the death-dip before various occasions, see Phillips (1970).

¹⁶The birthplace and citizenship of the deceased is normally listed on his death certificate (United Nations, 1954). Consequently, emigrant mortality can be separated from the mortality of indigenous populations, and we can calculate the size of the death-dip produced by emigrants alone.

ceremonies of both their cultures. For a time, they might have a "double culture," somewhat like Jews who celebrate both Christmas and Chanukah. On the other hand, there may be a time when the emigrant is marginal to both his cultures; perhaps he has been taught contempt for his mother culture and its ceremonies but has not yet committed himself to the ceremonies of his host culture. The size of the death-dip before the ceremonies of the two cultures should reflect the changing values of these events in the lives of the emigrants.

A particular segment of the culture (and its associated ceremonies) may be more important in one society than in another, and the relative importance of this segment could be investigated with the death-dip phenomenon. For example, elections probably interest Americans more than Russians; and if this is so, there should be a larger death-dip before U.S. elections than before the equivalent Russian events. Similarly, Russians are unlikely to produce death-dips before religious ceremonies; while Italians or Spaniards might be expected to do so. By comparing a list of ceremonies before which Russians produce death-dips with a list of ceremonies before which Italians produce death-dips, one might learn about the similarities and differences between the two cultures.

A similar technique might be used to compare and contrast subcultures within a society. In our society, males and females probably have different subcultures, and they probably value some U.S. ceremonies differently. Traditionally, men have been more interested than women in sports and in politics; while women are probably more interested in religion. There is some fragmentary evidence of a death-dip before the Olympic Games in the cities that hold them (Phillips, unpublished analyses). Do males produce a larger death-dip than females before this event? In general, what is the list of ceremonies associated with large male death-dips, and what is the equivalent list for females? How much do these lists overlap (how similar are the male and female subcultures) and what is the nature of the ceremonies associated equally with male and female death-dips? With time and with increasing equality between the sexes, have the male and female subcultures merged more and more completely?

Similar questions might be asked of other subcultures, for instance, that of the young, and that of the old. Has the "generation gap" between these subcultures widened, so that the ceremonies which interest the old arouse less and less interest in the young?

It is possible that subcultures overlap so that they all share a common "core" culture, symbolized by a set of "core" ceremonies which generate death-dips in all subcultures. Alternatively, subcultures might overlap like the strands of a rope—each strand overlaps with some others, but no strand overlaps with all. In this case, there should be no core ceremonies in the society.

Finally, cultural changes can also be investigated with the death-dip. In discussing the historical changes in his society, Durkheim (1961:476) noted:

The French Revolution established a whole cycle of holidays to keep the principles with which it was inspired in a state of perpetual youth (Durkheim, 1961:476).

[But] the great things of the past which filled our fathers with enthusiasm do not excite the same ardor in us. . . We can no longer impassionate ourselves for the principles of . . . Christianity . . . and human equality and fraternity seems to us today to leave too large a place for unjust inequalities. . . In a word, the old gods are growing old, or already dead, and others are not yet born (Durkheim, 1961:475).

Durkheim used his observations to explain the high French suicide rate, but we can use them for another purpose — to predict that death-dips before postrevolutionary ceremonies should grow larger for a while (as the new culture takes root) and then smaller as disillusionment and disinterest set in.¹⁷ Assuming that Durkheim's observations and the death-dip interpretation are both correct, one would also predict that the death-dips before the ceremonies of Durkheim's day should be quite small indeed.

Some Advantages and Disadvantages of the Death-dip Indicator

Like suicide, the death-dip is a social¹⁸ indicator constructed from mortality sta-

¹⁷ These predictions can be checked with French mortality statistics, which have been collected since 1792 (Shryock and Siegel, 1971:28).

¹⁸ At present, we have restricted ourselves to the sociological implications of the death-dip; biological and psychological implications have not been considered. In the future, one might wish to calculate the size of the death-dip produced by each disease separately. These calculations might reveal the degree to which diseases are differentially susceptible to psychological or social phenomena — in other words, the degree to which they are psychosomatic.

tistics; the nature of these statistics helps to determine the methodological strengths and weaknesses of the death-dip technique. First, mortality statistics are amongst the most accurate sources of social scientific data; consequently, the size of the death-dip can be accurately calculated. Secondly, death-dips can be calculated for a wide range of places and times, because many nations and their subunits collect and publish mortality statistics, and some have been doing so since the nineteenth century.¹⁹ Finally, the death-dip is an unobtrusive measure, in contrast to many conventional indicators of attitude or opinion. Verbal responses to a question are sometimes biased, because the respondent is too embarrassed or scared to reveal his true feelings. The death-dip as an indicator is unlikely to suffer from this bias.

Mortality statistics also have a number of limitations, which tend to restrict the applicability of the death-dip technique. First, mortality tables do not provide detailed information on the characteristics of the deceased. For example, many countries do not tabulate deaths by religion. In some cases, this type of limitation can be overcome by examining the mortality of very small areas, which tend to contain homogeneous populations. Thus, for

example, New York City classifies mortality statistics by race for thirty subareas of the city. When non-whites are excluded from the analysis, some of these subareas are almost entirely Jewish. If necessary, one could also get more detailed information on the characteristics of the deceased from an examination of his death certificate, or from biographical data.

A second limitation of the death-dip technique has been alluded to earlier in this paper: rigorous demonstration of a preceremonial death-dip is not possible if the ceremony occurs once a year at exactly the same date. Thus, we can seek a death-dip before Easter but not before Christmas, before Election Day but not before July 4th, before the World Series but not before the Indianapolis 500. Although a death-dip before a fixed annual event cannot be rigorously demonstrated, presumptive evidence for a death-dip can be provided if the annual event is more important to some people than to others. If Christians are less likely than Jews to die before Christmas, if patriots are less likely than others to die before July 4th, then there is some evidence that these events generate death-dips before them.

Despite these limitations, one could easily mention additional social and cultural processes that might be investigated with the death-dip technique. However, as Durkheim noted at the end of *Suicide*, "the important thing is not to draw up in advance a plan anticipating everything, but rather to set resolutely to work."

It would be interesting to see if diseases with unknown etiology produce larger death-dips than diseases whose causes are known.

The psychological implications of the death-dip might also be of interest. What are the specific motives which underly the general desire to experience a ceremony? Perhaps these motives can be analyzed in terms of the rewards associated with the realization of an identity appropriate to a ceremonial occasion. Although the activation of an identity (and of the rewards contingent upon it) is often problematic in everyday settings, such precariousness may well be less common during ceremonies. On these occasions, there is consensus on the identity to be activated; and the rewards for the activation can be reliably anticipated. Consequently, the individual may be unusually motivated to experience a ceremony, because on these occasions the rewards of identity activation may be unusually reliable and strong.

¹⁹Death registration systems were started in Austria, in 1784; in Belgium, 1796; Denmark, 1646; France, 1792; Ireland, 1864; Italy, 1865; Netherlands, 1811; Norway, 1685; Portugal, 1911; Sweden, 1686; United Kingdom (Scotland), 1864; United Kingdom (England and Wales), 1875. Some of the United States started death registration systems as early as 1848, but the U. S. as a whole started such a system in 1900, and attained full coverage in 1933 (Shryock and Siegel, 1971:28-9).

REFERENCES

- Adam and Charles Black Publishing Company
1961 Who was Who. Vol. 5. London: Adam and Charles Black.
- A.N. Marquis Company
1943 Who Was Who in America, 1897-1942. Vol. 2. Chicago: A.N. Marquis Company.
1951 Who Was Who in America, 1943-1950. Vol. 3. Chicago: A.N. Marquis Company.
1961 Who Was Who in America, 1951-1960. Vol. 4. Chicago: A.N. Marquis Company.
- Annuaire De La Noblesse De France
1967 Royalty, Peerage, and Aristocracy of the World. London: Annuaire De La Noblesse De France.
- Anonymous
1966 "Westminster Abbey." Chambers Encyclopedia Vol. 14:526-7. Oxford: Pergamon. Pergamon.

- 1972 "Westminster Abbey." Encyclopedia Americana Vol. 28:668-9. New York: Americana Corporation.
- Black, P. (ed.)
1970 Physiological Correlates of Emotion. New York: Academic Press.
- Bobbs-Merrill Company
1966 Childhood of Famous Americans Series. Indianapolis: The Bobbs-Merrill Company.
- Budapest
1925 Die Funfzigjahrige Entwicklung Budapests, 1873-1923. Budapest: Communal Statistisches Bureau.
1875-1915 Monatshefte der Budapester Communalstatistischen Bureaus. Budapest: Communal Statistisches Bureau.
1875-1915 Statistisches-Administratives Jahrbuch der Haupt-und Residenzstadt Budapest. Budapest: Communal Statistisches Bureau.
- Crane, D.
1970 "Dying and its dilemmas as a field of research." Pp. 303-25 in O.G. Brim et al. (eds.), The Dying Patient. New York: The Russell Sage Foundation.
- Dimascio, A. et al.
1957 "Physiological correlates of tension and antagonism during psychotherapy: a study of 'interpersonal physiology'." Psychosomatic Medicine. 19 (March-April): 99-104.
- Dodd, Mead and Company
1966 Famous Biographies Series. New York: Dodd, Mead and Company.
- Durkheim, E.
1961 The Elementary Forms of the Religious Life. New York: Collier Books.
- Farberow, N.S.
1972 Bibliography on Suicide and Suicide Prevention. Washington: Government Printing Office.
- Handlon, J.
1962 "Hormonal activity and individual responses to stresses and easements in everyday living." Pp. 157-170 in R. Roessler and N.S. Greenfield (eds.), Physiological Correlates of Psychological Disorder. Madison: University of Wisconsin Press.
- Harvard Computation Laboratory
1955 Tables of the Cumulative Binomial Distribution. Cambridge: Harvard University Press.
- Hess, E.H. and J.M. Polt
1960 "Pupil size as related to interest value of visual stimuli." Science 132(August):349-51.
- Hinton, J.
1967 Dying. Baltimore: Penguin Books.
- Kissel, S.
1965 "Stress reducing properties of social stimuli." Journal of Personality and Social Psychology 2 (September):378-84.
- Leiderman, P.H. and D. Shapiro (eds.)
1964 Psychobiological Approaches to Social Behavior. Stanford: Stanford University Press.
- Morris, R.B. (ed.)
1965a Four Hundred Notable Americans. New York: Harper and Row.
- 1965b The Encyclopedia of American History. New York: Harper and Row.
- New York City Department of Health
1967-1969 Annual Report. New York: Department of Health.
1959-1964 Monthly Vital Statistics. New York: Department of Health.
1968 Service and Vital Statistics Tables for Health Center Districts of New York City. New York: Department of Health.
1959-1964 Summary of Vital Statistics. New York: Department of Health.
- New York State Department of Health
1921-1969 Annual Statistical Report of the Department of Health. Albany: State Department of Health.
1959-1964 Monthly Vital Statistics Review. Albany: State Department of Health.
- Phillips, D.P.
1970 Dying as a Form of Social Behavior. Unpublished doctoral dissertation, Princeton University.
1972 "Deathday and birthday: an unexpected connection." Pp. 52-65 in J. Tanur et al. (eds.), Statistics: Guide to the Unknown. San Francisco: Holden-Day.
- Rees, W.D. and S.G. Lutkins
1967 "Mortality of bereavement." British Medical Journal 4(October):13-6.
- Roessler, R. and N.S. Greenfield (eds.)
1962 Physiological Correlates of Psychological Disorder. Madison: University of Wisconsin Press.
- Shapiro, D. and A. Crider
1969 "Psychophysiological approaches to social psychology." Pp. 1-49 in G. Lindzey and E. Aronson (eds.), The Handbook of Social Psychology. Vol. 3. 2nd ed. Reading: Addison-Wesley.
- Shryock, H.S. and J.S. Siegel
1971 The Methods and Materials of Demography. Washington: Government Printing Office.
- Sokolov, Y.N.
1963 Perception and the Conditioned Reflex. Oxford: Pergamon Press.
- Sternbach, R.A.
1966 Principles of Psychophysiology. New York: Academic Press.
- United Nations Department of Economic and Social Affairs
1955 Handbook of Vital Statistics Methods. New York: United Nations
- United States Bureau of the Census
1900-1936 Mortality Statistics. Washington: Government Printing Office.
United States Public Health Service
1969 Monthly Vital Statistics Report. Washington: Government Printing Office
- United States Public Health Service
1937-1968 Vital Statistics of the United States. Washington: Government Printing Office.
1969 Monthly Vital Statistics Report. Washington: Government Printing Office.

Appendix I

Evaluating the Statistical Significance of the Death-dip Before the Birthmonth and of the Death-peak Thereafter

The Statistical Significance of the Death-dip Before the Birthmonth (Sample 1).

To calculate the statistical significance of the death-dip before the birthmonth, we need to assume that the birth and death dates of any person in Sample 1 are independent of the birth and death dates of any other person in the sample. It is difficult, perhaps impossible, to evaluate this assumption; consequently, we will accept the assumption without attempting to test it. Given this assumption, we can now show that the death-dip before the birthmonth and the death-peak thereafter are both statistically significant.

Earlier, on page 8 we noted that, under the null hypothesis of independence between the month of birth and month of death, almost exactly 1/12 of all deaths are expected to fall in the month before the birthmonth. For convenience in testing H_0 , we will assume that this probability is *exactly* equal to 1/12. We observe sixteen deaths in the month before the birthmonth. If the probability under H_0 of sixteen or fewer deaths out of 348 deaths is less than .05, we will reject H_0 in favor of the alternative hypothesis of a dip in deaths before the birth month. This probability is approximately .01 (see *Tables of the Cumulative Binomial*, 1955). Accordingly, we reject the null hypothesis in favor of the death-dip hypothesis.

The Statistical Significance of the Death-peak on and After the Birthmonth (Sample 1).

Any test of the statistical significance of the death-peak after the birthmonth must not be affected by our prior finding of a statistically significant death-dip before the

birthmonth. The following procedure has been designed to ensure that the statistical significance of the death-peak after the birthmonth is independent of the statistical significance of the death-dip before it.

In evaluating the significance of the death-peak, we will use some of the definitions provided on page 8:

- period a = the month before the birth-month
- period b = the four month period consisting of the birthmonth and the three months thereafter
- period c = the eleven month period consisting of all months *except* the month before the birth-month

We will frame our death-peak hypothesis in terms of the relationship between periods b and c. Thus phrased, it can be tested independently of the death-dip hypothesis.

Under the null hypothesis of independence between the birth and death months, 4/11 of the deaths in period c should occur in period b (period b/period c = 4/11). We observe 140 deaths in period b, and 332 deaths in period c. We will reject H_0 if the probability under H_0 of 140 or more deaths out of 332 is less than .05. This probability is approximately .01, so that the null hypothesis is rejected in favor of the alternative hypothesis that there is a peak in deaths on the birthmonth and in the three months thereafter.

In sum, we have found a death-dip before the birthmonth, and a death-peak thereafter; and each of these findings is significant at the .05 level. Our testing procedures were designed to ensure that the statistical significance of the death-dip is independent of the statistical significance of the death-peak. Consequently, a significant death-dip *and* death-peak would occur in the same sample less than twenty-five times in ten thousand (.05 x .05), under the null hypothesis of independence between the birth and death-month.

THE INTERNAL STRATIFICATION OF THE WORKING CLASS: SYSTEM INVOLVEMENTS OF AUTO WORKERS IN FOUR COUNTRIES *

WILLIAM H. FORM

University of Illinois, Urbana-Champaign

American Sociological Review 1973, Vol. 38 (December):697-711

The relevance of structural differentiation for social stratification has rarely been studied empirically. This research compares automobile workers of different skill levels for their involvement in the family, work group, union, neighborhood, community, and nation in four countries varying in industrial development. In the more industrialized countries, compared to other employees, skilled workers participate more in work-related social systems and in systems which extend beyond family and neighborhood. The increasing differences in participation within the working class which accompany industrialization and the increasing social power of the skilled workers in the community partly result from their greater ability to link other systems to their solidary work groups. Industrialization and structural differentiation of society may not homogenize industrial workers but rather create internal cleavages which increase the social power of skilled workers and decrease the possibility of a unified or radical working class movement.

The Problem

SOME commentators on the present scene feel that social bonds among factory workers are weak; even though they inhabit the city, workers, like peasants, live in an organizational desert, clinging to their families (cf. Babchuk and Edwards, 1965: 150-2). Although some studies show that the work life and the non-work life of industrial employees are highly segmented and socially impoverished (Dubin, 1956; Goldthorpe, et al., 1968), other evidence supports an opposite conclusion: that industrialism forges solidary work groups, rapidly pulling workers into social systems beyond the family (Meissner, 1971; Lambert, 1963; Form, 1972).

The resolution of this issue is important for political analysis. If social life inside and outside the factory is poorly developed and segmented, a successful working class movement is unlikely. However, if workers are

able to forge a solidary work organization and link it to others in the community, a successful social movement is possible. The inability of factory workers in advanced industrial societies to bring about radical change suggests that they lack unity and consensus on political goals. This research attempts to explain an apparent anomaly: that, although industrial workers do forge a plant community and become involved in many social systems outside the factory, they fail to become an independent and cohesive political force in the society. My position is that industrial workers are not a homogeneous mass, but a stratified body with strong cleavages between the skilled and less skilled workers. The more industrialized the society, the more distinctive skill strata become in their plant and community involvements. The structural differentiation attending industrialization (Smelser, 1963:105-16) is accompanied by a pattern wherein skilled workers become increasingly active in various social systems of the society, more differentiated from the unskilled in their behavior, and more independent in their politics.

Evidence to demonstrate this position requires data on the entire range of social system involvements of workers for each skill level. This study obtained data on worker involvement in the family, work group, union, neighborhood, community, and nation. To ascertain whether industrializa-

* I am grateful to Joan Huber for many helpful suggestions and to Gary Abbott for help in preparing the data for analysis. I am also indebted to the field directors of this study: Paolo Ammassari for Italy; Richard Gale for Argentina; Baldev R. Sharma for India; and Steven E. Deutsch for the United States. The following agencies supported the research: The Social Science Research Council, the Fulbright Commission, the National Science Foundation, and the Institute of Labor and Industrial Relations at the University of Illinois, Urbana-Champaign.

tion affects patterns of involvement, factories were selected for study in four countries which varied in extent of industrialization: India, Argentina, Italy and the United States.

It would be pointless to examine the relationship of skill and level of industrialization to the system involvements of workers if those involvements were very small. Elsewhere (Form 1972; 1973a; and 1973b) I reported that workers in all four countries had developed a rich texture of social relations inside and outside the factory. They forged strong personal ties in the plants as well as strong attachments with others inside and outside their neighborhoods and in various community organizations. Life outside the factory was not completely absorbed by the family. Workers had interest in and knew the main problems of the union, neighborhood, community, and nation. Since similar patterns were found in all nations, it appears that workers adapt to industrialism rapidly (cf. Inkeles, 1969; Lambert, 1963; Reynolds and Gregory, 1965). However, in the more industrialized countries, those most active in the work group and union were also the most active in systems outside the factory. This evidence suggests that the system integration of workers may proceed from the economy to the broader society (Moore, 1965:42-86).

Given the relatively high levels of worker involvement in systems inside and outside the factory, the question is whether the patterns of involvement vary by skill level and, if they do, whether they are relevant for working class politics. Sociologists have demonstrated that social interaction opportunities in the plant are quite diversified. Sayles (1963), Blauner (1964), and Meissner (1969) have shown that the amount of technological constraints on workers determines their discretionary time on the job and this, in turn, affects the amount of their contact with fellow workers. But scholars disagree whether extraplant activities are related to inplant interaction. Wilensky (1962: 2-4) summarizes three main positions on the issue. The spillover hypothesis states that those who interact most in the work group are most socially active outside the plant; conversely, those most isolated at work withdraw the most from the social life

of the community. The compensation hypothesis states that those most isolated at work become most active socially outside the plant and vice versa. The no-relation hypothesis holds that plant and outplant activities are independent of each other.

Each hypothesis assumes a different pattern of institutional linkage or a different view of human nature. The spillover hypothesis assumes that the work place dominates other institutions; activity patterns at work carry over into the community. Thus Meissner (1971:243) suggests that social skills practiced on the job are useful in non-work situations. Since isolated workers cannot learn social skills, they withdraw from community activities. The compensation hypothesis assumes that humans need a minimum amount of interaction; what they do not get at work, they seek in the community. The no-relation hypothesis assumes complete institutional segmentation as well as segmented role structures.

The compensation hypothesis is psychological, and it has not been tested; whether isolated workers who participate in the community are less frustrated than those who do not remains a speculative question. Sociologists have generally preferred the spillover to the no-relation hypothesis on the assumption that economic institutions initiate change in industrial societies. Since Durkheim (1964), sociologists have thought that institutional articulation is tenuous in newly industrializing societies and that it increases with economic development. This study takes the position that the spillover hypothesis best describes what happens to skilled workers and the no-relation hypothesis, what happens to the less skilled. Thus, in all countries, the skilled are expected to be the most involved in both work and non-work social systems and this pattern should be most apparent in the most industrialized societies. Moreover, the participational advantages which the skilled have over the less skilled enables the skilled to exert greater influence in the systems in which workers participate; and this advantage should increase with industrialization. Why should this be so?

An industrial society is a consumption society and industrial workers, like others, seek to maximize their consumption. Social par-

ticipation and engaging in "creative" leisure cost money. In a monetized society, social participation is both a consumption activity and a resource that can be mobilized for organizational control. Among industrial workers, the skilled earn the highest wages and are in the best position to increase their consumption, participation, and social power. They do this by linking social systems. Thus, the skilled have the greatest opportunity to become a cohesive group at work because they have greatest control over their work. The more advanced the technology, the more discretionary time skilled workers have on the job (cf. Davis and Goodman, 1970) because they are used as specialists in tool-making and not directly on production, as is often the case at early stages of industrialization.

The strategic use of time enables the skilled to build a solidary informal social system which, in turn, becomes the political base for union control (Spinrad, 1960). Unskilled workers have little opportunity to learn how to use social solidarity for purposes of organizational control. Moreover, the skilled learn that the union increases its bargaining effectiveness by linking to similar organizations in the community and nation. Consequently, the widening set of system involvements from the factory to the larger community becomes for the skilled both a style of consumption and a pattern of increasing social power. This pattern solidifies with industrialization. As industrial societies become more integrated and more organizationally dense, skilled workers become more involved in both formal and informal systems inside and outside the factory. To what extent do the data on social system participation in the four nations conform to this theoretical model? Before this question can be addressed, the research sites of the study need to be described.

Research Sites

A single industry was selected for study to minimize variations in the technology and organization: the automobile industry is an advanced model of manufacturing, with a fairly standardized technology and organization. Representative samples of workers were selected from manufacturing departments

of the plants.¹ In general, unskilled workers were engaged in assembly operations; semi-skilled, in semiautomatic machine operations and in test, inspection and repair; and skilled workers, in operations to build, repair, and maintain machinery. Oldsmobile (OLDS) in Lansing represented the most technologically sophisticated plant in the most industrialized country. FIAT in Turin, Italy, represented a modern plant in a country with an old but rapidly growing industrial base. Industrias Kaiser Argentina (IKA) in Córdoba had a less modern plant than FIAT, in a country beginning mass production. Premier Automobiles Limited (PAL) in Bombay, represented a rather primitive technology in an essentially non-industrial country.

FINDINGS

*Privileged Social Origins to Present Elite Status*²

Does the social background of the skilled give them a participational advantage over other workers? Skilled workers were recruited from relatively more privileged backgrounds than other employees; in all four countries, a larger percentage of the skilled had grandfathers in non-farming occupations and fathers who were themselves urban-born skilled workers (see Table 1). Moreover, more of the fathers of the skilled provided their sons with superior educational and vocational training opportunities, and their sons experienced more upward occupational mobility than other factory employees. Somewhat older than their workmates, the skilled had longer job tenure, had experienced less unemployment, and were more satisfied with their jobs. Wages of skilled workers averaged 50 percent more than those of the unskilled in all companies. Finally, the skilled had a greater opportunity to participate in urban social organizations because, compared to the less skilled, more of them lived in the central city. All the ad-

¹ The description of the plants, samples, and the communities are found in Form, 1972.

² All differences among skill levels reported in this paper satisfy the chi-square test at or below the 5 percent level. Probabilities from 5 to 10 percent level are reported as "tendencies."

Table 1. Social Origins and Present Status of Workers, by Skill Level (in percent)

Item	OLDS			FIAT			IKA			PAL		
	Un	Ss	Sk	Un	Ss	Sk	Un	Ss	Sk	Un	Ss	Sk
Grandfathers-- farmer	70	70	61	76	63	51	63	65	60	72	47	57
Fathers-- urban born	18	23	21	29	31	36	27	18	23	5	16	7
Fathers-- skilled ^b	12	20	28	15	21	31	37	42	52	36	53	49
Education-- high	61	61	69	7	14	31	10	32	53	26	49	54
Vocational training	3	2	35	28	24	50	21	12	30	6	6	5
Upward occup. mobility	27	58	75	38	42	80	32	49	60	33	64	79
Plant senior- ity--high	38	83	81	21	34	61	38	50	67	34	27	52
Residence-- central city	41	38	63	68	72	78	80	79	91	97	84	88
Present occup. liked most	51	55	75	31	38	40	61	58	64	48	54	79
Age--median years	28	33	39	32	33	36	27	29	32	33	30	35
N	(69)	(133)	(47)	(94)	(156)	(56)	(133)	(97)	(45)	(95)	(102)	(65)

^aUn=unskilled, Ss=semiskilled, and Sk=skilled

^bFor IKA, fathers were factory workers; for PAL, fathers were white collar workers or skilled.

vantages which the skilled had over other employees increased with advancing industrialization.³

Workgroup Interaction

The question now is whether the skilled become more socially integrated in the factory compared to the less skilled and hence are in a better position to control the union. Although all the data cannot be presented here, measurement of the amount and the quality of worker interaction in the factory showed that: the higher their skill, the more workers were required to communicate with others in order to perform their jobs (Form, 1972), the more they moved about freely

at their jobs, the more they made contact with others, the more they actually talked to others during working hours, the higher was the intimacy of their interaction, the more confidants they had in their work group, and the more satisfied they were with contacts with their fellow workers (see Table 2). In short, the evidence supported the conclusion that skilled workers had forged a more solidary social system at work than the less skilled. As expected, these trends were stronger in the plants with the most complex technologies (OLDS and FIAT.)

Union Involvement

The greater work group solidarity of the skilled seemed to carry over into the union (cf. Sayles and Strauss, 1953).⁴ The skilled

³ These trends were least consistent in PAL because young well educated higher caste workers had been recently hired as semi-skilled workers. Many older employees with long tenure and little education were skilled workers or had their wages.

⁴ Blumberg's (1968:217) review of the literature on union participation in various countries shows that the skilled held relatively more of the local offices including representation on workers' councils.

Table 2. Coefficient of Contingency (corrected) between Skill Level and Indicators of Social Interaction at Work

Items	OLDS	FIAT	IKA	PAL
Must talk to five or more workers	.344	.306	N.A.	.370
Can move about freely at work	.579	.489	.564	N.S. ^c
Twenty-five or more persons in workspace	.408	.546	.209 ^a	T ^b
Talks to eleven or more people	.313	.562	.222 ^a	.222 ^a
Has confidant among coworkers	.270	.163 ^a	.228	T ^b
Two or more good friends among coworkers	T ^b	.177 ^a	.195 ^a	N.S. ^c
Satisfied with contacts with coworkers	.228	N.S.	.077 ^a	N.S. ^c
Index of quality of interaction (high)	.441	.292 ^a	.374	T ^b

^aProbability of chi-square is between .05 and .10.

^bTrend, but probability of chi-square above .10.

^cNot significant by chi-square test at the .10 level and no trend.

had longer tenure than the less skilled, had been union members longer, attended union meetings more often, were more acquainted with the noneconomic activities of the union, knew more of the names of local union officials, showed more interest in union affairs, and more often discussed political and union issues with their fellow workers (see Table 3). Moreover, the dominant posture which workers wanted their unions to take toward management varied. In the less industrialized countries (India and Argentina), workers wanted their unions to be militant; in Italy and the United States, they preferred unions to bargain and cooperate with management. Differences among skill levels in union posture preferred increased with industrialization.

An attempt was made to measure the amount of political activity and the type of political ideology of the workers (see Form, 1973a). In OLDS, the skilled workers were much more active politically than the less skilled; and they were also much more conservative in their political ideology. In FIAT, four unions, ranging in ideology from business to Communist domination, competed for the workers' loyalty; but only 15 percent of the employees were members and even fewer attended union meetings (cf. Neufeld, 1954). The two unions with the most power were supported by conservatives. In IKA, the skilled were the most highly

involved in union affairs and the most radical politically. However, their radicalism was directed not at changing the economic system of the country, but at winning a place for labor in the political economy (cf. Fillol, 1961:90-2; Germani, 1966:388-91). In India, recently employed and highly educated semi-skilled workers were the most radical but least active in union and political affairs. In no country did workers want the unions to focus primarily on political objectives. In short, skilled workers were more highly involved with unionism than other employees. This involvement was higher and more conservative in the more industrialized countries.

Contacts with Workmates Outside the Plant

The data on plant and union interaction suggest that the skilled workers formed a more cohesive occupational community than the less skilled workers. Did this cohesion carry over in the outside community as Durkheim (1964) and Simmel (1955:185-9) predicted? Evidence in Table 4 shows that relatively more of the skilled workers had extra-work contacts with friends both from their work group and other departments. Moreover, more of the skilled saw their work mates in meetings of the voluntary organizations (not including the union) to which they belonged. In FIAT and IKA, more of

Table 3. Indicators of Union Involvement by Skill Level (in percent)

Skill Level	OLDS	FIAT	IKA	PAL
Attends Meetings Half or More Times				
Unskilled	43	11	37	36
Semiskilled	54	9	31	46
Skilled	73	2	44	43
N	(57)	(9) ^a	(36) ^a	(42) ^a
Identifies Names of Union Officers				
Unskilled	41	38	42	65
Semiskilled	48	39	41	57
Skilled	66	28	50	79
N	(51)	(37) ^a	(44) ^a	(66)
Opinion of Union is Favorable				
Unskilled	79	74	73	78
Semiskilled	78	84	62	80
Skilled	95	84	73	72
N	(83)	(81) ^a	(69) ^b	(77) ^b
Discusses Political and Economic Issues with Workmates				
Unskilled	45	17	40	41
Semiskilled	66	27	35	37
Skilled	60	37	67	29
N	(60)	(26)	(46)	(36) ^a
Dominant Opinion on Preferred Union Posture				
	Bar- gain	Coop- erate	Mili- tant	Mili- tant
Unskilled	38	41	45	52
Semiskilled	26	52	35	54
Skilled	43	59	34	53
N	(33)	(50) ^a	(39) ^a	(53) ^a

^aNot significant by chi-square test at the .10 level.

^bChi-square probability between .05-.10.

the skilled spent some of their vacation with their fellow workers, while in OLDS and PAL more of the skilled exchanged visits with their neighbors who were fellow workers. A five-question index which summarized different types of contacts outside the plant⁵ (see Table 4), showed that more of the skilled workers had outside contacts with

⁵ Questions (simplified) were: Do you travel to work alone or with fellow workers? Do you visit friends in your neighborhood who are also your fellow workers? With whom do you get into discussions of economic and political problems most often (mentions fellow workers)? Apart from relatives, did you spend any time with friends during your vacation (mentions fellow workers)? Excluding the union, are your fellow workers members of organizations to which you belong?

Table 4. Five Indicators of Interaction with Workmates Outside the Factory, by Skill Level (in percent)^a

Skill Level	OLDS	FIAT	IKA	PAL
Meets Friends in Work Group Outside the Factory				
Unskilled	72	52	77	59
Semiskilled	69	53	62	71
Skilled	84	47	71	75
N	(74)	(52) ^c	(71)	(68) ^b
Meets Fellow Workers Not in Work Group Outside the Factory				
Unskilled	62	51	32	48
Semiskilled	58	57	43	50
Skilled	59	70	41	58
N	(59) ^c	(70) ^b	(37) ^c	(52) ^b
Meets Fellow Worker in Community Organizations				
Unskilled	14	18	15	10
Semiskilled	25	26	19	12
Skilled	57	29	20	17
N	(31)	(24) ^b	(17) ^b	(13)
Index of Outplant Interactions (0-5) High (3-5)				
Unskilled	14	29	17	17
Semiskilled	24	37	21	21
Skilled	27	48	14	14
N	(23) ^b	(36)	(18) ^c	(18) ^c
Quality of Interaction (0-6) High (3-6)				
Unskilled	33	40	51	65
Semiskilled	63	48	43	65
Skilled	68	56	61	72
N	(59)	(52) ^b	(50) ^b	(68) ^c

^aUnless otherwise noted the probability of the chi-squares are below the .05 level.

^bProbability of χ^2 between .05-.10.

^cProbability of the chi-square above .10.

their work mates than the less skilled (cf. Wilensky, 1961:443). Finally, a six-item index dealing with quality of contacts with fellow workers⁶ showed that more of the

⁶ Questions (simplified) were: How do you feel about your daily contacts with your fellow workers? Is there among your fellow workers a person to whom you can talk about your most important personal problems? How many good friends are there in your workgroup (three or more)? Do you visit friends in your neighborhood who are also your fellow workers? Whose view do you attach most value in discussions of political and economic problems (mentions fellow workers)? With whom do you get into discussions of economic and political problems (mentions fellow workers)?

skilled had intimate contacts (see Table 4). The trend for skilled workers to have more outplant contact with co-workers increased with level of industrialization of the country.⁷

Free Time and Family Activities

The influence of a stratum increased with the number of its ties to other groups in the community. Widened contacts should give a stratum a better view of how the community functions and how community concerns can be linked to work, union, and other arenas. Many have observed that unskilled workers limit their social contacts to family and kin (Knupfer, 1947; Berger, 1960:54-79; Hausknecht, 1964:209) and consequently have a narrow view of the political process and their ability to influence it (Lipset, 1960:115-20; 357-77). I examined the range of free-time activities in which workers participated during the week, the weekend, and vacation periods and the people with whom they interacted. The pattern of activities during the week was similar for all skill levels, except that more of the non-skilled tended to remain at home and rest, while more of the skilled reported that they read books and magazines. Workers at all skill levels interacted with their work mates and other people with equal frequency.

However, during the weekend (especially in the United States and Italy) more of the unskilled workers, especially the assemblers, spent their free time watching sports events and movies and visiting their friends and families. Relatively more of the assemblers in the IKA and PAL who were single or married and residing apart from their families, spent their weekends alone. In all plants, more of the skilled engaged in hobbies and participative sports; but in Argentina and India more of them held a second job and did house repairs.

Even greater differences in activity patterns were apparent for the vacation period. In

all countries comparatively more of the unskilled and especially married assembly-line workers visited their relatives. Although more of the unskilled stayed in town during vacations, when they left town, more of them visited their communities of origin. When the skilled travelled out of town, more of them visited other cities (see Table 5). Undoubtedly the skilled could better afford out-of-town travel, but amongst workers who stayed in town, more of the unskilled reported doing odd jobs around the house with resting; while more of the skilled workers engaged in hobbies, reading, and participative sports (see Table 6).

In sum, an analysis of the pattern of free time activities points to emerging differences in the life styles of workers at different skill levels; the higher their skill, the less passive their activities and the less they took place in local groups. An index of family orientation with items on preference for family members as confidants, the presence of kin in the neighborhood, and visiting kin as the major vacation activity, showed that skilled workers in OLDS and FIAT were less involved with their kin than the less skilled; but no differences among skill levels were found in IKA and PAL. Thus again, the skill levels were more differentiated in the most industrialized countries.

Neighborhood Involvement

Compared to the unskilled, the skilled workers are expected to be less involved with neighbors and local activities because they

Table 5. Percentage of Workers Who Spent at Least Part of Their Vacation Out of Town,^a by Skill Level

Skill Level	OLDS	FIAT	IKA ^b
Unskilled	65	24	40
Semiskilled	68	33	43
Skilled	83	52	49
N	(71)	(34)	(43)

^aDoes not include those workers who went to their home towns to visit relatives.

^bProbability of chi-square above the .05 level.

⁷ Because single workers allegedly are freer of family responsibilities than married workers, an analysis was made of outplant interaction according to skill and marital status. No differences were found for OLDS; but in FIAT and IKA, more of the married skilled workers had higher outplant interaction, and in PAL more of the single skilled workers had higher interactions.

Table 6. Activities Which Took Place at Home and Outside the Home During Vacation,^a by Skill Level (in percent)

Skill Level	OLDS		FIAT		IKA		PAL ^b		
	Home	Other	Home	Other	Home	Other	Visit	Odd Jobs	Hobbies Reading
Unskilled	44	48	45	35	62	37	43	28	11
Semiskilled	30	70	34	45	74	26	26	15	24
Skilled	14	86	25	54	51	49	30	34	16
N	29	72	36	43	64	36	33	24	17

^aHome activities include visiting relatives, doing odd jobs around the house, and resting. Others include touring and engaging in hobbies.

^bSince PAL did not have formal vacations, weekend activities are used.

are more occupied with events in the broader community. Table 7 shows that even though the skilled had resided in their neighborhoods longer than the less skilled, all strata were equally ready to change neighborhoods. Moreover, despite their longer local residence, the skilled had fewer friends living in their neighborhoods than other workers. Except for India, fewer of the skilled reported that most of the friends they visited also resided in their neighborhoods. The more cosmopolitan orientation of the skilled was also reflected in the fact that fewer had friends who came from their region of origin or their community of socialization. Only in Italy did more of the skilled report that some of their friends came from their home towns. In Argentina and India, where formal neighborhood associations were common, fewer of the skilled reported participating in them. Finally, more of the skilled shopped and patronized bars or teahouses located outside their neighborhoods. Although differences in items reflecting neighborhood involvement among the skill levels were not strong anywhere, they were in a consistent direction (see Table 7). In conclusion, although the skilled resided in their neighborhoods longer than the less skilled, the skilled had fewer personal, family, and local commercial ties; the more industrialized the country, the stronger the trend.

Community Involvement

Since skilled workers are less involved with their kin and neighbors, they may become more active in community and national systems, in conformance with the principle that people in higher socioeconomic strata tend to be more involved in larger social systems (Milbrath, 1965:101-41; Merton, 1957: 402-6).⁸ Being locally born might increase community involvement, but only in Italy were more of the skilled than unskilled born in the communities in which they were residing. Moreover, most workers everywhere, irrespective of skill, were highly satisfied with their communities.

Every indicator of community involvement revealed that the skilled were the most involved. In all countries, but especially in Italy and India, more of the skilled subscribed to local newspapers. In all countries the skilled were the most interested in news and events which were not exclusively local, while the less skilled reported more interest in purely local news (see Table 8). Manual workers do not normally join many formal organizations (Hyman and Wright, 1971) and this was the case in all four countries.

⁸ Hamilton (1967) feels that despite this, the skilled are more similar to the unskilled than they are to white collar workers.

Table 7. Position of Skilled Workers Compared to Less Skilled Workers on Items Relating to Neighborhood Involvement^a

Items	OLDS	FIAT	IKA	PAL
Years in neighborhood	more*	more	same	more
Prefers to move residence	same	same	more	same
Friends scattered in city	more*	more	more*	same
Friends from region of origin	fewer*	fewer*	same	fewer
Friends from hometown	same	more	same	fewer*
Percent of friends residing in neighborhood	smaller	smaller*	smaller*	same
Exchanges visits with Workmates in neighborhood	same	same	less	more
Participates in neighborhood organization	--	--	less*	less*
Shops outside neighborhood	more	same	more	same
Patronizes bar or teahouse outside neighborhood	more	more*	more	more

^aDifferences between skill levels below the five percent level by the chi-square test is indicated by an asterisk. No asterisks means that the chi-square probability was between .05 and .10. Probabilities above ten percent were interpreted as evidence of no differences between skill levels.

Table 8. Indicators of Local Community Involvement According to Skill Level (in percent)^a

Item	OLDS				FIAT				IKA				PAL			
	Un	Ss	Sk	N	Un	Ss	Sk	N	Un	Ss	Sk	N	Un	Ss	Sk	N
Interest in local news	50	43	35	(42)	43	41	37	(40)	26	19	17	(21)	20	26	18	(22)
Reads magazines	67	68	78	(70)	53	63	61	(60)	77	84	86	(81)	22	30	36	(29)
Organizational member	32	41	67	(46)	29	35	34	(33)	34	38	40	(37)	23	39	45	(34)
Workmate in organization	14	25	57	(31)	18	26	29	(24)	15	19	20	(17)	10	12	19	(13)
Religious organization member	26	52	36	(41)	30	16	6	(18)	--	--	--	--	69	21	10	(14)
Attends church weekly ^b	24	28	38	(30)	N.A.	N.A.	N.A.	N.A.	26	39	28	(31)	61	60	43	(56)
Community problems identified																
None and none known	38	21	12	(22)	39	29	21	(31)	13	9	4	(10)	24	26	29	(26)
Institutional	22	33	31	(30)	3	10	13	(9)	3	2	3	(3)	6	6	14	(8)
Moral-social	16	13	14	(14)	32	38	26	(34)	14	21	13	(16)	24	23	16	(22)

^aIn IKA, only 3 persons belonged to religious organizations, but the data for church attendance is for once a month or more frequently. For PAL, data are reported for reading magazines weekly.

^bN.A.=Not Available.

But everywhere, more of the skilled belonged to organizations than the less skilled. Yet in all nations, the less skilled participated more regularly in organizations to which they did belong. This unexpected finding reflected different patterns of religious activity. More of the unskilled or semi-skilled belonged to religious organizations (excluding the church); and, except for the United States, more of them attended religious services regularly. Meissner (1971:250) regards religious participation as an expressive activity, and his study shows that those who have less control over their work (e.g., unskilled) compensate by participating more heavily in religious organizations. Corroborative evidence was found in this study; more of the lower skilled workers participated in religious organizations, and they did so with family members. When they participated in non-religious organizations, compared to the skilled, more went with friends or relatives. On the other hand, more of the skilled reported participating in non-religious formal organizations and doing this with their fellow workers (see Table 8).

Finally, workers were asked to identify the most important problems facing their communities. In all nations except India, the higher their skill, the more workers could name specific problems. More important, the skilled tended to name problems which specific community agencies were designed to resolve (e.g., providing health care, educational facilities, welfare and recreational services); while the less skilled tended to identify vague problems which community agencies could not readily attack, such as the rebelliousness of youth and the break-

down of morality. For most of the indicators of community involvement, differences between skill levels were larger for the more than the less industrialized countries.

National Involvement

Three indicators of involvement in the nation were developed: ability to identify national issues which require organizational solutions, participation in discussions dealing with national economic and political problems, and interest in national news. Surprisingly, fewer workers in the more industrialized countries were able to name important national problems. Perhaps the problems facing the less developed nations were of such severity that even the least informed was aware of them. War with China and the threatening famine were on everyone's mind in India and in Argentina; everyone worried about runaway inflation and unemployment. However, within each nation, more of the skilled than the less skilled identified national problems; and this trend was stronger in the more industrialized societies (see Table 9).

Citizens need little knowledge of government to know that war and inflation call for government action, but they need more knowledge to identify specific problems which call for action by specific governmental agencies. Interviews were examined for references to problems requiring action by such specific governmental agencies as employment bureaus, medical organizations, educational offices, and economic development agencies. Data in Table 10 clearly show that relatively more of the skilled referred to specific governmental services, and this trend was stronger

Table 9. Interest in National News and Participation in Economic and Political Discussion, by Skill Level (in percent)

Skill Level	OLDS		FIAT		IKA		PAL	
	News	Discus- sion	News	Discus- sion ^a	News ^a	Discus- sion	News ^a	Discus- sion
Unskilled	50	46	57	36	74	37	80	66
Semiskilled	57	62	59	49	81	47	74	68
Skilled	65	67	63	48	83	52	82	80
N	(58)	(59)	(59)	(45)	(79)	(46)	(78)	(70)

^aProbability of the chi-square is above .05.

Table 10. Specification of National Problems and Organizational Means to Solve Problems, According to Skill Level (in percent)

Skill Level	OLDS		FIAT		IKA ^a		PAL ^a	
	Problems Named	Means Named	Problems Named	Means Named	Problems Named	Means Named	Problems Named	Means Named
Unskilled	65	9	61	18	88	7	96	10
Semiskilled	76	9	67	23	88	6	94	12
Skilled	94	17	85	42	97	15	99	19
N	(77)	(11)	(68)	(25)	(90)	(8)	(95)	(13)

^aThe probability of the chi-square is above .05.

in the more industrialized societies.

On the individual behavioral level, indicators of national involvement include voting, working for political parties, reading national news, discussing national issues, and attending meetings of organizations which have national concerns. Voting is not a valid cross-national indicator of national involvement because it was quasi-compulsory in Italy, voluntary in United States, and normative in India; in Argentina, elections were sometimes cancelled. But working for political parties directly or through the union is a valid indicator of national involvement. In no country did more than 3 percent of the workers engage in such activity, an insufficient number to analyze by skill level.

An index of political interest for the United States included the following items: interest in politics, voting in local, state, and national elections, contributing to a political campaign, and wearing a campaign button or car sticker. Skilled workers were found to be the most highly involved in politics ($\bar{C} = .38$). An index of political activity was also available for India with items relating to: interest in political news, voting in community, state, and national elections, membership in political organizations, and discussing politics with a party worker. Although the relationship between skill level and political activity was not statistically significant, more of the skilled than unskilled workers read political news and voted in the elections. However, nine long-tenured semi-skilled workers were the most active in political organizations.

Two remaining indicators of national involvement were reading newspapers and participating in discussion of political and eco-

nomic events. Data in Table 11 show that in all countries, the higher their skill the more workers were interested in both community and national news. This trend was much stronger in OLDS and FIAT than it was in IKA and PAL. Finally, more of the skilled, with the possible exception of FIAT, discussed political and economic events. As in the case of other social systems, so it was for national involvement; differences among skill levels were larger for the more rather than the less industrialized nations.

Anomie

By definition, where social integration is high, normlessness or anomie should be low. Thus, fewer of the people who are highly involved in related systems which emanate from the workplace and extend to the broader society should report feelings of normlessness. In this research, since the skilled most consistently exhibited such a

Table 11. High Normless Anomie According to Skill Level (in percent)

Skill Level	OLDS	FIAT ^a	IKA	PAL ^a
Unskilled	28	40	13	64
Semiskilled	28	42	9	53
Skilled	8	41	21	65
Total	23	41	14	60
N	(304)	(306)	(315)	(262)

^aProbability of the chi-square is above .05.

pattern, they should display the least anomie. Also, since more workers in the more rather than in the less industrialized countries were involved in related social systems, anomie should decrease with industrialization (cf. Durkheim, 1964:22-31; Moore, 1965:98-109).

A five-item Guttman scale of normlessness with a high coefficient of reproducibility (over 0.85) for each country⁹ showed that the highest anomie was found in PAL, followed by FIAT, OLDS, and IKA (see Table 11). Only IKA was out of order and its workers exhibited lowest anomie. Three factors may account for the low IKA scores: most of the workers were socialized in cities; compared to other workers in Córdoba, IKA employees received exceptionally good wages; IKA workers were quite young and well educated and they looked forward to a secure future in the expanding industrial sector.

It is more difficult to explain the relationship between skill level and anomie. Only in OLDS did the findings conform to expectations; that is, the skilled had lower anomie scores than the less skilled (see Table 11). In FIAT and PAL there was no relationship, but in IKA the relationship was reversed. The interpretative problem is not severe for FIAT because employees from all skill levels were newly exposed to industry and city life, so they responded similarly. The results in PAL are also understandable: the problems in Indian society were so immense that all workers, regardless of skill, saw them as beyond control; thus, their high anomie.

As indicated above, the Argentinians were urbanites and well-adapted to city ways. Only 20 percent of the skilled had high anomie scores compared to twice that percentage for FIAT and three times that for PAL. These highly anomic IKA em-

ployees were relatively young, highly educated, and upwardly mobile workers who wanted to rise even more. They were dissatisfied with their jobs and were planning to quit at the first opportunity. Significantly, they did not aspire to better industrial jobs; they wanted to become proprietors, managers, or officials. Yet the economic outlook in that sector was discouraging, and they felt blocked by forces beyond their control (cf. Germani, 1966).

THE RELEVANCE OF OTHER FACTORS

Factors other than the skill level of workers can influence their social system involvements. Compared to those reared on farms, urbanites should be expected to be more involved in the organizational life of the factory, union, community, and nation. Second, central city residents should have easier access to urban organizational life than those who live outside the city. Third, parents with school age children should be more involved with neighborhood and community activities than childless workers. Finally, those with superior education should be more involved in community and national affairs than the poorly educated because education gives people a wider view of the world. I expected these factors to be less important than skill in accounting for the social system involvements of workers.

These four variables were run against all indicators of social system involvement. In no country were age, marital status, and number of children associated with involvement in any social system. The only exception was that, compared to the married, single workers spent more of their free time outside the home during the week and weekend. In all countries, community of socialization was related only to neighborhood involvement; the rural-born were more highly involved. Community of residence was related to involvement in other systems, and it varied inversely with extent of industrialization. In India, residence had no effect, but in Argentina central city residents were more involved in community and national affairs. In Italy and the United States, residence was decisive for system involvement in the classic pattern: those living outside the city were more involved in the family and the local neighborhood, while central city

⁹ Items in this scale included: In the problems of daily life, it is always easy to know which is the right path to choose; nowadays, it is really difficult to rear children because what is right today is wrong tomorrow; nobody agrees on what is good or bad because everybody follows his own ideas; it is easy to get agreement on things that are morally correct; there are so many organizations with different ends, that one cannot trust any of them; the world today is changing so fast that it is difficult, in the problems of everyday life, to decide which is the right path to follow.

residents were more involved in community and national systems.¹⁰

Education influenced two areas in all four countries. The more highly educated spent more of their time during the week and weekends reading and they were more interested in national news than the less educated. Education was also associated with community and national involvements of OLDS and PAL workers, but when level of education was controlled, differences by skill persisted only for OLDS. In the entire gamut of social systems, skill level and occupation were found to be more consistently related to system involvement than any variable or combination of variables.

CONCLUSIONS

The basic hypotheses of this paper tend to be supported. The labor force of the automobile industry does not appear to be a homogeneous mass, but is socially stratified. Social system involvements for the skilled workers tend to conform to the spillover hypothesis, while the pattern for the less skilled seems to fit the no-relation hypothesis. Differences among skill levels increase with industrialization of the country.

The greater work group solidarity of the skilled seems to be the natural outgrowth of working in a less restrictive technological environment. In union politics, the skilled profitably put their greater solidarity to work. Moreover, they carry over their work group and union contacts into community affairs more than the less skilled. The skilled are less absorbed in kinship networks and in the social life of neighborhoods, but they participate more widely in community affairs. They are more interested and more knowledgeable about both community and national affairs. In short, compared to the unskilled, the skilled conform more closely to a social class or an occupational community, even more so in the most industrialized countries.

The social significance of the increasing

solidarity of the skilled and their increasing differentiation from the less skilled needs thorough exploration. Findings from this study support the observation that, if industrial workers are going to launch a political movement, their chances of success, insofar as it depends upon worker solidarity, are greater at the early stages of industrialization when the structural differentiation of the community and the nation are not highly developed and when skilled workers are not socialized to participate in external systems. When industry is new, the strongest bond among workers may not be their occupation, but their common identity as factory workers. The later elaboration of the occupational and organizational structure of the society attracts skilled workers much more than the less skilled.

Scholars have stressed that industrialization has some homogenizing effects (Kerr, et al., 1960); that wage differentials among the skill levels tend to decrease with time (Reder, 1968:403-14), that the working class becomes less distinct (see literature cited in Goldthorpe, et al., 1969:1-29), and that politically, industrial workers become part of the middle mass (Westley and Westley, 1971). But data from this study suggest that the internal stratification of the working class may be increasing. Whether this trend makes the labor movement politically conservative or diffuses labor's political punch cannot be determined from one study. However, sufficient material already demonstrates that many skilled workers feel that their problems differ from those of the less skilled (Chamberlain and Cullen, 1971: 424-6). The limited evidence from this study suggests that, with increasing industrialization, the skilled may become less excited about either a working class social movement or joining the middle status groups of the society. Organizationally, the style of life of the skilled is similar to strata above them even though the rhetoric of the skilled resonates with working class images. This paradox encourages non-ideological and pragmatic politics (cf. Hamilton, 1964:53-7; Michels, 1959:292-4). It may well be that in advanced industrial societies, skilled workers may become an autonomous special interest group in the union while joining conservatives in the national political scene.

¹⁰ This pattern was absent in India because more Bombay residents were unskilled workers and had below average education than non-central city residents. In the United States, the superior education of non-city residents did not increase their involvement in community and national affairs.

REFERENCES

- Babchuk, Nicholas and John N. Edwards
1965 "Voluntary associations and the integration hypothesis." *Sociological Inquiry* 35 (Spring):149-62.
- Berger, Bennett M.
1960 *Working-Class Suburb*. Berkeley: University of California Press.
- Blauner, Robert
1964 *Alienation and Freedom: The Factory Worker and His Industry*. Chicago: University of Chicago Press.
- Blumberg, Paul
1968 *Industrial Democracy: The Sociology of Participation*. London: Constable.
- Chamberlain, Neil W. and Donald E. Cullen
1971 *The Labor Sector*. New York: McGraw-Hill.
- Davis, Stanley M. and Louis Wolf Goodman, editors
1970 *Workers and Managers in Latin America*. Lexington, Mass.: D. C. Heath.
- Dubin, Robert
1956 "Industrial worker's worlds: a study of the 'central life interest' of industrial workers." *Social Problems* 3(January):131-42.
- Durkheim, Emile
1964 *The Division of Labor in Society*, Tr. by George Simpson. Glencoe, Illinois: The Free Press.
- Filloi, Thomas Roberto
1961 *Social Factors in Economic Development: The Argentine Case*. Cambridge, Mass.: MIT Press.
- Form, William H.
1972 "Technology and social behavior of workers in four countries: a sociotechnical perspective." *American Sociological Review* 37 (December):727-38.
- 1973a "Job unionism vs. political unionism in four countries: the relevance of industrial development, type of union, and skill level." *Industrial Relations* 12(May):224-38.
- 1973b "Automobile workers in four countries: the relevance of system participation for working class movements." Unpublished manuscript.
- Germani, Gino
1966 "Social and political consequences of mobility." Pp. 364-94 in Neil J. Smelser and Seymour Martin Lipset (eds.), *Social Structure and Mobility in Economic Development*. Chicago: Aldine.
- Goldthorpe, John H., David Lockwood, Frank Bechhofer and Jennifer Platt
1968 *The Affluent Worker: Industrial Attitudes and Behavior*. London: Cambridge University Press.
- 1969 *The Affluent Worker in the Class Structure*. London: Cambridge University Press.
- Hamilton, Richard F.
1964 "The behavior and values of skilled workers." Pp. 42-57 in Arthur B. Shostak and William Gomberg (eds.), *Blue-Collar World: Studies of the American Workers*. Englewood Cliffs, N. J.: Prentice-Hall.
- 1967 "Skill level and politics." *Public Opinion Quarterly* 31(Winter):390-99.
- Hausknecht, Murray
1964 "The blue-collar joiner." Pp. 207-15 in Arthur B. Shostak and William Gomberg (eds.), *Blue-Collar World: Studies of the American Worker*. Englewood Cliffs, N. J.: Prentice-Hall.
- Hyman, Herbert H. and Charles R. Wright
1971 "Trends in voluntary association membership of American adults: replication based on secondary analysis of national sample surveys." *American Sociological Review* 36 (April):1-31.
- Inkeles, Alex
1969 "Participant citizenship in six developing countries." *American Political Science Review* 63(December):1120-41.
- Kerr, Clark, John T. Dunlop, Frederick H. Harbison and Charles A. Myers
1960 *Industrialism and Industrial Man*. Cambridge, Mass.: Harvard University Press.
- Knupfer, Genevieve
1947 "Portrait of the under-dog." *Public Opinion Quarterly* 11(Spring):103-14.
- Lambert, Richard D.
1963 *Workers, Factories and Social Change in India*. Princeton: University Press.
- Lipset, Seymour Martin
1960 *Political Man: The Social Basis of Politics*. New York: Doubleday.
- Meissner, Martin
1969 *Technology and the Worker*. San Francisco: Chandler Publishing Co.
- 1971 "The long arm of the job: a study of work and leisure." *Industrial Relations* 10(October): 239-60.
- Merton, Robert K.
1957 *Social Theory and Social Structure*. Glencoe, Illinois: The Free Press.
- Michels, Robert
1959 *Political Parties*. Tr. by Eden and Cedar Paul. New York: Dover.
- Milbrath, Lester W.
1965 *Political Participation*. Chicago: Rand McNally.
- Moore, Wilbert E.
1965 *The Impact of Industry*. Englewood Cliffs, N.J.: Prentice-Hall, Inc.
- Neufeld, Maurice F.
1954 *Labor Unions and National Politics in Italian Industrial Plants*. Ithaca, N.Y.: Cornell University Press.
- Reder, Melvin W.
1968 *Wages: Structure*. Pp. 403-14 in David L. Sills (ed.), *International Encyclopedia of the Social Sciences*. New York: The Macmillan Co. and the Free Press.
- Reynolds, Lloyd G. and Peter Gregory
1965 *Wages, Productivity and Industrialization in Puerto Rico*. Homewood, Ill.: R. D. Irwin.
- Sayles, Leonard R.
1963 *Behavior of Industrial Work Groups: Prediction and Control*. New York: Wiley.

- Sayles, Leonard R. and George Strauss
1953 *The Local Union: Its Place in the Industrial Plant*. New York: Harper.
- Simmel, Georg
1955 *Conflict and the Web of Group-Affiliations*. Tr. by Kurt H. Wolff and Reinhard Bendix. Glencoe, Illinois: The Free Press.
- Smelser, Neil J.
1963 *The Sociology of Economic Life*. Englewood Cliffs, N. J.: Prentice-Hall.
- Spinrad, William
1960 "Correlates of trade union participation: a summary of the literature." *American Sociological Review* 25(April):237-44.
- Westley, William A. and Margaret W. Westley
1971 *The Emerging Worker*. Montreal: McGill-Queen University Press.
- Wilensky, Harold L.
1962 "Labor and leisure: intellectual traditions." *Industrial Relations* 1(February):1-12.
1961 "Orderly careers and social participation: the impact of work history on the social integration in the middle mass." *American Sociological Review* 26(August):521-39.

**MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES**

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

SOCIAL NETWORKS AND VOTING: THE RESURRECTION OF A RESEARCH AGENDA *

CARL A. SHEINGOLD

Department of Sociology, Cornell University

American Sociological Review 1973, Vol. 38 (December):712-20

This article focuses on the failure of researchers to follow up on the classical voting studies done during the 1940's at Columbia University. A major consequence was the lack of research on the social structure of the information flow generated during political campaigns. From an historical perspective, this has left a vacuum in our knowledge of voting. We also attempt to define a distinctly sociological dimension of voting.

THE classical studies of decision making in the 1940 and 1948 presidential elections, *The People's Choice and Voting* (Lazarsfeld et al., 1948; Berelson et al., 1954), radically altered our conception of the dynamics of voting behavior. If classics are to be measured by the research they generate, however, these products of Columbia University's Bureau of Applied Social Research have an ambiguous status.

Studies of the 1950 congressional elections (McPhee and Glaser, 1962) concluded the Columbia voting research. This dramatic halt was not the result of analytical closure. As Rossi (1959) and others noted at the time,¹ the Columbia voting studies raised as many interesting questions as they answered. Some of the most important, unanswered questions related to the flow of political information and influence during a campaign.

Using self-report data on the receipt of information and influence, the Columbia researchers were able to explore this issue from the perspective of individual respondents. Without social network data, however, they were unable to explore the social structure of information flow.

Whether the Columbia voting research, had it continued, would have pursued this

issue is open to question.² The critical point is that subsequent voting research has not included social network data and the implicit Columbia research agenda has not been addressed.³

Recent voting research has been oriented toward a very different intellectual tradition associated with the University of Michigan's Survey Research Center (S.R.C.).⁴

² The desirability of collecting social network data was discussed by critics of the Columbia research (cf. Rossi, 1959). Such data were collected in subsequent studies done within the Columbia tradition (e.g., Coleman et al., 1966). Further, at the end of *Voting* the authors alluded to the possibility of expanding their focus, "... to make the conversation—the pair or group of interlocutors—the unit of analysis" (Berelson et al., 1954: 30). This point, however, was only one of a long list of future possibilities which the authors briefly discussed. There was little explicit treatment of social networks, per se, in the Columbia studies. Their analytical focus was consistently on individuals. They explored social contexts to specify inputs to individual decision making and not as independent foci.

³ We will limit discussion of voting research to the Columbia and Michigan studies. Given the financial resources required to mount sophisticated voting research, intellectual trends in the area have been set by such research institutes. The influence of the S.R.C. has been further enhanced by the easy availability of its research for secondary analysis through the Inter-University Consortium for Political Research. Obviously, our contention that the major questions raised by the Columbia studies remain unanswered applies to subsequent voting research generally.

⁴ Cf., Campbell et al., (1954); Campbell et al., (1960); Campbell et al., (1966).

*I am indebted to Seymour Martin Lipset, Marshall Meyer, Karen Sheingold and Harrison White for comments on earlier drafts of this paper.

¹ For a collection of essays on voting research, many of which were reactions to the Columbia studies, see Burdick and Brodbeck (1959).

The S.R.C. researchers trace their intellectual heritage to V. O. Key, Jr., the late and eminent political scientist, and a prominent critic of the Columbia voting research (cf., Key, 1959).⁵

The analytical focus of the two schools differs greatly. The Columbia studies, concerned with individual decision making, focused on sociological independent variables—socio-economic and demographic attributes, and, most distinctively, the flow of information and influence during the campaign. The primary emphasis of the Michigan studies has been attitudinal variables and the political content and implications of voting. They have focused increasingly on aggregate level issues; on elections and electorates rather than individual voters as units of analysis. The Columbia research broke little ground in this area.

THE COLUMBIA RESEARCH⁶

The core of the Columbia analysis was an explanation of two findings which, at the time, was quite surprising. 1) The virtual absence of flexibility in the behavior of American voters.⁷ 2) The surprising characteristics of the minority of voters who were flexible. As noted in *The People's Choice*:

The notion that people who switch parties during the campaign are mainly the reasoned, thoughtful, conscientious people who are convinced by the issues of the election is just plain wrong. Actually they are mainly just the opposite. (Lazarsfeld et al., 1948:69).

Both studies found that switchers were likely to be peripherally involved in politics and the campaign.

The Columbia explanation of voting stability focused on two factors. 1) The socio-

logical base of political predispositions: Early voting intentions were not simply first hunches, but the cumulative product of the historical experience of social groups. The most important historical reference point for voters in 1940 and 1948 was their experience in and the response of the parties to the Great Depression of 1929.

2) The reinforcing effect of information received during the campaign.⁸ This effect was, in turn, related to:

- (a) The political homogeneity of primary groups. Most people interacted during the campaign with others with shared social characteristics, shared attitudes and, thus, shared political predispositions (Lazarsfeld et al., 1948:137-49; Berelson et al., 1954:88-109).
- (b) Selective perception and projection. With respect to direct media exposure, most people paid primary attention to information which reinforced their original intentions (Lazarsfeld et al., 1948:80-2). Further, the voter's perception of the candidate's stand on an issue was affected by his own viewpoint. The result of such projection was, again, to reinforce early intentions (Berelson et al., 1954:220-2).
- (c) The two-step flow of influence. The exposure of many voters to the media was not direct, but was mediated by opinion leaders and group discussion (Lazarsfeld et al., 1948:151-2).⁹ This filtering process eliminated or modified information inconsistent with the political predispositions and prejudices of the group.
- (d) The role of organizations. Politically involved organizations devoted most of their effort to mobilizing the faithful rather than converting the uninitiated (Berelson et al., 1954:50-3, 171-7).

Elaborating these findings helped to explain why the "wrong" people were the most flexible. Since social and political involvement are correlated, those who were

⁵ For a more extensive, but similarly oriented critique of the Columbia research, see Daudt (1961).

⁶ We will not attempt a complete exposition of the Columbia research, but will deal with those findings and analyses which bear most directly on the social structural lacunae mentioned above.

⁷ The vast majority of respondents in both 1940 and 1948 voted in November for the candidate they had initially supported and for the party they had supported four years previously. The actual proportion of stable voters was 87% in the 1940 sample (Lazarsfeld et al., 1948:xi) and 75% in the 1948 sample (Berelson et al., 1954:22).

⁸ Throughout this discussion we will refer to "within campaign" communication. Obviously, the flow of political information is not limited to the formal campaign period. Nor, for that matter, is the campaign-related activity of politicians. Our purpose in using this phrase is to distinguish between decision making and communication oriented toward explicit partisan choices, in contrast to more diffuse political discourse.

⁹ The "two-step flow" model was also pursued in other research (cf., Katz and Lazarsfeld, 1955; Katz, 1957).

more involved in politics were also likely to be more intensively exposed to the reinforcing political cues which dominated primary group communication. Those who were more involved in politics also had the deepest partisan commitments and were thus more likely to activate processes such as selective perception and projection (Lazarsfeld et al., 1948:90-5; Berelson et al., 1954:223-6).

LIMITS OF THE COLUMBIA STUDIES

The major limitations of the Columbia research derived from dependence on self reports regarding the opinions and votes of their respondents' associates. The researchers recognized the high probability that some respondents projected their own views onto their associates (Berelson et al., 1954: 89, f. 1; see also Laumann, 1969); thus doubts were raised about the accuracy of certain key findings. Furthermore, these self report data did not and could not reveal the social structural context of decision making. This point is central to our whole argument and, thus, calls for elaboration.

The major conclusion of the Columbia research was that voting must be seen as a group process. What does this mean? The analytical focus of most of the Columbia work is on individual decision making. In this context, "group process" refers to their finding that a decisive element in decision making is the receipt of information and influence through interpersonal communication. The major concern here is with the content of information received. If one can be reassured about their accuracy, self-report data are perfectly adequate for exploring this issue.

An alternative focus is the social networks through which information and influence flow. This raises the question of how information gets to the individual, in contrast to what information finally arrives. Social networks constitute social structures which exist independent of the perceptions of discrete individuals. The information an individual receives may emanate from others with whom he is not in direct contact and of whom he may be unaware. Thus, network structures cannot be directly studied within the confines of self report

data. Direct network data are required.¹⁰ Some specific examples of this analytical distinction might be useful.

A recurring theme in the Columbia research was that social isolation provides a promising base for political flexibility. More recent work by Converse (1962), Pinard (1968) and others, however, suggests that isolation is a double-edged sword. It provides a base for both flexibility and inaction:

[uninvolved people] . . . show a high susceptibility to short term changes in partisan attitude *provided that any new information reaches them at all*. On the other hand, when the flow of information through the society is weak, these are the individuals who are most likely to experience no new information intake, and hence are individuals least likely to show changes in patterns of behavior if indeed they are constrained to behave at all (Converse, 1962:587).

In other words, social isolation can be a base for flexibility, rigidity or inaction depending on what and how much information the individual is exposed to.

What determines whether a piece of information will reach a given individual or, at the aggregate level, how widespread the diffusion of a piece of information will be? Here isolation is a function not so much of individuals, but of social networks.

Most research on isolation has had a psychological orientation deriving from Durkheim's theory of anomie. This perspective emphasizes subjective alienation from social norms rather than structural isolation from information flow.¹¹ Even when the latter has been the focus, isolation has been treated as an individual rather

¹⁰ Much research on social networks has been done in recent years. For relatively up-to-date research and bibliographies, see Mitchell (1969: 350-9), Bott (1971:331-43) and Wellman and Whitaker (1972).

¹¹ The definition of isolation in Seeman's influential paper on alienation conveys the above emphasis: "[isolation is the assignment] . . . of low reward values to goals or beliefs that are typically highly valued in a given society" (Seeman, 1959:89). Indeed, Seeman has objected to definitions of isolation related to friendship status because, ". . . it comes very close to being a statement of either social adjustment or differences in associational styles . . . and as such seems irrelevant to the root historical notion of alienation" (Seeman, 1959:59, f. 20).

than a network property, the operative meaning of the concept being paucity of social ties.

The two most obvious analytically distinct dimensions of isolation are network insulation and paucity of social ties. The latter relates to an individual's direct contacts with other individuals. The former relates to the extent to which the individual's network is linked, directly or indirectly, with other networks.

Network attributes may be more important than individual attributes in determining the likelihood of new information reaching an individual. If a person has few social ties, but is connected to an open social network (i.e., one which is linked to many other networks), a diffusion process would have to be widespread to reach him, but there would be no structural bar to eventual contact.¹² By contrast, an individual who has more direct ties, but whose network is structurally insulated, might not be reachable by a standard diffusion process.

These distinctions are vital at the aggregate level. Under what circumstances and by what processes can new social and political movements (or parties) reach potential supporters who are socially isolated and poorly exposed to the media? If the isolation in question is characterized by both paucity of social ties and membership in open social networks, the general support for and level of discussion about the movement in the population as a whole would be a critical parameter.

If, however, we are talking about individuals who are structurally insulated—members of closed networks or virtually no networks at all—the structural parameters of their mobilization would be quite different. A standard diffusion process would, presumably, have little impact here. Variability in the organizational effectiveness of the new movement—particularly in its ability to reach people at the local level di-

rectly—would be a more critical parameter.

To give a concrete example, Lipset (1950:167; 1960:122-3, 149-52) has reported evidence suggesting that the poor are least likely to be early joiners of new political parties, but often join in large numbers after the party has achieved a certain level of electoral success. Lipset has interpreted these findings to be a function of the "hopelessness" of the poor. "Where the party is small and weak it cannot hold out the promise of immediate changes in the situation of the most deprived" (Lipset, 1960: 120-3).

Irrespective of the correctness of Lipset's hypothesis, this phenomenon may also have a structural dimension. It may be that only after a new party has achieved a certain level of general support and organizational strength is its base adequate for diffusing information about the party to the politically and socially uninformed. Note this alternative hypothesis relates not just to attributes of individuals, but also to attributes of the social networks in which they are embedded.

To cite one other issue, the Columbia researchers devoted considerable attention to the apparently important political role of opinion leaders in primary groups. The major finding reported in *The People's Choice* was a high correlation between propensity to serve as an opinion leader and exposure to the media. From this the authors developed the influential "two step flow of influence" model (Lazarsfeld et al., 1948:151-2).

In *Voting* they uncovered another interesting finding. Opinion leaders, in addition to being heavily exposed to the media, were also highly prone to seek the advice of others (Berelson et al., 1954:110-12). This suggested to the authors the possibility that, "... unending circuits of leadership [run] through the community like a nerve system through the body" (Berelson et al., 1954: 112).

While these findings have great practical and theoretical significance, it must be pointed out that models such as "two step flow ..." or "... circuits of leadership ..." relate to attributes of social networks, not of individuals. Correlations of individual attributes can neither confirm the empirical

¹² Indeed, the diffusion process need not be widespread if the network is strategically located. Or, conversely, a non-isolated individual may not be exposed to new information if he is not strategically located. In other words, if our concern is with information flow, the key issue is not isolation, as we typically mean it, but patterns of disconnectedness in and between social networks.

reality of these models nor reveal their empirical shape. They can only provide suggestive metaphors.

THE MICHIGAN RESEARCH

The S.R.C. researchers, following Key, were and are interested in analytically politicizing the study of voting, that is in studying voting as an input into the political system. They are less concerned with the decision making *process* which precedes voting—what they label “the psychology and sociology of human choice”—than with the cumulative effect of such decisions on the partisan division of the electorate and how it reflects the response of voters to public policy (Campbell et al., 1960:4-6).

This general orientation and the Michigan researchers' primary interest in attitudinal independent variables are clearly connected. In studying political attitudes (and related variables such as knowledge and ideology), one uncovers the political content of voting. A lesser interest in variables intervening between attitudes and votes—voting intentions and within campaign communication—is also explicable. The Columbia work suggested that such variables have little independent effect on the vote. The power of attitudinal variables in predicting votes revealed in the early S.R.C. research (Campbell et al., 1960: 34-40) further confirmed this finding. In general, the classical voting studies all suggested that attributes the individual brings with him to the campaign effectively determine voting behavior.

There is a final point relevant to the issue of research continuity. The Columbia theoretical perspective dictated local sampling. Had the implicit research agenda we have been alluding to been pursued, local sampling would have been even more imperative. In contrast, a primary interest in aggregate characteristics of the electorate, particularly attitudes, calls for the kind of national random sample which the S.R.C. researchers have employed. In other words, for the S.R.C. researchers systematically to have followed up on the Columbia research would have required more than adding or deleting items on their questionnaire. A basic and, from the S.R.C. point of view,

undesirable change of research design would have been required.

The S.R.C. researchers have systematically studied the attitudinal bases of voting behavior. They have also, as we indicated earlier, increasingly addressed their attention to aggregate level issues.¹³ At the heart of this approach has been a typology of different kinds of presidential elections—an historical perspective on voting behavior (Campbell et al., 1960:274-9).

In *The American Voter* they enumerated three kinds: 1) “Maintaining elections” in which basic party loyalties remain intact and are acted on by most voters; 2) “Deviating elections” in which short term forces lead to across-the-board gains by a particular party without affecting basic party loyalties; 3) “Realigning elections” in which basic party loyalties are in transition for many voters. In *Elections and the Political Order* the authors recognize that “deviating elections” logically imply a fourth category, “reinstating elections,” in which voters return to their traditional party and, thus, the “normal” partisan division of the electorate is recreated (Campbell et al., 1966:78-95).

All the above fits into the more general cyclical pattern of American political history—long periods of stability of partisan affiliations separated by brief but intense periods of crisis and realignment. Key was a pioneer in developing this interpretative scheme (cf., Key, 1955).¹⁴

AN HISTORICAL PERSPECTIVE

Viewing voting from an historical perspective raises a central analytical question. If the political dynamics of elections vary, is it not likely that the dynamics of the decision making process lying behind voting also vary with differences in historical situations? This simple point raises profound doubts about the general validity of the Columbia and Michigan voting studies, all

¹³ This can be seen most concisely by comparing the titles of the early S.R.C. publications *The Voter Decides* (1954) and *The American Voter* (1960) with the title of their most recent work *Elections and the Political Order* (1966).

¹⁴ For a recent work in this research tradition, see Burnham (1970).

done during a stable phase of American political history. In so doing, it illumines the significance for analysis of the social structural vacuum in voting research.

Consider the image of the typical voter conveyed by the Columbia and early Michigan studies. He is an individual guided by strong, historically grounded images of the major parties, exposed to reinforcing cues during the campaign, but, more importantly, largely inattentive to the details of the campaign. He has made in Key's words, a "standing decision." The result is stable voting behavior.

Consider, however, such a voter during a period of partisan realignment, i.e., a period when individuals and social groups are in political movement. First, the average voter should be exposed to a politically more heterogeneous social environment. He should, independently, be more open and attentive to political information received during the campaign. In other words, the social structural and social psychological filtering processes uncovered in the Columbia research should play a less prominent role.¹⁵

These hypotheses have a number of implications. For instance, during such a period we would expect the average voter to be more aware of the issues of the campaign and, under certain circumstances, more ideological, than was the average respondent in the early Michigan studies. In other words, decision making would be focused more directly on the actual political content of the campaign and less on historically derived images of the parties. Data from the 1964 (Field and Anderson, 1969) and 1968 (Converse et al., 1969; Pomper, 1972) presidential elections support these hypotheses.

The central implication here is that during a period of realignment, information received during the campaign should have an important independent effect on the vote. Thus, the social structure governing the flow of information should play an im-

portant role in shaping the partisan outcome of individual decision making and, therefore, of the election as a whole. In short, the failure of students of voting to follow up on the Columbia research has created a vacuum of considerable analytical and practical significance.¹⁶

A SOCIOLOGICAL DIMENSION OF VOTING

A recurring theme in the voting literature—and, indeed, in political sociology generally—has been interdisciplinary conflict. The major criticism of the Columbia voting research was that it has "taken the politics out of the study of voting" (Key and Munger, 1959:281). This essay can be read as an attempt to put the sociology back into the study of voting and as a critique of the social psychological emphasis of most voting research.

Our essay, however, questions the framework of such interdisciplinary debate. For instance, it implies that criticisms of the Columbia research's alleged lack of political relevance put the cart before the horse. When voting is habitual action little informed by issues and ideologies—when voting is, in this sense, depoliticized—group dynamics are of little significance in determining votes. Under such circumstances students searching for the political content of voting may well want to study attitudes. The early Michigan studies revealed, however, precious little political content to be uncovered (cf., Campbell et al., 1960:97-108) in such stable periods.

¹⁶ Several political scientists have recently expressed renewed interest in environmental effects. For instance, Pomper has written, "Perhaps the major fault of the Michigan studies has been the comparative neglect of the political environment as an independent variable. The methodology of survey research has brought an overemphasis upon the individual behavior of isolated respondents" (1972:427). In a similar vein, Burnham advocates renewed work on community political norms (1968: 29-32). To the best of our knowledge, such suggestions have not been translated into research. Moreover, neither author has fully developed the sociological implications of his perceptions. To talk about community norms and political environment is not sufficient. One must talk also about the link between such contextual effects and voting, i.e., about the structure and process of the flow of political information.

¹⁵ We would expect the latter to be true in any circumstances where the individual's "standing decision" is either an inadequate or irrelevant guide to action (e.g., in primary or non-partisan elections).

In contrast, when voting itself is politicized—when voters are attentive to issues and involved in active political decision making—the dynamics of information flow become critically important analytically and politically. Indeed, the analytical importance of the Columbia variables relating to group dynamics increases with the political significance of the election under study.

Yet these points should not be read to imply that during a realigning period voting is, in some sense, sociologically determined and, thus, research on political attitudes will not be fruitful. Since it is precisely during such a period that the political content of attitudes which shape the vote should increase, these attitudes should provide a promising focus for research. Moreover, the causal significance of attitudinal variables will remain great. It is, in fact, hard to imagine any circumstances in which an analysis of individual votes, or the partisan outcome of elections, will not stress the interaction between attitudes and social (demographic and socio-economic) characteristics. This, as we read it, is what critics who accused the Columbia research of sociological determinism were saying.¹⁷ In other words, to stress group process when explaining partisan decisions is to distort the substantive meaning of voting.

What such critics missed was that the Columbia researchers—particularly in their analysis of group process—were not primarily interested in explaining the vote itself. Their dependent variable had shifted, albeit implicitly, from the partisan outcome of voting to the within campaign movement of voting intentions. And in the same sense that attributes of individuals are logically of primary importance in determining votes (as measures of general partisan orientation), so information received, or not received, by the individual would seem to be of critical importance in explaining this new dependent variable. This should be true irrespective of historical circumstances. That is to say, whether or not information received has the effect of changing or rein-

forcing voting intentions is of little consequence for the analytical importance of information flow variables in this new context.

Again, the issue is similar at the aggregate level. The political movement of social groups during a campaign—the success of parties in translating their starting potential (or “normal”) vote into election day strength—is a social process. An analysis of it which ignores the flow of information through social groups will have produced a very partial explanation.¹⁸

The implications of this discussion can be briefly summarized. The importance of—indeed, the substantive meaning of—independent variables changes with historical situations.¹⁹ Thus, professional specialization in the study of voting focusing on the relative importance of particular independent variables will lead, inevitably, to research distortions. The failure to follow up on the pioneering Columbia voting research is an illustration of this problem.

We contend that the most useful sociological contribution to voting research will come not from asking how important sociological variables are in determining voting, but from asking what is the distinctively sociological dependent variable? If sociology identifies and studies social structures, in contrast to attributes of individuals, then we would propose that the sociological dimension of voting is the dynamics of the movement of voting intentions and general partisan dispositions between and within campaigns.

¹⁸ In a sense, this variable redefines a problem in voting into a problem in the diffusion of innovations, the innovation being new political behavior. Important recent research in this area has treated the social structure of information flow as a distinct sociological dimension of a problem which, like voting, ultimately comes down to individual decision making (cf., Coleman et al., 1966). For a study done in this framework, of the Northern Wallace vote in the 1968 presidential election, see Sheingold (1971).

¹⁹ Burnham has put this point more generally: “It is very likely that generalizations about voting behavior at any level of analysis which are based on a limited span of observations in any dimension—across time, geographically, or by type of election situation—may prove to be partly or wholly invalid under circumstances other than those in which the generalizations were made” (1968:33).

¹⁷ These critics were also probably responding to the Columbia emphasis on the flow of political influence as compared to the flow of political information.

CONCLUSION

Social network data are both difficult and expensive to collect. It is, thus, imperative for researchers in this area to assess carefully, in advance, in what areas such research will be most fruitful. This essay represents, in part, an effort to clarify this issue with respect to a particular substantive area of research.²⁰ In so doing, we have advanced parallel arguments with respect to both individual and aggregate level issues. At a theoretical level, this parallelism does not require qualification. From a practical point of view, however, we should make an important distinction relating to level of analysis.

Contrast the following explanations of a hypothetical union member who persisted in his intention to vote for a candidate opposed by the union leadership: *Explanation A*: Mr. X voted for candidate Y because he was not informed about the anti-labor record of the candidate; *Explanation B*: Mr. X voted for candidate Y because he and other members of his social network were not politically involved or informed; *Explanation C*: Mr. X voted for candidate Y because his politically uninvolved network was not integrated with the politically active networks in his union.

As we move from "Explanation A" to "Explanation C" the necessity of collecting social network data increases. For "Explanation C" it is probably mandatory. The data on which "Explanation C" are based, however, are more relevant to answering aggregate level rather than individual level questions. With respect to explaining Mr. X's vote, these hypothetical data may be regarded as icing on the cake.

Explanation "C" is right to the point,

however, in answering a very different question: why was candidate Y able to win votes in this union despite the opposition of the union leadership? The answer might be that the union's network structure did not enable leaders to disperse information to the membership at large.

This hypothetical analysis implies that direct study of social networks of political communication may not be warranted at the individual level. It may suffice for students of voting to study information received using traditional survey instruments and self-report data.

But for aggregate level issues like the aggregate dynamics of partisan realignment, a significant contribution to voting research and sociology will result from pursuing the research agenda suggested in this paper.²¹ At this level distinctively social structural issues are involved.

Walter Dean Burnham recently suggested that the "... great many lacunae in our understanding of American politics ... appear to be particularly concentrated at the level of macro-analysis, where the concern is with the quantitative study of an aggregate system of behavior rather than with the behavior of individuals within the system" (Burnham, 1970, ix). Such lacunae may stem, in part, from limitations of sociology's contribution to the study of voting.

²¹ Professional specialization would seem to have two principle justifications. First, it can lead to exploring issues, or dimensions of issues, which would otherwise not be studied. Second, it can increase the probability that exploring often narrow substantive issues will, at the same time, help develop broader intellectual disciplines. Satisfying these criteria is particularly important when a professionally narrow approach also entails an expensive and difficult research design.

²⁰ In so doing, we have refrained from presenting an extensive review of the research literature on social networks. This is primarily because of space limitations. Furthermore, most network data have been collected in laboratory studies of small groups or field studies of specialized networks of adopters of innovations. Most of what we know about non-specialized networks in naturalistic settings is based upon inferences from individual data. Thus, the direct relevance of this research to the issues raised here is, at best, a complicated issue requiring extensive discussion.

REFERENCES

- Berelson, Bernard, Paul F. Lazarsfeld and William N. McPhee
1954 *Voting*. Chicago: The University of Chicago Press.

Bott, Elizabeth

1971 *Family and Social Network* (Second Edition). New York: The Free Press.

Burdick, Eugene and Arthur J. Brodbeck

1959 *American Voting Behavior*. Glencoe: The Free Press.

Burnham, Walter Dean

1968 "American voting behavior and the 1964 election." *Midwest Journal of Political Science* 12(February):1-40.

Burnham, Walter Dean

1970 *Critical Elections and the Mainsprings of American Politics*. New York: Norton.

Campbell, Angus, Gerald Gurin and Warren E. Miller

1954 *The Voter Decides*. Evanston: Row, Peterson and Co.

Campbell, Angus, Philip E. Converse, Warren E. Miller and Donald E. Stokes

1960 *The American Voter*. New York: Wiley.

1966 *Elections and the Political Order*. New York: Wiley.

Coleman, James S., Elihu Katz and Herbert Menzell

1966 *Medical Innovation: A Diffusion Study*. New York: Bobbs Merrill.

Converse, Philip E.

1962 "Information flow and the stability of partisan attitudes." *Public Opinion Quarterly* 26(Winter):578-99.

Converse, Philip E., Warren E. Miller, Jerrold G. Rusk and Arthur C. Wolfe

1969 "Continuity and change in American politics: parties and issues in the 1968 election." *American Political Science Review* 63(December):1083-1105.

Daudt, Henri

1961 *Floating Voters and the Floating Vote*. Leiden: Stenfort Kroese.

Field, John Osgood and Ronald E. Anderson

1969 "Ideology in the public's conceptualization of the 1964 election." *Public Opinion Quarterly* 33(Fall):355-69.

Katz, Elihu and Paul F. Lazarsfeld

1955 *Personal Influence*. Glencoe: The Free Press.

Katz Elihu

1957 "The two step flow of communication: an up-to-date report of an hypothesis." *Public Opinion Quarterly* 21(Spring):61-78.

Key, V. O., Jr.

1955 "A theory of critical elections." *Journal of Politics* 7(February):3-18.

Key, V. O., Jr. and Frank Munger

1959 "Social determinism and electoral decision: the case of Indiana." Pp. 281-99 in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior*. Glencoe: The Free Press.

Laumann, Edward O.

1969 "Friends of urban men: an assessment of accuracy in reporting their sociometric attributes." *Sociometry* 32(March):54-9.

Lazarsfeld, Paul F., Bernard Berelson and Hazel Gaudet

1948 *The People's Choice*. New York: Columbia University Press.

Lipset, Seymour Martin

1950 *Agrarian Socialism: The Cooperative Commonwealth Federation in Saskatchewan*. Berkeley: The University of California Press.

1960 *Political Man: The Social Bases of Politics*. Garden City: Doubleday.

McPhee, William N. and William A. Glaser (eds.)

1962 *Public Opinion and Congressional Elections*. New York: The Free Press.

Mitchell, Clyde J.

1969 *Social Networks in Urban Situations: Analysis of Personal Relationships in Central African Towns*. Manchester: Manchester University Press.

Pinard, Maurice

1968 "Mass society and political movements: a new formulation." *American Journal of Sociology* 73(May):682-99.

Pomper, Gerald M.

1972 "From confusion to clarity: issues and American voters." *American Political Science Review* 66(June):415-28.

Rossi, Peter H.

1959 "Four landmarks in voting research." Pp. 5-54 in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior*. Glencoe: The Free Press.

Seeman, Melvin

1959 "On the meaning of alienation." *American Sociological Review* 24(December):783-91.

Sheingold, Carl A.

1971 "New parties and new voting: a social structural analysis of the Wallace vote in 1968." Unpublished Ph.D. Dissertation, Harvard University.

Wellman, Barry and Marilyn Whitaker

1972 *Community—Network—Communication: An Annotated Bibliography*. Monticello: Council of Planning Libraries.

THE ASSEMBLING PROCESS: A THEORETICAL AND EMPIRICAL EXAMINATION*

Clark McPhail

University of Illinois at Urbana-Champaign

and

David Miller

Western Illinois University

American Sociological Review 1973, Vol. 38 (December):721-35

This paper examines the process wherein persons move from disparate points in space at time one to a common location at time two. Theoretical explanations of this phenomenon are reviewed and critically examined. An alternate interpretation is advanced and supported by data from a study of one non-periodic assembling process. A correlation of $R = .67$ is obtained between the receipt of assembling instructions, and related activities, and the completion of the assembling process. We discuss the implications of these results, and the alternate interpretation, for future theory and research on the assembling process and other aspects of collective behavior.

INTRODUCTION

...when people come together anywhere in the most casual way, on the street corner or at a railway station, no matter how great the social distances between them, the mere fact that they are aware of one another's presence sets up a lively exchange of influences, and the behavior that ensues is both social and collective (Park and Burgess, 1921:381 [emphasis supplied]).

The literature suggests that many collective behavioral phenomena require large assemblies of people. Such assemblies are neither omnipresent nor continuous. They are produced when people move from disparate points in

space at time one to a common location at time two. This assembling process creates those large congregations of people from which collective behavior may develop.

Our prime concern in this paper is neither to define nor explain collective behavior, but to ask how people come together when they do. We will (1) briefly review and critique current theoretical explanations of the assembling process, (2) outline an alternate formulation, (3) present data from one study which supports that formulation, and (4) discuss the implications of the research for subsequent examinations of the assembling process and related phenomena.

REVIEW AND CRITIQUE OF CURRENT EXPLANATIONS

The four dominant theoretical orientations in the collective behavior literature vary in their attention to and explanations of the assembling process. The *contagion* position (e.g., LeBon, 1897; Park and Burgess, 1921; Blumer, 1951; Lang and Lang, 1961) emphasizes explanatory variables which precede and follow the formation of assemblies but fails to treat the processes by which large numbers of

*Revised version of a paper presented at the 1971 meeting of the American Sociological Association, Denver, Colorado. Data analysis and preparation of this paper was supported by NIMH Grant RO 3 16696-01 and a grant from the University of Illinois Graduate Research Board. Edith Cobb and Bob Pickens assisted in the data preparation. This contribution is gratefully acknowledged as are comments on earlier drafts of this paper by Carl Couch, Norm Denzin, Joan Huber, Tony Orum, Bob Pickens, Ernie Rigney, Stan Saxton, Dick Smith, Ken Southwood, Bob Stewart and Charles Tucker.

people come together. Blumer's work is representative. There is a theoretical gap between his discussion of social unrest (1951:172-3), which by implication develops among small and disparate collections of people, and his detailed discussion of the "milling process" (1951:174-7), which develops within and transforms behavior within larger assemblies of people. The assembling process is the nexus between these diffuse and compact phases of collective behavior.

The *convergence* position (e.g., Allport, 1924; Miller and Dollard, 1941) is a response to the contagion orientation's emphasis on the transformation of individuals in "the milling process." In contrast, the convergence position attributes behavior within assemblies and the assembling process to people's common characteristics or predispositions. Allport (1924:292) states: "*the people are brought together by a common interest* preparing them for a certain type of action" (emphasis supplied). This emphasis on individual predispositions has been retained, though in modified fashion, by the "value-added" and "emergent norm" orientations.

Smelser's *value-added* position (1963) uses individual and systemic components to explain a number of collective behavioral phenomena. His discussion of the development of "hostile outbursts" is his most explicit treatment of the assembling process,¹ and two

components of his model are advanced to account for this phenomenon. "Mobilization for action" refers primarily, though not exclusively, to a two-phase process wherein different categories of people are implicated in hostile outbursts.² In the "real phase," the initial and primary participants are those who have most directly experienced "structural strain," a "precipitating factor," etc., (1963:259) and who are, therefore, most possessed by a "hostile belief." In the subsequent "derived phase," people with more diverse beliefs and motivations are attracted to the hostile outburst. A second component in Smelser's model — "structural conduciveness" — refers among other things to the possibility that people can communicate their beliefs, and to those persons' access to an assemblage location (1963:240).³ While Smelser recognizes non-predispositional variables in his discussion, he emphasizes the common beliefs of participants (cf. Quarantelli and Hundley's discussion of this point, 1969:550). Indeed, Smelser defines a hostile outburst as "mobilization for action under a [generalized] belief" (1963:226).

The *emergent norm* orientation (Turner, 1964a, 1964b; Turner and Killian, 1972) rejects the premise that people must be commonly or uniformly predisposed before acting uniformly. While acknowledging that "personal characteristics act as a selective factor in the recruitment of individuals to a crowd" (1972:21), Turner and Killian suggest that "...the hundreds or thousands of individuals who converge on the scene...are heterogeneous in motivation despite the similarity of their behavior" (1972:27). The emergent norm position advances a typology of different participation motivations

¹ Smelser defines a "hostile outburst" as "mobilization for action under a hostile belief" (1963:226) and issues the disclaimer that his definition "...does not include mere disturbances of the peace, revelrous displays of crowds, etc." (1963:226). This formulation is problematic for several reasons. First, Smelser defines the hostile outburst in terms of one of the value-added components — "hostile beliefs" — which he subsequently advances to explain hostile outbursts. This line of reasoning is tautological. Second, those "mere disturbances of the peace" and "revelrous displays of crowds" often cannot be behaviorally distinguished from the riots Smelser presents as illustrations of "hostile outbursts"; i.e., they involve the full range of violence against persons and property that constitute a riot. Consider the riots in Pittsburgh after the 1971 World Series victory, in Rio de Janeiro following Brazil's World Soccer Cup victory in 1970 or the campus area in Columbus, Ohio following several Ohio State University Big-Ten football championships. These are "riots of celebration" or "issueless riots" (cf. Gary Marx, 1970) and appear to develop without the "hostile beliefs" Smelser requires for "hostile outbursts" even though their behavioral components are quite similar.

² Smelser also refers to (1) leadership in the form of unwitting modeling behavior, individual and organizational agitation, and (2) the degree of organization among participants which exists prior to the event. We do not take issue with either of these possibilities.

³ The other two aspects of "structural conduciveness" to which Smelser refers are (1) structural cleavages in terms of which responsibility is assigned for the structural strain, and (2) the availability of channels for legitimately expressed grievances. Both considerations assume that all instances of collective behavior, or the assembling processes preceding those phenomena, presuppose strain, grievances, or hostility. We categorically disagree with this assumption. See footnote "1" above.

(1972:25-9): "the ego-involved or committed," "the concerned," "the insecure," "the curious spectator," and "the exploiter." Turner and Killian sporadically refer to communication processes (1972:38,56,61) and to ecological factors which facilitate communication (1972:61-2) as important elements in the assembling process. They do not, however, develop these elements commensurate with their systematic treatment of participant motivations. This is puzzling given the importance of communication in Turner and Killian's analysis of other phases of collective behavior.

The preceding treatments of the assembling process give inordinate, though perhaps not exclusive, weight to individual predispositions. As Melbin (1969:664) notes, this emphasis on "what people carry within them from place to place" has been the prevailing explanation of human behavior for the past half century. We will briefly consider the logical and empirical connections between individual predispositions and participation in the assembling process.

The emergent norm orientation's (1972:25-9) use of the participant motivation typology is problematic. The different types of motives appear to be inferred from the behaviors they are supposed to explain. The "ego-involved" motive is known by a participant's advocacy of some course of action (1972:27), the "concerned" motive is known by a participant's sympathetic response to a proposed action (1972:27) and the "curiosity" motive is known by a participant's mere "spectatorship" (1972:28). This tautological reasoning could be eliminated by securing independent measures of the "motives." Even so, the result would still be a variety of inferred motives. This variety is a problem when we try to identify some common variable(s) in terms of which covariation with assembling can be empirically examined.

The convergence and value-added positions' emphases on the common interests, beliefs and predispositions of participants makes them more amenable to, at least indirect, empirical examination. The predispositions are, of course, inferred from some attribute or behavior of the person and are, in turn, tested against the behavior to be explained. The validity of that inference rests on the magnitude of the correlation between the

predisposition indicator and the dependent variable. This tack has been pursued at length in studies of the relationship between personality orientations, attitudes and a variety of behaviors. The correlation seldom exceeds .30 (see Mischel, 1968 and Wicker, 1969, respectively).⁴ There is also a large body of empirical evidence on the relationship between individual attitudes, attributes and participation in collective behavior events. McPhail's (1971) secondary analysis of 215 separate tests of relationship between such variables and riot participation⁵ revealed that twenty-eight percent were not significant ($p < .05$), sixty-four percent were less than .30, seven percent were between .30 and .59, and only one percent were .60 or higher.⁶ Hence, we must conclude that individual predispositions are, at best, insufficient to account for the assembling process (cf. the similar findings and conclusions of Moinat, et al., 1972:60).

We have stated that existing orientations do not attribute participation in the assembling process solely to individual predispositions. Nonetheless, this variable is emphasized in the theoretical statements of those positions and in empirical work on the phenomenon.⁷ Communication processes and ecological factors have been underplayed and

⁴Two recent studies (Acock and DeFleur, 1972; Brannon, et al, 1973) report high correlations between specific attitudes and specific behaviors. These are striking exceptions to the general pattern of low or no relationship found in previous attitude and behavior research.

⁵The majority of "riot participation measures" failed to specify behavioral referents or to differentiate the variation in riot and non-riot behaviors across time (cf. McPhail's discussion, 1971:1068-69). In effect they only establish the person's presence in or absence from the riot area. Thus the data on riot participation are relevant to the current issue of presence at, or absence from, assemblies.

⁶One hundred and seventy-three of these tests of relationship dealt specifically with persons' attributed deprivation and/or frustration, and riot participation. Again, only seven percent of those tests yielded associations in excess of .30 (McPhail, 1971:1064).

⁷Of the 273 relationships with riot participation reported in ten studies of five different riots, 215 (78%) involved an examination of individual attitudes, socioeconomic or demographic attributes (McPhail, 1971:1061). The remainder examined social relationships and prior participation in the political process. None examined the communication processes which informed and directed people toward or away from the riot location.

underdeveloped. The formulation below is addressed to these limitations.

AN ALTERNATE APPROACH TO THE ASSEMBLING PROCESS

Students of collective behavior have typically been concerned with precipitous and sporadic vis-a-vis routine assemblies of people (Brown, 1954:833-7). While this distinction is warranted, and while our concern in this paper is primarily with the former type of assembly, the sequence of events by which both types of assemblies are formed contains important similarities and differences.

Periodic assemblies include work groups, students in Sociology 100, season ticket holders to concert, film and athletic series, as well as persons who daily congregate in front of taverns and restaurants (cf. Liebow, 1967). They are distinguished by the recurring attendance of the majority of the same participants, and by advance scheduling which establishes the time-space location. By word of mouth, course bulletins, or ticket stub information, persons are oriented to a common location in space (e.g., Room 300 Lincoln Hall) and are instructed to be at that location at common points in time (e.g., MWF, 10-11:00 a.m.).

Non-periodic assemblies include demonstrations, rallies and parades, and collections of persons at the scene of fires, accidents and arrests. Even when such assemblies occur sporadically, their membership is seldom if ever the same across time. Non-periodic assemblies are also preceded by instructions which establish common time-space locations and direct people thereto. These assemblies vary, however, with respect to the sources and processes by which people are notified of and directed to their location and with respect to the time-lag between notification and commencement.

Variations in Sources of Instructions.

Three patterns are familiar. The first is "short range notice" which precedes assemblies at the scene of accidents, fires or arguments. Such assemblages are at first composed of persons near the location by virtue of their residence, work, recreation, or fortuitous passage through the area⁸ (cf. Conot's 1967 account of the arrest of Marquette Frye and

the onset of the 1965 Watts Riot). The sights or sounds of automobile crashes, smoke, explosion, emergency vehicles, or loud voices establish that "something is happening" (cf. Turner and Killian, 1972:38). This may be followed by instructions to move to the location (e.g., "Let's go see what is happening.") or away from it (e.g., "Let's get the hell out of here.").

The second pattern is comparatively "long-range notice" — from several hours to several days — preceding rallies, demonstrations, or riots in progress. Such assemblies are primarily composed of people who are initially distant from but converge on the common location. They may learn before or during the event by radio or television, sound trucks, newspapers, leaflets, or word of mouth, that an event will occur, or is occurring, at another location.⁹ Again, this information may be accompanied or followed by instructions to move toward or away from the event location.

A third instructional pattern occurs following both the first and second patterns. The visible and audible converging movements of others, and the growing assembly size, act as non-verbal instructions to passers-by (cf. Quarantelli and Hundley, 1969:546, and Turner and Killian, 1972:62). As Milgram, Bickman and Berkowitz' (1969) research indicates, the greater the number of people involved in non-verbal gestures in a common direction, the greater the number of passers-by who will stop and orient in the same direction.

The majority of non-periodic assemblages are formed by some combination of these types of communication sources and processes. We suspect the first and third, or second and third, are most frequent; although in some instances, all three may occur. common to all three, and to periodic assemblies, is the occurrence of verbal or non-verbal instructions which establish the time-space location of an event and specify movement thereto. We will refer to these as *assembling instructions*.

It is less than novel to suggest that the

riots they investigated began at or near major pedestrian and/or vehicle intersections.

⁹ Lachman and Singer's (1968) study of the 1967 Detroit riot reports the majority of persons arrested for "participation" in the riot learned by word of mouth—either face-to-face or by telephone—rather than by direct observation or mass media announcement of the event.

⁸ Thus, the National Advisory Commission on Civil Disorders reports (1968:71) the majority of

instructions people receive affect their subsequent behaviors. Four additional considerations are in order regarding people's implication in the assembling process preceding and following the receipt of assembling instructions.

Variation in Location Access. Proximity to the location of an event increases sensory access to instructions that something has occurred. Initial observers to fires, accidents or arrests become temporary members of the assemblage without engaging in spatial relocation behaviors. Persons who can hear but cannot see are subject to the sounds of the event; e.g., crashes or explosions or sirens as well as others' voices present at or moving toward the location. In general, the nearer persons are to such events, the more likely they are to be cued that something has occurred, and the more likely they are to instruct themselves to move toward or away from the event.

Persons who can neither see nor hear the event are dependent on others for instructions — face to face communications, phone calls, or mass media announcement — to establish the existence of the event or to specify movement toward or away from it (cf. Lachman and Singer, 1968). Furthermore, the greater the number of people co-present when these announcements occur, the more likely the occurrence of additional instructions to move toward or away from the event.

Finally, assemblage participation is, in part, a simple function of the number and range of people having physical access to the event. The majority of the 1967 riots began at or near major pedestrian and vehicle intersections and in the heart of densely populated residential areas (National Advisory Commission on Civil Disorders, 1968:71) (cf. Grimshaw's, 1960:119, discussion of racial violence at transportation transfer points during the WW II riots). Further, large assemblies can quickly form in urban ghettos and on college campuses alike by virtue of the immediate access of large numbers of people living in close proximity. Where the event location is beyond walking distance, access to private transportation is important for the number and range of people who can participate (cf. Michel McCall's 1970:354, discussion of this point).

Variation in Prior and Ongoing Behaviors. We have described sources and processes by

which people may learn of an event, may be directed toward or away from its location, and may have access to that location. It is more difficult to account for whether the instructions will be to move toward or away from the location. It is possible that others selectively designate people on the basis of their prior and ongoing activities and that this designation contributes to the generation and receipt of instructions for behavior. This interpretation emphasizes what people are doing with and in relation to one another rather than the predispositions of individuals.

It is axiomatic in sociology that people are designated by others, and in turn designate themselves, on the basis of their prior and ongoing activities and the responses of others to those activities (Mead, 1934:138, 171; 1938:152). There is considerable empirical evidence of a relationship between the frequency and/or duration of prior activities and corresponding self designations (Kuhn, 1960; Tucker, 1967, 1969; McPhail, 1972). By implication, both "radicals" and "athletic fans" are designated by others and themselves on the basis of their prior and ongoing participation in political and athletic activities. When people have been designated in a manner correspondent with pending events, they may be more likely to receive instruction from others and to instruct themselves to move to the location of those events.

While our discussion emphasizes the instructions people receive from others, we recognize that people instruct themselves. The proportion of and relationship between self instructions and instructions from others, and the effect on behavior, is an important issue we do not propose to resolve in this paper. Nonetheless, three points should be noted. First, covert self instructions cannot be directly observed but must be inferred from subject reports. Overt instructions, from self or others, can be observed, reported and recorded. Second, we speculate that the greater the number of consistent instructions a person receives from others, the more likely that person will engage in self instructions commensurate with others' instructions (Mead, 1925:288). (For a review of relevant research, see Bem, 1972.) Third where overt instructions can be observed, reported and recorded, it seems plausible to examine that sub-set of instructions in relation to the dependent variable under examination. Previous research

in a variety of settings consistently supports the assertion that overt instructions from others produce and/or alter the subsequent behavior of those to whom they are addressed (for a summary see McPhail and Rigney, 1973).

We do not suggest that all people at demonstrations or rallies — and certainly not those at the scene of fires, accidents or riots — *have a prior history of assembling for such events.* Fortuitous proximity to an event, or to large numbers of others when the existence of an event is established, may largely determine who is subject to instructions. We suspect that prior behaviors, in themselves, are insufficient to implicate a person in the assembling process. Prior behaviors may, however, increase the likelihood of receiving instructions about an event. We shall return to this question later.

Variation in Availability. Instructions received are not inevitably or immediately instructions carried out. People may be engaged in behavior which prior commitments — i.e., competing instructions — require them to complete before they can move to the assemblage location. Similarly, competing demands from family, job or other social relationships may preclude their movement altogether. Conversely, people subject to fewer competing demands are more available for assemblage participation. A variety of evidence supports this interpretation.

Of all the demographic and socio-economic attributes and social relationships examined during the riot studies of the 1960's, the only consistent associations with riot participation involved young, black, single males who lived alone, had no organizational memberships and/or who had lived in the riot city less than ten years (McPhail, 1971:1069). These data could suggest that "impetuous youth," "disenchanted blacks," "daring males," and "alienated isolates" are more predisposed to riot. An equally plausible interpretation is that young, single, black males, minimally involved in scheduled social relationships with their competing claims on time, are simply more available for participation.

This interpretation is further supported by the report of the National Advisory Commission on Civil Disorders (1968:71) that the

majority of the 1967 riots originated during the evenings or on weekends. At those times large numbers of people are off work. Similarly, most campus demonstrations begin after noon, when comparatively fewer classes are scheduled and almost never occur during final exam week.

Thus, involvement in routine activities can preclude or at least delay participation. Lachman and Singer's (1968) study of participants in the 1967 Detroit riot revealed the majority were some distance removed from the riot location when they learned it was underway, were engaged in routine activities, and were delayed over an hour between learning of the event and moving to its location. In sum, we suggest that the greater the relative freedom of people from the competing demands of routine scheduled activities — competing instructions — the more likely they are to carry out the assembling instructions they receive.

Space-Time Lag, Sequence and Frequency of Instructions. The greater the space and time lag between a pending assemblage and its commencement, the greater the opportunity for people to be engaged in action which moves them away from the assembly or to be involved in commitments from which they are not readily extricated. Similarly, people may be subject to instructions directing them to an assembly location, begin to go there and be deflected enroute by the competing instructions of their traveling companions or others along the way. People may stop to shop, eat, drink, service automobiles or converse. In the event of intervening and alternate instructions, the assembling instructions must recur for persons to resume their course toward the assembly location.

The examination of one non-periodic assemblage generated data pertinent to the interpretation of the assembling process discussed above. Those data are presented and discussed in the following section.

THE EXAMINATION OF ONE NON-PERIODIC ASSEMBLING PROCESS

The Incident. On February 28, 1968, between 11:00 p.m. and 12:30 a.m., approximately 4,000 people assembled at the airport serving the capitol city of a southeastern

Atlantic coastal state. Some two and one-half hours earlier, a radio broadcast had relayed the upset victory of the state university's basketball team over a nationally ranked and traditional rival team in a neighboring state. The announcer also reported that the victorious team would be returning to the capitol city airport at approximately midnight. The congregation at the airport provided an opportunity to examine the sources and processes by which non-periodic assemblies are formed.

The Sample. Within three weeks following the assembly, a questionnaire was administered to a sample of 531 students in sociology and psychology classes at the state university, located in the capitol city.¹⁰ The questionnaire solicited information regarding respondents' participation in the assembly, a sample of assembling instructions addressed to them during the interim between the radio broadcast and the assembly, a sequential description of their activities, locations, and the number of persons with whom they were co-present during that period, their prior activities regarding the basketball team, their access to transportation to the assembly location, the number of scheduled competing demands during and/or immediately following the assembly period, and their sex and year in

school. The indices constructed from these responses are described below.

The Indices: 1. *Space-Time Proximity to Assembly Location.* The majority of our respondents were located in the capitol city during the time period in question and were between six and eight miles from the airport. Further, the majority of respondents who learned of the event did so between one and two and one-half hours in advance. All were dependent on mediated sources of instructions about the event¹¹ and on access to private transportation to get to the location. Therefore, space-time proximity to the assembly site was treated as a constant and was not entered in the analysis.

2. *Social Density Index.* Our questionnaire solicited reports of respondents' locations, and the number of others with whom they were co-present, at two points in time between the initial announcement and the commencement of the event. Respondents reporting they were in dormitories, fraternity houses, the student union, campus area bars, theatres, or other places with large numbers of co-present others, were given a high density score. Those reporting they were alone or with only two or three others, in parental homes, in remote off-campus residences, or driving in their automobiles were given a low density score. People in high density locations at both points in time were assigned a score of three; those in early low density and later high density locations were assigned a score of two; those in early high density and later low density locations were assigned a score of one; and those in low density locations at both points in time were assigned a score of zero. Consistent with the interpretation discussed above, we would expect a direct association between social density and the receipt of assembling instructions.

3. *Assembling Instructions Index.* Our questionnaire solicited reports of whether or not people were subject to requests, suggestions or directives to go to the assemblage on five separate occasions during the time period between the final minutes of the game broadcast (approximately 9:30 p.m.) and

¹⁰ Early descriptions of the airport assembly indicated the participants were primarily university students. Consequently, we suspected the student body represented a population from which samples of participants and non-participants could be drawn. Of the 531 questionnaires administered, 466 (87%) were complete for all variables in the analysis reported here with the exception of the Social Density Index. The coding procedure for this variable yielded usable information on only 409 respondents, and the analysis reported in this paper is based on the smaller sample. The proportion of respondents who reported attendance at the assembly (32%) was the same in all three samples.

Various newspaper accounts estimated the size of the assembly between 3,000 and 5,000 people. Assuming 4,000 people at the assembly, it should be noted that the analysis reported here is based on approximately three percent of those in attendance. We cannot attest to the representativeness of our sample from the population of persons assembled at the airport. This is a difficult problem to resolve when sampling from non-periodic assemblies where the population is transitory and its characteristics cannot be known. We do know that 49% of our sample were females and 51% were males. We also know that 35% were freshmen/women, 27% were sophomores, 23% were juniors, and 15% were seniors and graduate students.

¹¹ One respondent reported he learned of the event at the assembly site as he was leaving the airport where he had just put a friend on a departing flight. As he left the airport he encountered the converging people, inquired "What's happening?" and was informed of the event.

12:00 midnight.¹² For example: "Immediately following the announcement (of the upset victory) did anyone you were with propose that you go to the airport?" Similar questions inquired about two subsequent points during the pre-assembly period. In addition, we inquired whether or not people were subject to public announcements, e.g., dorm or student union loudspeakers, radio, or television, directing them to the assembly, and whether or not they were asked by others to provide transportation to the assembly.¹³ Guttman scalogram analysis yielded a coefficient of reproducibility of .94 among these five reported instructions, and they are treated here as an Index of Assembling Instructions with a score ranging from zero to five.

Consistent with the interpretation discussed above, we would expect a direct association between people's scores on the Assembling Instructions Index and their presence at the assembly, and that such an association would remain relatively constant

¹² Respondents were also asked to identify the source of the proposal to go to the assemblage. Less than four percent of the respondents identified themselves as the source of the instruction. The majority identified a friend or acquaintance.

¹³ The questions asked of respondents who listened to the game broadcast (63% of the sample) differed somewhat in content, but not in form, from those asked of non-listeners. Responses to both sets of questions were combined to provide comparable information about similar behaviors which occurred in approximately the same time period. For example, broadcast listeners were asked: "While listening to the game did anyone you were with (or anyone else) propose that you go to the airport?" Non-listeners were asked: "Immediately following the announcement of the win, did anyone you were with (or anyone else) propose that you go to the airport?" "Yes" answers to questions were treated the same and assigned a value of "1" in the construction of the Assembling Instructions Index. Following this question we then asked respondents for the source of the proposal (see fn. 12 above), descriptions of their subsequent activities, location and relationships to co-present others, and then repeated questions about assembling instructions and source at two subsequent points in the temporal sequence covered by the questionnaire. In addition, we asked for reports of the receipt of two other assembling instructions. Both broadcast listeners and non-listeners were asked: "Did you hear any public announcements (over TV, radio, loudspeakers, etc.,) urging people to go to the airport?" and "Were you asked to provide rides to the airport?" "Yes" answers to each of the five questions were assigned a value of "1" in the construction of the Assembling Instructions Index which ranged from 0 to 5.

when other variables are considered.

4. *Prior Behavior Indices.* Our questionnaire solicited reports of three types of "fan behavior" prior to the assembly: frequency of attendance at prior home basketball games, prior plans to listen to the broadcast of the game preceding the assemblage in question, and listening to the broadcast of that game.

Consistent with the interpretation advanced above, we would expect relatively high associations among each of these indices of prior fan behavior as well as between the frequency of prior game attendance and the receipt of assembling instructions. Assuming the operation of a "labelling process," which we did not measure, the more frequent the prior participation in activities related to a pending event, the more likely the person is to receive the "attribution" of "athletic fan" and subsequently, to be the target of instructions from others to participate in such events. Finally, we would expect a direct association between these indices of prior fan behavior and presence at the assembly, but would also expect this association to diminish when controlling for the effect of the receipt of assembling instructions.

5. *Transportation Access.* Our questionnaire asked: "Were you offered, or did you have access to, a ride to the airport?" Given the distance between most respondents' locations when they received initial assembling instructions and the assembly location, access to private transportation would be required to carry out those instructions.¹⁴ Therefore, consistent with the above interpretation, we would expect a direct association between transportation access and presence at the assemblage but would also expect that association to diminish when controlling for the effect of the receipt of assembling instructions.

6. *Index of Relative Availability.* The majority of university students have comparatively few scheduled commitments during the evening hours when the assembly in question occurred. Nonetheless, in response to our questionnaire, many respondents did report

¹⁴ Although our questionnaire inquired if respondents owned a car and/or had transportation to the assemblage location, these variables were not considered in the initial analysis and report of our data. We are indebted to an anonymous reviewer for urging us to examine these "obvious," but also equally obviously overlooked, variables.

work, family, and study obligations (e.g., examination preparation) during the time of the assemblage and/or the following morning (e.g., work, 8:00 a.m. classes, or early morning examinations). An Index of Relative Availability was constructed from this information. Respondents reporting no scheduled competing demands during the time of the assembly or the following morning were assigned a score of two. Those reporting one or more competing demands the following morning were given a score of one. A score of zero was assigned those reporting one or more competing demands during the assembly period plus, in some instances, competing demands the following morning.

We do not suggest that the absence of competing demands will result in persons' implication in the assembling process. Rather, assembling instructions are more likely to be implemented when people are relatively free from such demands.

7. Other Variables. Respondents' sex and year in school were included in our analysis for several reasons, none of which were discussed in the above interpretation. Young males have long occupied a special status in lay and academic discussions of collective behavior participation. The carefree and irresponsible freshman male has often been singled out as the "ideal typical" participant in campus water fights, and in athletic victory celebrations like the one in question. Further, several reviewers of an earlier draft of this paper acknowledged they would be more convinced by our results if they held up when controlling for respondent's sex and age or year in school. Therefore, respondents' sex was entered in the analysis as a dummy variable, with males receiving a value of "1" and females a value of "0." Similarly, freshmen/women students were assigned a score of "3," sophomores a score of "2," juniors a score of "1," and seniors and graduate students a score of "0." Consistent with the above interpretation, we would not expect a high association between either of these variables and presence at the assembly, and would expect any association to diminish when controlling for the receipt of assembling instructions.

8. Presence at the Assemblage. Finally, respondents were asked: "Did you go to the airport?" "Yes" answers were given a score of "1" and "no" answers were given a score of

"0" and presence at the assemblage—or the completion of the assembling process—was treated as a dummy variable in the analysis below.

Results of Analysis. The zero-order relationships among all the aforementioned variables and Presence at the Assembly are reported in Table 1.¹⁵ The multiple correlation between all variables and Presence at the Assembly was .67. Consistent with our interpretation, the Assembling Instructions Index yielded the highest zero-order correlation with Presence at the Assembly: .59. Respondents' sex and year in school, advanced as "direct" causal variables by other interpretations, yielded low zero order correlations with Presence at the Assembly: .26 and -.09, respectively. We also call attention to the moderate relationships among the three indices of prior "fan" behavior. The correlation between Attendance at Prior Home Games and Planned to Listen to the Game Broadcast was .44; between Planned to Listen and Listened to the Game Broadcast was .47; between Attendance at Prior Home Games and Listened to the Broadcast was .36. Our interpretation of the assembling process advanced no direct relationship between any of the remaining variables and Presence at the Assembly. We will discuss their indirect relationships with the dependent variable following an examination of some possible sources of assembling instructions.

Our discussion of the assembling process suggested that the receipt of assembling instructions results, in part, from the number of others in the person's pre-assembly location, when the existence of the event is established, the person's prior behaviors, and an intervening attribution process, which we did not examine. The Social Density Index, three indices of prior "fan" behaviors, and respondents' sex and year in school were all regressed on the Assembling Instructions Index. The results of that analysis are presented in Table 2.

The multiple correlation of all these variables with the Assembling Instructions Index was .57. Consistent with our interpretation, the Social Density Index and Attendance at

¹⁵ We are reporting product moment correlation coefficients. For a discussion of the assignment of numbers to rank order categories, see Labovitz (1970).

Table 1., Zero Order Correlation Matrix for all Variables

	1.	2.	3.	4.	5.	6.	7.	8.	9.
1. Respondent's year in school	---								
2. Index of relative availability*	-.04	---							
3. Planned to listen to game broadcast	-.07	.02	---						
4. Sex of respondent	-.15	.13	.34	---					
5. Social density index	.10	.11	.12	.05	---				
6. Listened to game broadcast	-.13	.06	.47	.37	.14	---			
7. Attendance at prior home games	.04	.06	.44	.35	.22	.36	---		
8. Transportation access	-.12	.02	.19	.22	.21	.21	.26	---	
9. Assembling instructions index	-.01	.07	.27	.26	.42	.30	.43	.45	---
10. Presence at the assembly	-.09	.20	.25	.26	.51	.32	.42	.44	.59
R = .67									

*Scored "2" for relative absence of competing demands through "0" for relative increase in competing demands.

Prior Home Games yielded the highest standardized regression coefficients with the Assembling Instructions Index: .33 and .27, respectively. The remaining indices of prior behaviors, respondents' sex and year in school yielded quite low coefficients. Although accounting for the sources of assembling instructions was not our primary aim, and we will return to this issue in the last section of the paper, those data we did examine are consistent with our discussion of possible sources.

Table 2. Standardized Regression Correlation Coefficients with the Assembling Instructions Index

Variable	Beta
Social density index	.33
Attendance at prior home games	.27
Listened to game broadcast	.10
Sex of respondent	.09
Planned to listen to game broadcast	.04
Respondent's year in school	-.02
ALL	R = .57

Our interpretation of the assembling process places primary importance on the receipt of instructions which establish the location of a common time-space frame and specify movement to that location. The assembling process will not be initiated or completed without the receipt of such instructions. Hence we expect that the zero-order correlations between the other variables and Presence at the Assembly should diminish when controlling for people's scores on the Assembling Instructions Index. Conversely, the relationship between the Assembling Instructions Index and Presence at the Assembly should remain constant when controlling for each other variable. The results of partial correlation analysis of these relationships are presented in the first three columns of Table 3.

Column (1) of Table 3 lists the zero-order relationships of all the variables with Presence at the Assembly. Column (2) of that table presents the partial correlation of each variable with Presence at the Assembly when controlling for the person's score on the Assembling Instructions Index. It is clear that the magnitude of correlation between each variable and Presence at the Assembly diminishes considerably—in most instances by nearly half—when controlling for people's scores on the Assembling Instructions Index. The only exception to this pattern is the

Table 3. Zero Order, Partial, and Standardized Regression Correlation Coefficients with Presence at Assembly

Variable(s)	(1) Zero Order Correlation of Each Variable with Presence at Assembly	(2) Partial of Each Variable with Presence at Assembly Controlling for A I Index	(3) Partial of A I Index with Presence at Assembly Controlling for Each Variable	(4) Beta Coefficients of Each Variable with Presence at Assembly
Assembling instructions index (A I Index)	.59	---	---	.38
Transportation access	.44	.23	.49	.18
Attendance at prior home games	.42	.23	.50	.17
Listened to game broadcast	.32	.18	.55	.08
Social density index	.31	.09	.54	.06
Sex of respondent	.26	.14	.55	.01
Planned to listen to game broadcast	.25	.12	.56	-.02
Index of relative availability	.20	.19	.59	.14
Respondent's year in school	-.09	-.10	.59	-.06
ALL	R = .67			R = .67

Index of Relative Availability. While the zero-order correlation between this index of scheduled competing instructions and Presence at the Assembly was low ($r = .20$)—as suggested by our interpretation—this relationship alone remains relatively constant (.19) when controlling for the Assembling Instructions Index.

Column (3) of Table 3 presents the partial correlation of respondents' scores on the Assembling Instructions Index with Presence at the Assembly when controlling for each other variable. The zero-order relationships between assembling instructions and completion of the assembling process ($r = .59$) is not altered by the respondent's year in school nor by the number of scheduled competing demands. There is some diminution of the relationship when controlling for respondents' reported plans to listen to the game broadcast (.56), sex of respondent (.55), listening to the game broadcast (.55), and the number of others with whom respondents were co-present prior to the assembly (.54). The largest drop in the relationship between the receipt

of assembling instructions and completion of the assembling process occurs when controlling for prior attendance at home games (.50) and the availability of transportation to the assemblage location (.49)¹⁶

Column (4) of Table 3 presents the standardized regression correlation coefficients

¹⁶In view of the zero-order correlation between Transportation Access and the Assembling Instructions Index ($r = .45$ see Table 1), the question can be raised about a causal relationship between those two variables. We have no theoretical reason to advance such an hypothesis. We considered the possibility that one of the items in the Assembling Instructions Index (a request to the person for transportation to the assemblage location) was yielding a relationship with the Transportation Access question which asked: "Were you offered, or did you have access to, a ride to the airport?" That is, it might be possible for others to seek out a person known to have available transportation and to request such transportation to the assemblage. We checked this and the relationship between car ownership and receiving a request for transportation. Both were statistically nonsignificant, yielding zero-order correlations of $-.04$ and $-.06$ respectively.

between each variable in the analysis and Presence at the Assembly. Consistent with the earlier relationships, and with our interpretation of the assembling process, the Assembling Instructions Index is the best predictor of completing the assembling process ($B = .38$) when all other variables in the analysis are simultaneously controlled. The availability of transportation to the assembly location, the number of prior home games attended, and the relative absence of scheduled competing demands during and immediately after the assembly yield Beta coefficients of .18, .17, and .14, respectively. The remaining coefficients are considerably lower.

In sum, the empirical evidence from one study of a non-periodic assembling process supports the alternate formulation we have advanced. We have demonstrated a strong relationship between the receipt of instructions which establish and specify movement to a common time-space location, and the convergence of people on that location. At the same time, however, a number of questions should be raised about this research and the interpretation we have advanced.

DISCUSSION

First, we suggested that the receipt of instructions is responsible for producing the convergent movement which results in both periodic and non-periodic assemblies. That assertion remains untested as we have only presented data for the non-periodic assembling process. The periodic assembly remains to be investigated.

Second, we have only examined one non-periodic assembly, and it differs from many such assemblies including some we discussed above. This particular assembly involved a comparatively long time interval between announcement and congregation. Further, the respondents in our study were several miles from the event when they received instructions to assemble. Their access to transportation was important if they were to carry out those instructions. In contrast, many non-periodic assemblies which form around the site of accidents, fires or arrests may be first composed of persons who are within sight or sound or, at least, within walking distance of the location. While we would argue that such assembling processes also depend on the receipt of verbal and/or non-verbal instructions,

the present research cannot speak to the contention.

Third, while we have spoken of "the assembling process" throughout the paper, our research dealt almost exclusively with the initiation and completion of that process: it did not deal with the variations in locomotion behaviors. We emphasize this shortcoming because of the parallel with the "monolithic conception" of collective behavior which pervades the literature; i.e., the erroneous assumption that individual or collective behavior sequences, be they violent or non-violent, are continuous for and inclusive of all participants. Our suspicion is that the assembling process does not proceed without interruption, deflection, and often termination, of the participants' locomotion. For example, three respondents—who reported receiving all five assembling instructions—did not reach the assembly. One was enroute when she encountered her fiancé who insisted she spend the evening with him. Another was enroute with friends who proposed a detour by a dormitory to pick up some record albums and the assembling movement was terminated. A third was enroute when his car broke down, and he spent the remainder of the evening making repairs. Theoretically, such delays and alterations should be preceded by alternate or competing instructions for behavior and should be followed by a repetition of assembling instructions if those persons are to return to and proceed on course to the assembly. Future research should give attention to these deflections in the direction and velocity of behaviors in the assembling process.

Fourth, this research did suggest some factors which can contribute to the receipt, and to the "directional content," of assembling instructions. We have not empirically examined all those factors, nor have we sought to. On this point we are in agreement with Merton's (1960:xxxv) reminder. While the sociologist

.... is sometimes prepared to say what might be reasonably expected to occur under specified conditions, he is often not in a position to say when or even whether these conditions, indispensable to the predicted outcome, will themselves occur.

Students of collective behavior will not be able to speak to this issue until they move

from efforts to predict outcomes within "natural systems" to controlling outcomes within "constructed systems."¹⁷ That is, a desirable next step for the student of assembling processes is to produce an assembly by manipulating the source and sequence of assembling and alternate instructions which persons receive (cf. the practices of "advance men" in political campaigns, Bruno and Greenfield, 1972:48-9, 58-65, 71-4). Following such procedures, a sociologist could state when or whether the "indispensable conditions" had occurred.

Fifth, and by conventional standards, we have accounted for somewhat less than half the variance in "the assembling process" ($R^2 = .45$). While this is considerably better than previous research in collective behavior, much variance remains to be accounted for. Subsequent investigations could account for an even greater proportion by (1) obtaining more detailed information about the sequencing of assembling and alternate or competing instructions, and/or (2) manipulating the sources and sequences of assembling and alternate instructions in a "constructed system." Undoubtedly some critics will contend that our success would have been greater had we also examined our respondents' attitudes. In all candor, we did not do so because of the unsatisfactory theoretical specification of the relationship between attitudes and behavior (Blumer, 1955), because of the poor predictive success of attitude research at the time we conducted the study, and because of nearly total empirical neglect of the effect of others' behaviors on people's participation in collective behavior events. When we have a clearer theoretical specification of the relationship between the individual's attitudes and others' behaviors, future investigators may be able to examine this configuration of influences in relation to the assembling process.¹⁸

¹⁷ We refer here to Henshel's (1971:214) excellent discussion of "sociology and prediction" in "natural" and "constructed systems."

It is essentially true that . . . the future cannot be predicted, but futures can be invented. The basic point is that man tends to predict by controlling . . . We draw a distinction, then, between prediction in 'natural' and 'constructed' systems—meaning by the latter not only that they are man-made but that they are fashioned strictly according to the requirements of the scientist or engineer. Prediction can be attempted in either case, but the results are likely to be considerably different.

¹⁸ The "configuration" approach advocated by Acock and DeFleur (1972) emphasizes the individual's attitudes and perception of others' sup-

Sixth, the strength of relationship we obtained between instructions for and participation in the assembling process warrants further exploration of the relationship between instructions for and behavior within assemblages. Milgram, Bickman and Berkowitz (1969) report an impressive association between non-verbal instructions and elementary collective behavior in an assemblage (for another discussion see McPhail, 1969). This proposal would allow one to account for space-time variations in behavior sequences within assemblies in terms of variations in the verbal and non-verbal instructions to which people are subject by virtue of their location within an assembly.

Finally, neither our research nor our alternate interpretation has addressed the relationship between the sources of the assembling process and the behaviors which may develop in an assemblage. Some persons may be instructed to assemble on the campus quadrangle at seven o'clock for an anti-war demonstration, while others may be instructed to assemble at the same time and place to "bug the peaceniks," or "trash the armory." While all three sets of instructions should

port for those attitudes. Relatively little attention is given the behaviors others—including the investigator—address to the individual in the situation in which the dependent behavioral variable is observed and recorded. Nonetheless, by eliciting respondents' statements of specific attitudes toward specific objects and subsequently confronting them with a clear and unequivocal opportunity to engage in that behavior, Acock and DeFleur's research (as well as that of Brannon, et al., 1973) yields an impressive correlation between attitude and behavior.

Equally impressive correlations have been obtained between other's instructions for and the individual's corresponding changes in behavior in experimental and non-experimental studies (see McPhail and Rigney, 1973, for a review of these studies). It may be possible to theoretically and empirically integrate these potential sources of behavioral change. Attitudes can be viewed as self instructions for behavior. Self instructions, like instructions from others, may yield greater correspondence in behavior: (1) the more precisely they designate an object and specify conduct toward that object; (2) the greater the opportunity of the behavior in question; and (3) the shorter the time lag between designating and specifying conduct toward an object and confronting the behavioral opportunity. The greater the time lag, on the other hand, the greater the chance for intervening and alternate instructions. If these criteria could be met, and if self instructions could be measured in combination with instructions from others, a more promising "configuration" approach would be at hand.

produce an assembly, subsequent instructions at the assembly, for violent or non-violent collective action, may have differential effects which correspond to the assembling instructions.¹⁹ This issue, along with those discussed above, warrants careful attention in future theoretical and empirical examinations of the assembling process and its consequences.

¹⁹In the study reported here, no respondent reported that any assembling instruction implied or suggested that individual or collective violence would occur at the assemblage. Nonetheless, a riot did occur at the assemblage involving several hundred dollars of looting and property damage, police use of tear gas to disperse the assemblage, an attempt to commandeer and then to overturn a police car, and several arrests. See footnote 1, above.

REFERENCES

- Acock, Alan C. and Melvin L. DeFleur
1972 "A configuration approach to contingent consistency in the attitude-behavior relationship." *American Sociological Review* 37(December):714-26.
- Allport, Floyd
1924 *Social Psychology*. Cambridge, Mass.: Houghton-Mifflin.
- Bem, Daryl J.
1972 "Self-perception theory." Pp. 1-62 in Leonard Berkowitz (ed.), *Advances in Experimental Social Psychology*. Volume 6. New York: Academic Press.
- Brannon, Robert, et al.
1973 "Attitude and action: a field experiment attached to a general population survey." *American Sociological Review* 38(October): 625-36.
- Blumer, Herbert
1951 "Collective behavior." Pp. 167-222 in Alfred McClung Lee (ed.), *Principles of Sociology*. New York: Barnes and Noble.
1955 "Attitudes and the social act." *Social Problems* 3(Summer):59-65.
1957 "Collective behavior." Pp. 127-58 in Joseph B. Gittler (ed.), *Review of Sociology*. New York: John Wiley.
- Brown, Roger
1954 "Mass phenomena." Pp. 833-76 in Gardner Lindzey (ed.), *Handbook of Social Psychology*. Volume II. Reading, Mass.: Addison-Wesley.
- Bruno, Jerry and Jeff Greenfield
1972 *The Advance Man*. New York: Bantam Books.
- Conot, Robert
1967 *Rivers of Blood, Years of Darkness*. New York: Bantam Books.
- Grimshaw, Alan
1960 "Urban racial violence in the United States: changing ecological considerations." *American Journal of Sociology* 64(September):109-19.
- Henshel, Richard
1971 "Sociology and prediction." *The American Sociologist* 6(August):213-20.
- Kuhn, Manfred
1960 "Self attitudes by age, sex and professional training." *The Sociological Quarterly* 1(January):39-55.
- Labovitz, Sanford
1970 "The assignment of numbers to rank order categories." *American Sociological Review* 35(June):515-24.
- Lachman, Sheldon J. and Benjamin Singer
1968 *The Detroit Riot: 1967*. Detroit, Mich.: Behavior Research Institute.
- Lang, Kurt and Gladys Engel Lang
1961 *Collective Dynamics*. New York: Crowell.
- LeBon, Gustav
1897 *The Crowd*. New York: Viking Press (1960 edition).
- Liebow, Elliot
1967 *Talley's Corner*. Boston: Little, Brown Company.
- Marx, Gary
1970 "Issueless riots." *Annals of the American Academy of Political and Social Science* 391(September):21-33.
- McCall, Michel
1970 "Some ecological aspects of Negro slum riots." Pp. 345-61 in Joseph Gusfield (ed.), *Protest, Reform and Revolt*. New York: John Wiley.
- McPhail, Clark
1969 "Student walkout: a fortuitous examination of an elementary form of collective behavior." *Social Problems* 16(Spring): 441-55.
1971 "Civil disorder participation: a critical examination of recent research." *American Sociological Review* 36(December): 1058-71.
1972 "Religious self designating behaviors." *Journal for the Scientific Study of Religion* 11(September):262-70.
- McPhail, Clark and Ernest Rigney
1973 "Instructions and compliance: a re-examination of the demand characteristics of experiments and other social encounters." Paper presented at the annual meetings of the Midwest Sociological Society. Milwaukee, Wisconsin (mimeographed).
- Mead, George
1925 "The genesis of the self and social control." *International Journal of Ethics* 35(April):251-77.
1934 *Mind, Self and Society from the Standpoint of a Social Behaviorist*. Chicago: University of Chicago Press.
1938 *The Philosophy of the Act*. Chicago: University of Chicago Press.
- Melbin, Murray
1969 "Behavior rhythms in mental hospitals." *American Journal of Sociology* 74(May): 650-65.
- Merton, Robert K.
1960 "The ambivalences of LeBon's *The Crowd*." Pp. v-xxxix in Merton's introduction.

- tion to the re-issue of Gustave LeBon's *The Crowd*. New York: Viking Press.
- Milgram, Stanley, Leonard Bickman and Lawrence Berkowitz
1969 "Drawing power of different size crowds." *Journal of Personality and Social Psychology* 13(October):79-82.
- Miller, Neal E. and John Dollard
1941 *Social Imitation and Learning*. New Haven: Yale University Press.
- Mischel, W.
1968 *Personality and Assessment*. New York: John Wiley.
- Moinat, Sheryl, et al.
1972 "Black ghetto residents as rioters." *Journal of Social Issues* 28(October):45-62.
- National Advisory Commission on Civil Disorders
1968 *Report of the National Advisory Committee on Civil Disorders*. Washington: U.S. Government Printing Office.
- Park, Robert E. and Ernest W. Burgess
1921 *Introduction to the Science of Sociology*. Chicago: University of Chicago Press (1969 abridged edition).
- Quarantelli, Enrico and James Hundley
1969 "A test of some propositions about crowd formation and behavior." Pp. 538-54 in Robert Evans (ed.), *Readings in Collective Behavior*. Chicago: Rand-McNally.
- Smelser, Neil
1963 *Theory of Collective Behavior*. New York: Free Press of MacMillan.
- Tucker, Charles
1967 "Occupational prestige and self designation: a critical examination." *Sociological Focus* 2(Summer):107-16.
1969 "Occupation and work self identification: a reconsideration." *The Sociological Quarterly* 7(Summer):345-58.
- Turner, Ralph
1964a "Collective Behavior." Pp. 382-425 in Robert E. L. Faris (ed.), *Handbook of Modern Sociology*. Chicago: Rand-McNally.
1964b "New theoretical frameworks." *The Sociological Quarterly* 5(Spring):122-32.
- Turner, Ralph and Lewis Killian
1972 *Collective Behavior*. Englewood-Cliffs, N.J.: Prentice-Hall. Second edition.
- Wicker, Alan C.
1969 "Attitudes versus actions: the relationship of verbal and overt behavioral responses to attitude objects." *Journal of Social Issues* 25(Autumn):41-78.

VALUES AND VIOLENCE: A TEST OF THE SUBCULTURE OF VIOLENCE THESIS*

Sandra J. Ball-Rokeach

Washington State University

American Sociological Review 1973, Vol. 38 (December): 736-49

This research examines the hypothesis that violent behavior results from a commitment to a subcultural value and attitude pattern. Special attention is given to Wolfgang and Ferracutti's subculture of violence thesis. Two types of violent behavior are considered - interpersonal violence and violent crime. Findings are drawn from two independent studies: one on the values and attitudes associated with interpersonal violence in a national area probability sample of 1,429 adult Americans, and the other on value differences among 363 men incarcerated in a Michigan prison for various violent and non-violent felonious offenses. The data do not support the subculture of violence explanation of violent behavior. Additional findings are presented which indicate, in the national sample, that neither socio-economic status nor social class values are associated with violence.

Attempts to explain violence abound. Among these are various subcultural theories, the most comprehensive of which holds that violent behavior results from a commitment to a subcultural value and attitude pattern. While our overriding concern is to assess the validity of any subcultural approach to violence, special attention will be given to the best known contemporary statement of the position, Wolfgang and Ferracutti's (1967).

Like any subculture, the subculture of violence shares many values and attitudes with the parent culture. To understand why adherents to the "subculture" behave violently, one must look to the values and attitudes of the subculture. Accordingly, members of this subculture behave more violently than others because they conform to the conduct norms, attitudes, and ultimately values of that subculture.¹ While Wolfgang and Ferracutti do not identify the distinctive values of this subcul-

ture, they indicate that subculture members evaluate each other in terms of their conformity to a "machismo" life style, which emphasizes such things as leading an exciting life, achieving status, and protecting one's honor. Moreover, since violence is associated with masculinity in the "machismo" concept, adherents to the subculture of violence are primarily male.

Two types of violence will be considered in the present research: interpersonal and deviant. Interpersonal violence is defined simply as "the threat or exertion of physical force which could cause bodily injury" (Ball-Rokeach, 1972). All acts between two or more persons, legal and illegal, legitimate and illegitimate, which involve the actual or threatened use of injurious physical force are included. Interpersonal violence is more generic than deviant violence. Deviant violence is defined as those acts of interpersonal violence considered illegitimate by those with

*This research was supported, in part, by an institutional grant from the University of Western Ontario and by a grant from the National Science Foundation to Milton Rokeach. Paul B. Sheatsley and Eve Weinberg of the National Opinion Research Center receive my special thanks for their unusual assistance and support, and Dr. Raymond Cochrane's generosity in granting me access to his data is sincerely appreciated.

¹Although Wolfgang and Ferracutti focus their attention on subcultural determinants of homicide, there is no compelling theoretical reason why their

thesis, if valid, should not also apply to less extreme forms of violence. Wolfgang and Ferracutti state: "We have said that overt use of force or violence, either in interpersonal relationships or in group interaction, is generally viewed as a reflection of basic values that stand apart from the dominant, the central, or the parent culture. Our hypothesis is that the overt (and often illicit) expression of violence (of which homicide is only the most extreme) is part of a subcultural normative system, and that this system is reflected in the psychological traits of the subculture participants" (p. 158).

the power to enforce norms of violent behavior. In the present context, it refers to such violent crimes as homicide, assault, and armed robbery.

Values are assumed to be the cornerstone of all explanations of subcultural modes. Empirical tests of the subculture of violence, therefore, must ultimately depend on evidence of value differences between persons engaged in violent behavior. Though scholars vary greatly in their conceptions of values, they agree that values are the standards that guide or determine attitudes and behavior (Williams, 1971; Parsons and Shils, 1962; Kluckhohn, 1952; Rokeach, 1973). The fundamental proposition of the subculture of violence theory is that people who exhibit violent behavior compared with those who do not, place significantly different degrees of importance on some identifiable set or subset of values.

An empirical test of any subculture of violence thesis thus requires, first, a clear definition of values and, second, its operationalization via a reliable and valid value measure. Rokeach's explication of the value concept and his technique of value measurement are particularly well suited for the present research. He defines values as a belief either about a "desired end-state of existence" such as striving for freedom, or salvation, or world peace (terminal values) or a belief about a "preferred mode of conduct" such as being honest, or courageous, or loving (instrumental values).

By way of contrast, an attitude is an organization of beliefs about a specific object (e.g., an ethnic or racial group), or about an ongoing activity or situation (e.g., selling one's house or hiring someone to fill a job vacancy). Several consequences follow from these definitions and the perspective which relates them to one another: 1) a person may have thousands of attitudes toward specific objects and situations, but relatively few values; 2) values occupy a more central position than attitudes because they are generalized internal standards that transcend specific objects or situations; 3) values determine attitudes and action, so that a change in values should lead to changes in attitudes and actions; 4) values are hierarchically organized by their relative importance to one another; and 5) a person's value system, and variations in value priorities account, in large part, for variations in atti-

tude and action (Rokeach, 1968, 1973).

Hypotheses

Two testable hypotheses follow from the subculture of violence thesis: 1) persons who vary in participation in violent behavior should also vary in their attitude toward violence — favorable attitudes toward violence being positively associated with frequent participation in violent behavior; 2) persons who vary in participation in violence should also vary in underlying value patterns — patterns that should be logically related to violent behavior. Since the subculture of violence thesis is for the most part an hypothesis about male violence, these hypotheses were tested with male samples only.

Research Procedure

The findings to be reported come from two independent studies: one on values and attitudes underlying interpersonal violence in a national area probability sample of 1,429 adult Americans over twenty-one years of age, the other on value differences among 363 men incarcerated in a Michigan prison for various felonious offenses. Data for the first study were obtained through a national Amalgam Survey administered by the National Opinion Research Center in April, 1971. The prison inmate data were collected by Cochrane (1971) in 1968 and 1969 and were kindly made available for purposes of the present study.

Violent Behavior: Participation in interpersonal violence in the national sample was ascertained by self-reports — "yes" or "no" responses to four questions:²

a. Have you ever been punched or beaten by another person?

b. Have you ever been threatened or actually cut with a knife by another person?

²These items were first used in a 1968 Violence Commission Survey. The Task Force on Violence and Media, of which the author was co-director, designed and analyzed that portion of the Survey which concerned interpersonal violence. Three separate participation indices were constructed, a victim index based on five items, an observer index also based on five items, and an assailant index based on three items. Moreover, two separate attitude indices were constructed, each based on responses to eight items (see Baker and Ball, 1969 pp. 506-14 for these items).

c. Have you ever punched or beaten another person?

d. Have you ever had to use your fists, a knife, or some other weapon to defend yourself from another person?³

These items were selected because they encompass both victim and assailant experiences and severe as well as moderate experiences of violence.

Responses to these four questions were found to be positively intercorrelated,⁴ and analyses of individual items revealed no substantial demographic or value differences. Findings to be reported will therefore be based on a single index of extent of participation in interpersonal violence. This index is obtained by counting the number of "yes" responses to the four items, ranging from 0 to 4 "yes" responses.

Participation in violent crime was assessed by a known groups procedure. All inmates incarcerated for an offense involving violence (primarily homicide, assault, armed robbery, rape, or kidnapping) were identified as participants in violent crime. Inmates serving sentences for other crimes (primarily forgery, burglary, unarmed robbery, drugs, and indecent liberties) were identified as participants in non-violent crime. Some prisoners may have been erroneously convicted, others may have committed crimes for which they were not convicted, and still others may have been convicted of lesser crimes than they committed as in the case of successful plea bargaining. Such validity problems deserve concern, but their impact on the findings of the present research should be lessened by the rather general offense categories employed.

Attitude Toward Violence: These items were used only in the national sample to ascertain respondents' attitudes toward acts of violence which were ambiguous with respect to their social legitimacy.⁵

³In the Violence Commission Survey, this item was worded somewhat differently: "Have you ever been in the situation where you had to defend yourself with a knife or gun?"

⁴Strong positive inter-item associations ranging from $r = .74$ to $.92$ were found between victim and assailant experiences.

⁵In the total sample, 69%, 54%, and 56% made approving or "yes" responses to items a, b, and c respectively. Following two of the attitude items (a and b), additional questions were asked which

Are there any situations you can imagine in which you would approve of:

a. a teenage boy punching or beating another teenage boy?

b. a public school teacher hitting a student?

c. a judge sentencing a person to death?

Again, these three items were combined into a single index ranging from 0 to 3 "yes" responses, for reasons identical to those given for using the participation index.

Values: The Rokeach Value Survey (1967) presents the respondent with reasonably comprehensive lists of human values. It can be economically used in large scale survey research, and it is a reasonably reliable and valid instrument (Rokeach, 1973). The Value Survey consists of eighteen terminal values (desired end-states of existence) and eighteen instrumental values (preferred modes of behavior). These two sets of values, and the brief definitions that were presented to respondents, are shown in Tables 1 and 2.⁶ Respondents rank these values in terms of "their importance as guiding principles in YOUR life."⁷ The median reliability with an interval of two to four months between test and retest is .76 for the terminal values and .65 for the instrumental values. The test-retest reliabilities with an interval of fourteen to sixteen months are still reasonably good — .69 for terminal values and .61 for instrumental values (Rokeach, 1973). The Value Survey takes fifteen to twenty minutes to administer. Large and statistically significant differences in

assessed the extent of provocation required before the respondent would approve (see Baker and Ball, 1969, pp. 510-14 for these items). The results of analyses based on the provocation items are very similar to those based on the three global attitude questions, and thus to preserve space are not included in this report.

⁶The values are presented on gummed labels which are alphabetically arranged on the right-hand side of the page, one page each for the terminal and instrumental values. The respondent proceeds by peeling off one value at a time and placing it in one of the eighteen boxes on the left-hand side of the page which are numbered from one, the most important value, to eighteen, the least important.

⁷The lack of independence inherent in an ipsative measurement procedure does not appear to be a significant problem for this test. The average inter-item correlation is only $r = -.06$.

values have been reported for persons varying in income, education, race, age, and political affiliation (Rokeach and Parker, 1970; Rokeach, 1973). These values have also been found to be significantly related to many different kinds of attitudes, actions, and occupational roles. Rankings of the terminal value Equality, for example, have been shown (Rokeach, 1968, 1973) to be a highly significant statistical indicator of race; political liberalism or conservatism; attitudes toward civil rights, the poor, and women's liberation (Ball-Rokeach, 1973). Importantly for the present study, the utility of the Value Survey in testing the validity of subcultural hypotheses has been demonstrated by Rokeach and Parker (1970) in their application of the "subculture of poverty" thesis to black-white and poor-rich value differences. The Value Survey was used in both the national survey and the prison study.

Attitudes and Violent Behavior: A Test of Hypothesis 1

Subcultural explanations of violence predict a positive association of pro-violent attitudes and violent behavior. With a rather large number of cases ($N = 686$), it is not surprising to find a statistically significant association between violent attitudes and behavior in the national sample ($\chi^2 = 22.63$, $p < .001$). The magnitude of the association between approval of violence and participation in interpersonal violence is small, however ($\gamma = .20$). Additional attitude indices were constructed to reflect the degree of victim provocation required for the respondent to approve of violence (see footnote 5). Measures of association between such indices and reported participation in violence were also weak (γ ranging from .05 to .17). At this national level, therefore, Hypothesis 1 receives little support.

The weak association between approval and reported participation in interpersonal violence may be due to the lack of correspondence between the kinds of violence reflected in the attitude and participation items (Blumenthal, et al., 1972). For example, favorable attitudes toward violence used for social control, indicated by responses to the capital or corporal punishment items in this research, may be poor predictors of whether one participates in violence for social change

or economic gain. We can assess this possibility better after the relation between values and violent behavior has been discussed.

Values and Violent Behavior: A Test of Hypothesis 2

Hypothesis 2 will be examined by first describing value differences among males in the national sample who report varying degrees of participation in interpersonal violence. This will be followed by a description of value differences among prison inmates.

Values and Self-reported Violence in the National Sample: Terminal and instrumental value differences between American males reporting different degrees of participation in interpersonal violence are presented in Tables 1 and 2. For each value, three types of data are reported: median rank, composite rank (in parentheses), and the significance of the differences between rankings among males who are, by their own reports, high, medium, and low in participation in violence. The composite rank is obtained by locating the position of a median rank in the ordered array of eighteen median ranks. Thus, a composite rank of 1 means that the median rank is the highest of the eighteen medians, while a composite rank of 18 means it is the lowest.

If Hypothesis 2 is valid, we should find significant value differences among American males varying with respect to reported participation in interpersonal violence. Six of the thirty-six values differentiate significantly among males who vary in the degree of reported participation in violence. Compared to males reporting no participation in violence, those who report moderate or high participation place significantly more importance on An Exciting Life, Mature Love, and being Imaginative; and they place less importance on Social Recognition, A Comfortable Life, and A World at Peace. Very similar findings occur when the three participation groups are matched on group size, income, and education, and for this reason are not reported separately.

Since approximately two significant differences at the .05 level could be expected to occur by chance with thirty-six values, the six value differences found empirically fail to provide convincing evidence for Hypothesis 2. Moreover, the six differences that are signifi-

Table 1. Terminal Value Differences between Males Varying in Participation in Violence and between Males and Females

	Degree of Participation: Males (N = 681)				Sex: Total (N = 1,430)		
	None N = 124	Moderate N = 189	High N = 368	p ^a	Male N = 687	Female N = 743	p
A comfortable life (a prosperous life)	7.3 (4)	10.7 (12)	9.2 (9)	*	9.2 (9)	11.7 (13)	**
An exciting life (a stimulating, active life)	15.0 (18)	14.9 (17)	14.2 (16)	*	14.6 (17)	15.6 (18)	**
A sense of accomplishment (lasting contribution)	10.1 (11)	9.2 (10)	9.1 (8)		9.3 (10)	9.8 (12)	
A world at peace (free of war and conflict)	2.7 (1)	3.0 (1)	3.4 (1)	*	3.2 (1)	2.6 (1)	*
A world of beauty (beauty of nature and the arts)	12.2 (15)	13.0 (15)	12.8 (15)		12.8 (15)	12.4 (14)	
Equality (brotherhood, equal opportunity for all)	8.3 (7)	7.9 (5)	8.0 (6)		8.0 (6)	7.4 (4)	
Family security (taking care of loved ones)	4.5 (2)	3.9 (2)	4.0 (2)		4.0 (2)	4.4 (2)	*
Freedom (independence, free choice)	4.9 (3)	5.7 (3)	5.0 (3)		5.1 (3)	5.5 (3)	
Happiness (contentedness)	8.3 (6)	7.6 (4)	7.5 (4)		7.6 (4)	7.8 (8)	
Inner harmony (freedom from inner conflict)	11.4 (13)	10.8 (13)	10.9 (12)		11.0 (13)	9.5 (11)	**
Mature love (sexual and spiritual intimacy)	12.1 (14)	11.8 (14)	10.9 (13)	*	11.5 (14)	12.4 (15)	*
National security (protection from attack)	8.0 (5)	9.0 (8)	9.4 (10)		9.0 (8)	8.9 (9)	
Pleasure (an enjoyable, leisurely life)	13.3 (16)	14.3 (16)	14.6 (17)		14.3 (16)	15.1 (17)	**
Salvation (saved, eternal life)	10.8 (12)	9.5 (11)	12.3 (14)		10.9 (12)	7.6 (6)	**
Self-respect (self-esteem)	9.4 (8)	8.0 (6)	7.7 (5)		7.9 (5)	7.4 (5)	
Social recognition (respect, admiration)	13.9 (17)	15.0 (18)	14.7 (18)	*	14.6 (18)	15.1 (16)	
True friendship (close companionship)	9.0 (9)	9.2 (9)	9.7 (11)		9.4 (11)	9.5 (10)	
Wisdom (a mature understanding of life)	9.8 (10)	8.7 (7)	8.8 (7)		8.9 (7)	7.7 (7)	*

^aKruskal-Wallis Test; * < .05, ** < .001.

cant are small in magnitude, the largest being only 2.0 median ranks. There remains the question of the substantive significance for the Wolfgang-Ferracuti thesis of the value differences obtained.

The concept of "machismo" implies a sexual basis for value differences wherein violence becomes a symbol or a defining characteristic of masculinity. The proposition that certain men are more violent than others

because of their greater commitment to "machismo values" can be interpreted to mean either that some men are more violent than other men because they have a stronger commitment to certain subsets of "male values" especially salient in a "machismo" life style, or because some men have a stronger commitment than others to the total American male value system.

The first version of the "machismo" hy-

Table 2. Instrumental Value Differences between Males Varying in Participation in Violence and between Males and Females

	Degree of Participation: Males (N = 681)				Sex: Total (N = 1,430)		
	None N = 124	Moderate N = 189	High N = 368	p ^a	Male N = 687	Female N = 743	p
Ambitious (hard-working, aspiring)	5.5 (2)	5.5 (2)	5.2 (2)		5.4 (2)	8.0 (6)	**
Broadminded (open-minded)	6.9 (3)	7.0 (4)	7.3 (4)		7.1 (4)	7.8 (5)	
Capable (competent, effective)	9.3 (9)	8.7 (6)	8.5 (6)		8.7 (6)	10.3 (12)	**
Cheerful (lighthearted, joyful)	10.8 (14)	10.6 (14)	11.2 (13)		11.0 (13)	9.8 (10)	*
Clean (neat, tidy)	9.7 (10)	10.5 (13)	10.1 (11)		10.2 (11)	9.0 (9)	*
Courageous (standing up for your beliefs)	8.2 (5)	8.3 (5)	7.8 (5)		8.1 (5)	8.2 (8)	
Forgiving (willing to pardon others)	8.2 (6)	8.7 (7)	9.0 (8)		8.8 (7)	5.9 (2)	**
Helpful (working for the welfare of others)	8.5 (7)	8.9 (8)	9.7 (9)		9.2 (9)	8.0 (7)	*
Honest (sincere, truthful)	3.0 (1)	4.1 (1)	3.2 (1)		3.3 (1)	3.2 (1)	
Imaginative (daring, creative)	15.9 (18)	14.6 (18)	13.9 (18)	*	14.5 (18)	15.8 (18)	**
Independent (self-reliant, self-sufficient)	10.2 (11)	9.9 (10)	9.7 (10)		9.9 (10)	10.4 (13)	
Intellectual (intelligent, reflective)	12.3 (15)	12.8 (15)	12.5 (15)		12.5 (15)	12.9 (15)	
Logical (consistent, rational)	12.9 (17)	12.6 (16)	12.9 (16)		12.8 (16)	13.9 (17)	*
Loving (affectionate, tender)	10.8 (13)	10.1 (11)	10.8 (12)		10.6 (12)	7.3 (4)	**
Obedient (dutiful, respectful)	12.3 (16)	13.6 (17)	13.7 (17)		13.4 (17)	13.2 (16)	
Polite (courteous, well- mannered)	10.6 (12)	10.5 (12)	11.7 (14)		11.2 (14)	10.8 (14)	
Responsible (dependable, reliable)	7.1 (4)	6.2 (3)	6.0 (3)		6.2 (3)	6.7 (3)	
Self-controlled (restrained, self- disciplined)	8.9 (8)	9.6 (9)	9.0 (7)		9.2 (8)	10.0 (11)	*

^aKruskal-Wallis Test; * < .05, ** < .001.

pothesis requires us to identify some subset of the thirty-six values that represents a "machismo" life style. Distinctively "machismo" values were identified with the aid of Wolfgang and Ferracutti's description of the value characteristics of the subculture of violence: "Quick resort to physical combat as a measure of daring, courage, or defense of status appears to be a cultural expression..." (p. 153); "The adult male who does not defend his honor or his female companion will be socially emasculated..." (p. 160). These descriptions suggest certain values listed in the

Value Survey as being significantly more or significantly less important in the "machismo" compared to the parent culture: more importance apparently is placed in "machismo" culture on An Exciting Life, Freedom, Pleasure, Social Recognition, being Courageous and Independent, and less importance is placed on being Forgiving.⁸ Though

⁸On the Value Survey, as shown in Table 1, the value Mature Love is accompanied by the phrase "sexual and spiritual intimacy." The present conception of "machismo" would lead one to expect

we cannot be certain that others would have come up with exactly this same list of "machismo" values, these seven value stances seem to reasonably represent the meaning of the "machismo" life style. Thus, we should expect these to differentiate significantly between males reporting high participation from those reporting low participation in interpersonal violence.

Tables 1 and 2 show that only one of the seven "machismo" values corresponds in the predicted direction to the obtained value differences between participation groups — high participation males placing more importance on An Exciting Life than low participation males. But even here, the difference between low and high participation group medians is only .8. More importantly, An Exciting Life is ranked very low in importance by all three participation groups. The only other significant difference involving a "machismo" value is in the opposite direction from that predicted, namely, high participation males place less, not more, importance on Social Recognition than do males reporting low participation in interpersonal violence.⁹ Thus, these findings do not support the first

version of the "machismo" subculture hypothesis.

Testing the second version, namely, that some men are more violent than others because of a stronger commitment to the over-all American male value system, requires identifying "male" compared to "female" values. The subculture of violence thesis would appear to predict much the same differences between men and women as between violent and non-violent males. In the 1971 national sample, there are nineteen significant value differences between males and females, as shown in Tables 1 and 2. With respect to terminal values, males place significantly more importance on A Comfortable Life, An Exciting Life, Family Security, Mature Love, and Pleasure; and they place less importance on A World at Peace, Inner Harmony, Salvation, and Wisdom. Looking at the significant instrumental value differences, males place more importance on being Ambitious, Capable, Imaginative, Logical, and Self-controlled and less importance on being Cheerful, Clean, Forgiving, Helpful, and Loving. These findings are highly consistent with the male-female value differences reported for a national sample in 1968 (Rokeach, 1973). Moreover, they are consistent with sex-role stereotypes (Broverman, et al., 1972): men seem more concerned with economic and personal success, and hedonistic pursuits; while women seem more concerned with getting along with others in this world and getting to the hereafter.

But only four of the nineteen significant value differences between males and females correspond to the significant value differences between high and low violence participation groups in the expected direction — more importance on An Exciting Life, Mature Love, and being Imaginative, and less importance on A World at Peace. Thus, these data fail to provide convincing evidence for the hypothesis that males reporting relatively high participation in violence have a strong commitment to "male values" in general.

In summary, the significant value differences found between groups differing with respect to reported participation in interpersonal violence are few in number, small in magnitude, and not consistent with either version of the "machismo" subculture of violence hypothesis. All these findings suggest that values play little or no role as determi-

"machismo" adherents to place more importance on sexual, but less importance on spiritual intimacy. It, therefore, seemed preferable to exclude Mature Love from the list of "machismo values" on the assumption that the sexual dimension would be reflected in rankings of the value Pleasure.

⁹The fact that no significant differences were found between violent and non-violent men on the five "machismo" values — Freedom, Pleasure, and being Courageous, Forgiving, and Independent — does not appear to be a result of low discriminatory power of these values. For example, women who participate in sex-role change activities compared to those who do not, place significantly more importance on Freedom and significantly less importance on Forgiving (Ball-Rokeach, 1973). In other research (Rokeach, 1973), Pleasure rankings significantly differentiated between groups varying in age, church attendance, and attitudes toward blacks; Courageous rankings significantly differentiated those with extrinsic compared to intrinsic religious orientations and between persons having different attitudes toward church activism; Independent rankings significantly differentiated between subjects expressing pro- and anti-communist attitudes, favorable and unfavorable attitudes toward the poor, and favorable and unfavorable attitudes toward religion. Thus, independent evidence from other research suggests the construct and predictive validity of the values identified here as "machismo" values.

nants of interpersonal violence in a national representative sample of adult male Americans.

Values and Violent Crime

A more stringent test of Hypothesis 2, it may be contended, requires a comparison of the values of males independently known to vary in their involvement in crimes of violence, rather than self-reports of participation in interpersonal violence. Such a test is provided by a value comparison of males imprisoned for violent and non-violent crimes. Table 3 shows the terminal value differences between these two groups and Table 4 the comparable instrumental value differences.

Of the thirty-six values, none significantly differentiates between inmates incarcerated for violent as opposed to non-violent crimes. Moreover, the correlation between the composite ranks of the violent and non-violent inmates is $\rho = .97$ for the terminal values, and $\rho = .95$ for the instrumental values. These are unusual findings, since in numerous other comparisons of the values of various groups based on the Value Survey, the average number of significant differences is eleven or twelve (Rokeach, 1973).

Wolfgang and Ferracutti focus their argument concerning a subculture of violence mainly around the case of homicide. It would therefore be interesting to know whether the values of inmates imprisoned for homicide ($N = 57$) differ significantly from the values of inmates convicted of all other crimes ($N = 302$). A comparison of these two groups reveals that inmates convicted of murder differ significantly from the other inmates only in their rankings of two of the thirty-six values, an outcome no better than chance. Thus, there is no evidence that the value systems of murderers differ from the values of other inmates, or that the value systems of violent prison inmates differ from those of non-violent inmates.

It might be argued that this absence of differences results from value congruence occurring after imprisonment; that is, the differences result from conformity to a prison subculture. This possibility is unlikely. Cochrane states:

The possibility existed that many of the differences between the prison and control groups were a function of imprisonment and prison experience rather than a basic

difference in value systems. To check this possibility a comparison was made between the value systems of men who had been in prison for less than one month and those with more than one month's prison experience. There was no substantial difference between these two groups (Cochrane, 1971, p. 77).

Moreover, Cochrane's data show no substantial differences in values between inmates having a minimum sentence of less as compared to more than ten years, or between inmates who had spent less as compared to more than ten years in prison, or between recidivists and first offenders. These findings consistently argue against the proposition that the non-significant value differences between violent and non-violent offenders or between convicted murderers and all other inmates can be attributed to post-crime socialization into the values of a prison subculture.

Supplementary Findings

In the national sample, socio-economic status was found to be unrelated to participation in interpersonal violence: the association between education and reported participation in interpersonal violence was $\gamma = .04$ and between income and participation, $\gamma = .07$. Somewhat larger associations were obtained between education and approval of violence ($\gamma = .22$) and between income and approval ($\gamma = .24$). These findings replicate the more extensive Violence Commission associations between socio-economic status on the one hand and violent attitudes and behavior on the other (Baker and Ball, 1969).

Perhaps middle as compared to lower class persons have relatively rare encounters with violence and are, therefore, more likely than the latter to recall such experiences. Such a line of argument is, however, inconsistent with the fact that a sizeable majority of all respondents who reported experience with a violent act, with the exception of victimization involving a knife, reported having had more than one experience with that act.¹⁰ More-

¹⁰A crude measure of frequency (Did that happen just once or more than once?) followed each participation item. Of those respondents reporting that they participated in violence, 81 percent said they had been punched or beaten more than once, 32 percent said they had been threatened or cut with a knife more than once, 80 percent said they

Table 3. Violent and Non-violent Prisoners: Terminal Values

	Violent (N = 157)		Non-violent (N = 202)	
	Median	Rank	Median	Rank
A comfortable life	6.81	(5)	7.77	(7)
An exciting life	13.78	(16)	12.60	(14)
A sense of accomplishment	8.04	(7)	7.41	(6)
A world at peace	8.57	(9)	9.40	(10)
A world of beauty	14.41	(17)	14.50	(17)
Equality	8.13	(8)	9.10	(8)
Family security	4.20	(1)	5.03	(2)
Freedom	5.27	(2)	5.38	(3)
Happiness	6.24	(4)	6.75	(5)
Inner harmony	10.23	(12)	9.59	(11)
Mature love	9.54	(10)	9.81	(12)
National security	13.47	(15)	14.09	(16)
Pleasure	13.12	(14)	12.63	(15)
Salvation	15.68	(18)	14.58	(18)
Self-respect	7.08	(6)	6.40	(4)
Social recognition	12.00	(13)	12.03	(13)
True friendship	9.61	(11)	9.39	(9)
Wisdom	5.45	(3)	4.81	(1)

Table 4. Violent and Non-violent Prisoners: Instrumental Values

	Violent (N = 157)		Non-violent (N = 202)	
	Median	Rank	Median	Rank
Ambitious	6.00	(1)	6.17	(2)
Broadminded	7.25	(5)	7.03	(5)
Capable	7.89	(6)	8.22	(7)
Cheerful	11.68	(15)	12.29	(15)
Clean	8.19	(7)	7.00	(4)
Courageous	9.00	(9)	8.45	(9)
Forgiving	9.69	(10)	10.39	(12)
Helpful	10.42	(12)	10.35	(11)
Honest	6.63	(2)	5.87	(1)
Imaginative	14.66	(18)	14.70	(18)
Independent	8.71	(8)	8.38	(8)
Intellectual	11.00	(13)	9.30	(10)
Logical	13.11	(16)	13.43	(16)
Loving	11.43	(14)	10.97	(13)
Obedient	13.13	(17)	13.77	(17)
Polite	10.08	(11)	11.29	(14)
Responsible	6.81	(3)	7.45	(6)
Self-controlled	7.00	(4)	6.81	(3)

Table 5. Terminal Value Differences between Groups Varying in Education
(N = 1,428)

	0-4 Years	5-8 Years	Some High School	Complete High School	Some College	Complete College	Graduate School	p ^a
	N = 51	N = 257	N = 290	N = 444	N = 216	N = 105	N = 65	
A comfortable life	7.0(5)	7.5(5)	9.0(10)	11.0(13)	12.3(14)	14.6(16)	13.7(15)	**
An exciting life	15.4(18)	15.1(18)	15.8(18)	15.4(18)	15.5(17)	13.8(14)	13.0(14)	**
A sense of accomplishment	12.0(12)	11.0(12)	10.1(12)	9.6(10)	8.0(7)	7.7(8)	6.2(3)	**
A world at peace	2.6(1)	2.6(1)	2.7(1)	2.8(1)	3.3(1)	3.4(1)	4.4(1)	*
A world of beauty	13.9(16)	12.7(14)	12.9(14)	12.5(15)	12.7(15)	11.3(12)	10.9(12)	
Equality	8.4(10)	8.3(7)	8.3(9)	7.4(5)	6.8(5)	7.4(5)	6.3(5)	
Family security	5.1(3)	4.2(2)	3.6(2)	3.9(2)	4.3(2)	4.8(2)	5.5(2)	*
Freedom	4.6(2)	5.3(3)	5.4(3)	5.2(3)	5.4(3)	5.3(3)	6.7(7)	
Happiness	7.7(6)	6.6(4)	7.9(5)	7.4(4)	8.1(8)	8.4(9)	9.4(11)	
Inner harmony	13.1(13)	12.4(13)	10.2(13)	10.2(11)	9.1(9)	7.6(7)	8.4(8)	**
Mature love	13.6(15)	13.3(15)	13.4(15)	11.5(14)	11.3(13)	9.7(11)	8.9(9)	**
National security	7.9(7)	8.9(10)	7.9(4)	8.1(7)	9.4(11)	11.9(13)	12.4(13)	**
Pleasure	14.1(17)	13.6(16)	15.1(17)	14.8(16)	15.2(16)	14.2(15)	15.6(18)	*
Salvation	5.1(4)	8.5(8)	8.2(8)	8.8(9)	10.4(12)	14.9(17)	14.3(16)	**
Self-respect	8.3(9)	8.0(6)	8.1(7)	7.5(6)	6.7(4)	7.3(4)	6.2(4)	
Social recognition	13.1(14)	14.0(17)	14.4(16)	15.1(17)	15.8(18)	15.6(18)	14.6(17)	**
True friendship	8.1(8)	8.6(9)	9.9(11)	10.3(12)	9.3(10)	9.1(18)	9.3(10)	**
Wisdom	11.6(11)	9.4(11)	8.1(6)	8.3(8)	7.1(6)	7.6(6)	6.7(6)	**

^a χ^2 Median Test; * < .05, ** < .001.

over, the data do not indicate that respondents, whether middle or lower class, were less willing to report participation as an assailant as opposed to being a victim of violence.¹¹

had punched or beaten another person more than once, and 76 percent said they had had to defend themselves with fists, knife, or some other weapon more than once.

¹¹ Out of the total N of 1,428, 46 percent report having been punched or beaten, 40 percent report having punched or beaten another person, 15

In contrast to the negligible relationships between violence and indicators of social class, the Value Survey was found to be a particularly sensitive indicator of social class differences. As shown in Tables 5 and 6, twenty-seven values differentiated significantly between respondents in the national

percent report having been threatened or cut with a knife, and 37 percent report having had to use their fists, a knife, or some other weapon to defend themselves.

Table 6. Instrumental Value Differences between Groups Varying in Education (N = 1,428)

	0-4 Years	5-8 Years	Some High School	Complete High School	Some College	Complete College	Graduate School	p ^a
	N = 51	N = 257	N = 290	N = 444	N = 216	N = 105	N = 65	
Ambitious	8.1(7)	6.4(3)	6.8(4)	6.4(3)	6.5(4)	10.1(12)	9.3(10)	*
Broadminded	8.8(9)	8.3(8)	7.9(7)	7.5(4)	6.4(3)	5.7(3)	6.4(3)	*
Capable	9.3(10)	10.3(12)	9.8(10)	9.4(10)	9.2(8)	9.0(8)	8.4(7)	
Cheerful	8.2(8)	9.2(9)	10.3(13)	10.5(12)	11.6(14)	10.9(14)	13.4(15)	**
Clean	5.0(3)	7.5(5)	7.8(5)	9.7(11)	12.0(15)	14.8(17)	14.9(17)	**
Courageous	10.6(13)	7.2(4)	7.8(6)	8.3(7)	8.7(7)	8.6(6)	7.6(5)	
Forgiving	5.0(2)	5.5(2)	6.7(3)	8.0(5)	8.7(6)	9.3(9)	10.1(13)	**
Helpful	7.4(4)	7.6(6)	8.5(8)	8.9(9)	9.7(10)	9.5(10)	9.2(9)	*
Honest	4.6(1)	3.6(1)	3.3(1)	3.0(1)	3.0(1)	3.4(1)	4.1(1)	
Imaginative	15.9(18)	15.7(18)	15.9(18)	15.6(18)	14.7(17)	12.0(15)	9.9(12)	**
Independent	10.4(12)	9.8(11)	10.4(14)	10.9(14)	9.7(11)	8.4(5)	7.9(6)	*
Intellectual	14.6(16)	14.5(16)	13.6(16)	12.9(15)	11.2(13)	9.9(11)	7.3(4)	**
Logical	15.4(17)	15.2(17)	14.1(17)	13.3(17)	10.7(12)	10.3(13)	9.8(11)	**
Loving	7.9(5)	9.2(10)	9.2(9)	8.1(6)	9.6(9)	7.7(4)	11.0(14)	
Obedient	10.7(14)	11.9(15)	12.0(15)	12.9(16)	15.2(18)	16.3(18)	15.9(18)	**
Polite	9.9(11)	10.5(13)	10.1(12)	10.7(13)	12.6(16)	12.7(16)	13.4(16)	**
Responsible	8.0(6)	7.9(7)	6.7(2)	6.1(2)	5.7(2)	5.2(2)	5.7(2)	**
Self-controlled	11.8(15)	11.4(14)	10.0(11)	8.9(8)	7.8(5)	8.9(7)	9.0(8)	**

^a χ^2 Median Test; * < .05, ** < .001.

sample who varied in education; and, as shown in Tables 7 and 8, twenty-four of the thirty-six values differentiated significantly between respondents varying in income.

Nineteen of the value differences between education groups differentiate income groups in the same direction. In contrast to medium and high education and medium and high income groups, low education and low income groups place significantly more importance on A Comfortable Life, A World at Peace, National Security, Salvation, Social Recognition, and being Clean, Forgiving, Helpful, Obedient, and Polite. They place less importance on A Sense of Accomplishment, Inner Harmony,

Mature Love, Wisdom, and being Imaginative, Independent, Intellectual, Responsible, and Self-controlled.

These 1971 findings closely replicate Rokeach and Parker's (1970) national NORC findings obtained in 1968. Thus, the 1968 and 1971 data suggest that it is meaningful to speak of class bound subcultures. A question now arises as to whether the values which differentiate among social classes also distinguish among groups reporting different degrees of participation in interpersonal violence.

Not one of the values which differentiate males who report high participation in inter-

Table 7. Terminal Value Differences between Groups Varying in Income
(N = 1,410)

	Under \$2,000	\$2,000- 3,999	\$4,000- 5,999	\$6,000- 7,999	\$8,000- 9,999	\$10,000- 14,999	\$15,000 and over	^a p
	N = 107	N = 172	N = 196	N = 186	N = 186	N = 338	N = 225	
A comfortable life	7.9(6)	8.9(9)	8.9(9)	9.0(9)	11.7(14)	11.6(14)	12.3(13)	**
An exciting life	14.8(18)	15.8(18)	15.6(18)	15.8(18)	14.8(16)	15.6(18)	14.0(16)	**
A sense of accomplishment	11.1(12)	11.0(13)	9.4(12)	9.2(10)	9.8(12)	9.6(10)	7.8(7)	**
A world at peace	2.6(1)	2.4(1)	2.6(1)	2.8(1)	2.7(1)	3.4(1)	3.4(1)	*
A world of beauty	12.4(13)	12.7(14)	12.6(14)	12.9(15)	12.7(15)	12.2(15)	12.6(14)	
Equality	8.9(9)	7.2(4)	7.2(4)	7.5(5)	7.6(6)	7.9(6)	7.8(6)	
Family security	5.0(3)	4.8(2)	4.2(2)	3.7(2)	4.2(2)	3.7(2)	4.7(2)	*
Freedom	4.7(2)	5.0(3)	6.0(3)	5.5(3)	5.5(3)	5.2(3)	5.4(3)	
Happiness	6.6(4)	7.6(7)	8.4(7)	7.3(4)	8.2(7)	7.4(5)	7.9(8)	
Inner harmony	12.8(14)	10.5(12)	10.8(13)	10.3(12)	9.6(11)	9.4(9)	9.2(9)	**
Mature love	13.3(16)	13.3(15)	13.5(15)	12.3(14)	11.1(13)	10.7(13)	11.0(12)	**
National security	9.8(11)	7.4(5)	7.8(5)	8.7(8)	8.5(8)	9.7(11)	10.5(11)	*
Pleasure	13.9(17)	14.7(16)	14.4(16)	15.1(17)	14.9(17)	14.8(16)	14.7(17)	
Salvation	7.7(5)	7.4(6)	8.9(10)	8.3(7)	8.8(9)	9.3(8)	13.9(15)	**
Self-respect	8.0(8)	7.8(8)	8.2(6)	8.1(6)	7.6(4)	7.3(4)	7.0(4)	
Social recognition	13.2(15)	13.5(16)	14.7(17)	14.6(16)	15.2(18)	15.4(17)	15.4(18)	**
True friendship	7.9(7)	9.3(11)	9.1(11)	10.5(13)	9.5(10)	9.8(12)	9.5(10)	*
Wisdom	9.1(10)	9.0(10)	8.9(8)	9.4(11)	7.7(5)	8.1(7)	7.0(5)	*

^a χ^2 Median Test; * < .05, ** < .001.

personal violence from those who report low participation corresponds to the nineteen "lower class values" identified above. This finding coupled with the consistent finding of a negligible relationship between socioeconomic status and reported participation in violence, indicate that membership in the lower class by virtue of one's income, education, or value system does not increase the

likelihood of participation in interpersonal violence.

Discussion

The relatively weak association between attitudes and violent behavior (Hypothesis 1) taken together with the fact that there is little or no evidence of a relationship between values and violent behavior (Hypothesis 2),

Table 8. Instrumental Value Differences between Groups Varying in Income
(N = 1,410)

	Under \$2,000	\$2,000- 3,999	\$4,000- 5,999	\$6,000- 7,999	\$8,000- 9,999	\$10,000- 14,999	\$15,000 and over	^a
	N = 107	N = 172	N = 196	N = 186	N = 186	N = 338	N = 225	
Ambitious	7.8(5)	8.2(7)	6.3(2)	6.8(4)	6.8(4)	6.5(3)	6.8(4)	
Broadminded	8.7(8)	8.5(9)	7.8(6)	7.6(5)	6.6(3)	7.2(4)	6.6(3)	
Capable	10.3(12)	11.2(12)	9.6(10)	9.7(11)	10.0(12)	9.3(9)	8.5(6)	*
Cheerful	8.2(7)	10.1(11)	10.6(13)	10.2(12)	10.0(11)	10.7(12)	11.7(14)	
Clean	6.4(3)	6.9(3)	8.3(8)	9.1(9)	9.7(10)	10.8(13)	12.8(16)	**
Courageous	8.8(10)	8.0(6)	8.2(7)	8.1(7)	7.5(5)	8.8(7)	7.5(5)	
Forgiving	4.6(1)	6.7(2)	7.0(4)	6.9(4)	7.8(6)	7.4(5)	9.5(10)	**
Helpful	7.7(4)	7.5(5)	7.6(10)	9.6(10)	8.6(7)	9.7(10)	9.2(8)	*
Honest	4.8(2)	2.9(1)	4.3(1)	2.8(1)	3.2(1)	3.0(1)	3.4(1)	*
Imaginative	15.0(17)	15.9(18)	15.7(18)	15.4(18)	15.5(18)	15.0(18)	13.5(17)	*
Independent	9.9(11)	10.5(14)	10.5(12)	11.1(14)	10.7(13)	10.0(11)	8.9(7)	
Intellectual	13.3(16)	13.8(16)	12.8(16)	13.9(16)	13.2(15)	12.0(16)	10.6(12)	
Logical	15.1(18)	14.6(17)	14.1(17)	14.8(17)	13.5(16)	12.0(15)	11.0(13)	**
Loving	7.8(6)	8.4(8)	9.7(11)	7.7(6)	8.9(8)	8.8(8)	9.3(9)	
Obedient	12.4(15)	11.5(15)	12.6(15)	12.2(15)	13.7(17)	13.7(17)	15.7(18)	**
Polite	10.8(13)	8.7(10)	10.8(14)	10.4(13)	11.1(14)	11.5(14)	12.8(15)	**
Responsible	8.7(9)	7.4(4)	7.0(3)	6.1(2)	6.2(2)	6.3(2)	5.3(2)	**
Self-controlled	11.6(14)	10.3(13)	9.3(9)	8.9(8)	9.6(9)	8.6(6)	9.8(11)	*

^a χ^2 Median Test; * < .05, ** < .001.

suggests that the subculture of violence thesis is, at best, incomplete and at worst invalid as an explanation of violent behavior. This conclusion applies to both interpersonal violence and violent crime.¹²

A plausible interpretation of these findings is that values and attitudes are relatively unrelated to violent behavior because violence is primarily interpersonal rather than intra-

personal. Values and attitudes are intra-personal in the sense that one person can have and express them. In contrast, the types of violent behavior examined in this research require some interaction between at least two persons. Thus, one ought not to expect a causal connection between values and violent behavior or between attitudes and violent behavior when the values and attitudes of only one interacting party are taken into account. This interpretation suggests a line of research which regards violence as neither victim-precipitated nor assailant initiated. The dynamics giving rise to violent encounters may not involve clearly defined victim or assailant roles and may be better explained as an emergent property of interaction (Cohen and Short, 1971). In other words, participation in violence is some interacting function

¹² There is little correspondence between the values which differentiate between males varying in participation in violence and those which differentiate between female participation groups. Females reporting high as opposed to low and moderate participation place significantly more importance on Equality, Freedom, Wisdom, and being Logical, and less importance on Salvation and being Capable. There are significant but non-directional differences on Mature Love and being Imaginative.

of the values and attitudes of all parties involved.

An equally plausible interpretation is that values and attitudes have little direct influence on violent behavior. Violent behavior may be largely determined by such situational, ecological, or demographic factors as access to weapons, exposure to alcohol, the rate of crime and drug use in the immediate environment, population density, or level of inter-group conflict.

A final methodological comment is in order. Self-reports about participation in interpersonal violence in the national sample are somewhat suspect; the "have you ever" format of the questions is neither time- nor situation-specific and is confounded with age. Other objections can perhaps be raised about the objective assessment of participation in violence by survey methods. But such criticisms cannot be raised when the values of known violent and non-violent groups are being compared.

The known groups and self-report methods lead to similar conclusions, namely, lack of support for the proposition that "violent men" have a stronger commitment to "machismo" values. More generally, values do not distinguish reliably between "violent" and "non-violent" males. We conclude that we must therefore look elsewhere to explain violent behavior.

REFERENCES

- Baker, R., and S. Ball.
1969 *Violence and the Media*. Washington D.C.: U.S. G.P.O.
- Ball-Rokeach, S.
1972 "The legitimization of violence." Pp. 100-11 in J. Short, Jr. and M. Wolfgang (eds.), *Collective Violence*. Chicago: Aldine-Atherton.
- Ball-Rokeach, S.
1973 "Values underlying receptivity to sex-role change." Paper read at the American Association for the Advancement of Science - Conacyt international meeting, Mexico City, July.
- Blumenthal, M., R. Kahn, F. Andrews, and K. Head.
1972 *Justifying Violence: Attitudes of American Men*. Ann Arbor: Institute for Social Research, University of Michigan.
- Broverman, I. K., S. R. Vogel, D. M. Broverman, F. E. Clarkson, and P. S. Rosenkrantz.
1972 "Sex-role stereotypes: a current appraisal." *Journal of Social Issues* 28 (No. 2): 59-78.
- Cochrane, R.
1971 "The structure of value systems in male and female prisoners." *British Journal of Criminology* 11 (January): 73-9.
- Cohen, A. K., and J. F. Short, Jr.
1971 "Crime and juvenile delinquency." Pp. 88-146 in R. K. Merton and R. A. Nisbet, *Contemporary Social Problems*, 3rd edition. Harcourt, Brace and World, Inc.
- Kluckhohn, C.
1952 "Values and value orientations in the theory of action." Pp. 388-433 in T. Parsons and E. Shils (eds.), *Toward a General Theory of Action*. Cambridge: Harvard University Press.
- Parsons, T., and E. Shils.
1962 "Systems of value-orientation." Pp. 159-89 in T. Parsons and E. Shils (eds.), *Toward a General Theory of Action*. New York: Harper and Row.
- Rokeach, M.
1967 *Value Survey*. Sunnyvale, California: Hलगren Tests.
1968 *Beliefs, Attitudes, and Values: A Theory of Organization and Change*. San Francisco: Jossey-Bass.
1973 *The Nature of Human Values*. New York: Free Press.
- Rokeach, M., and S. Parker.
1970 "Values as social indicators of poverty and race relations in America." *The Annals of the American Academy of Political and Social Science* 388 (March): 97-111.
- Williams, R.
1971 "Change and stability in values and value systems." Pp. 123-59 in B. Barber and A. Inkeles (eds.), *Stability and Social Change*. Boston: Little, Brown and Company.
- Wolfgang, M., and F. Ferracutti.
1967 *The Subculture of Violence: Towards an Integrated Theory in Criminology*. New York: Tavistock.

ENVIRONMENT, TECHNOLOGY, AND THE ADMINISTRATIVE INTENSITY OF MANUFACTURING ORGANIZATIONS*

John Henry Freeman
University of California, Riverside

American Sociological Review 1973, Vol. 38 (December):750-63

Data from forty-one manufacturing organizations produced no support for the position that variations in administrative intensity may be attributed to scale factors. However, technological and environmental variables did correlate with administrative intensity. In addition, interaction effects were observed which were consistent with hypotheses stressing constraints on structural change imposed from outside the organization and the vulnerability to environmental disruption produced by complex technologies.

An often studied dimension of organizational structure is administrative intensity. As defined by Pondy (1969:47), the term, "administrative intensity," denotes "the number of managers, professionals and clerical workers divided by the number of craftsmen, operatives, and laborers employed by the organization." It was used to identify a long series of studies in which the primary dependent variable was either the ratio of administrators to production workers or the percent in administration. Other writers have referred to this subject with such phrases as "the relative size of the supportive component" (Haas et al., 1963), "administrative-production ratios" (Hendershot and James, 1972), and "the administrative ratio" (Blau and Schoenherr, 1971). The concepts used to define these terms, such as "administrator" and "production worker," are themselves defined in widely varying ways (Rushing, 1967).

Administrative intensity has been viewed as pertaining to a rather broad range of theoretical issues. Among them are overhead costs (Melman, 1951 and 1956), the process of bureaucratization (Bendix, 1956), the trend toward monopolization (Starbuck, 1965), and the problem of resource allocation as it relates to decision processes (Pondy, 1969). Most commonly, however, administrative intensity has been studied as a reflection of coordination problems.

Based on mathematical work by Graicunas

(1933), Bossard (1945), and Kephart (1950), first Terrien and Mills (1955) and then Caplow (1957) developed the hypothesis that administrative intensity increases with (population) size. The number of possible social relationships increases as an exponential function of size.¹ Since administrators are responsible for providing coordination,² and since coordination becomes more difficult as the number of social relationships increases,³ it follows that the size of the administrative component increases disproportionately as the organization grows — increases in size lead to increases in administrative intensity.

This was the line of reasoning followed by Terrien and Mills (1955). Their data seemed to support it, but subsequent studies produced negative findings. With the exception of Raphael (1965), no other researcher has found a positive association using cross-sectional data. Her study is only roughly comparable since the measure of administrative intensity she used was level of the hierarchy at which certain decisions were reputed to be

¹ The proportion of possible social relationships actually observed would be variable and almost certainly less than unity, however. The smaller this proportion, the more highly structured the unit as each relationship *not* observed could be viewed as part of a boundary.

² The argument ignores the fact that coordination may occur as a result of what Blau and Scott (1962: 176-83) have called "impersonal mechanisms of control," and may result from the activities of non-administrators.

³ This part of the argument suggests that the relationship between secretary and secretary poses a coordination problem as severe as that posed by the relationship between a design engineer and a shop foreman.

*I would like to thank Richard L. Simpson for the guidance he provided during the entire course of this project. Edgar W. Butler, Richard H. Hall, Michael T. Hannan, and Fred W. Reed read early drafts. Their help is gratefully acknowledged.

made, not a staff ratio. Since 1955 most researchers have found that size is inversely related to administrative intensity. Curvilinear functions are often reported. To circumvent this, some researchers sacrifice information by using rank-order measures of association (Haas, Hall and Johnson, 1963; Rushing, 1967) and others study the effects of log size (Indik, 1964; Hawley et al., 1965; Pondy, 1969; Blau, 1970). While cross-sectional studies regularly report inverse associations, longitudinal research has yielded highly inconsistent results. Melman (1951 and 1956) and Bendix (1956:211-22) found that both size of average establishment and administrative intensity increased over time for national data aggregated to the industrial level. Haire (1959) studied four manufacturing organizations in which size and percent top and middle managers decreased over time, while percent clerk increased. Chester (1961) found that his European manufacturing firms grew during the 1950's, and became more administratively intense.

Studies of governmental organizations and voluntary associations also produced inconsistent findings. Tsouderos (1955) found that while membership in his sample of voluntary associations first increased and then decreased, administrative staff increased monotonically. In the figure he presents (1955:208) it appears that administrative intensity and size were inversely correlated. The voluntary associations studies by Akers and Campbell (1970) displayed a tendency toward increases in membership and decreases in administrative intensity between 1949 and 1964.⁴ Parkinson (1957) reported that although dockyard workers increased by only 10% (1914 to 1928), and the number of men in the Royal Navy declined, administrative intensity increased dramatically. In their study of Canadian school districts, Holdaway and Blowers (1971) found that size generally increased over a five-year period, but there was no consistent tendency for administrative intensity to increase or decrease.⁵ And in the study conducted by Hendershot and James

(1972) enrollment growth in U.S. school districts was inversely associated with administrative intensity (supervisors and principals to teachers). In addition, the rate of growth had an effect. Rapid growth increased, while slow growth rates decreased the level of administrative intensity.

Why should longitudinal studies report such inconsistent results while cross-sectional studies repeatedly show inverse relationships? One possible answer is to be found in the variable definitions.⁶ When administrative intensity is defined as the A/P ratio or as percent in administration [$100 \cdot A/(A+P)$] and size is defined as A+P or as P, correlations are to be expected *even if A and P are random variables*. Further, when P is the definition of size, this relationship is negative and decelerating (i.e., it flattens out as P increases). When A+P is the definition of size, the relationship can be either positive and linear or negative and decelerating. The nature of the function as well as the magnitude of the correlation depends on the frequency distributions of A and P, particularly on their relative variances. Given that the mean and variance of P are quite likely to be larger than the comparable statistics for A in any sample one would draw in practice, one would expect a negative decelerating function.⁷ Cross-sectional studies usually define administrative intensity and size this way. Longitudinal studies vary considerably with regard to variable definition and analytic technique. For example, in plotting A/P against P for 1937, Melman found the expected negative decelerating function (1951:100). When percent change in A/P was plotted against percent change in P (1899 to 1937) the same function was observed (1951:108). But when A, P, and A/P are plotted against year, all three increase (1951:98-9). The consistency of the cross

⁶It should be noted that all explanations of which this writer is aware focus on demand for administration and production workers. Supply factors may be of equal importance when samples are heterogeneous (and the skill mixture of administrators varies from organization to organization) and especially when the data are longitudinal.

⁷These arguments are developed in more detail elsewhere. See Freeman and Kronenfeld (1973). More general treatments of dependencies built into analyses through definitions in which components appear redundantly are presented in Schuessler (1973), Fuguitt and Lieberman (1972).

⁴This analysis assumes that voluntary association members are somehow analogous to production workers, which is more than a little tenuous.

⁵Here teachers are production worker analogues. The administrative component includes secretaries and clerks as well as supervisory personnel.

sectional studies may be due to the common definitional dependency. The inconsistency of the longitudinal studies may result from the fact that some definitions involve these dependencies while others do not.

After describing the data gathered for this study, we investigate the size-administrative intensity hypothesis. We then develop additive and interaction models in which technology and environment are taken as sources of causation. While we do not ignore coordination problems, our emphasis is on the multiplicity of functions administrators perform and on the constraints which limit structures developed to carry them out.

DATA

In the Spring of 1971 a survey of manufacturing establishments was carried out in a southern California city (population, circa 150,000). Organizations with forty or more employees which had been classified under "manufacturing" by the Bureau of the Census were studied. Cooperation was surprisingly good. Interviews were successfully completed in thirty-eight of the fifty-three firms listed (72%). Three others were added from nearby areas producing a sample size of forty-one.⁸

Interviewing was conducted by undergraduate students who had pretest experience. Completed schedules were evaluated for internal consistency and several firms were called by telephone to check validity. In addition, the students wrote term papers on the organizations in which they interviewed, and these were compared with the schedules. Survey data were checked against information supplied in a Chamber of Commerce listing.

Limitations on measurement were imposed by the inexperience of the interviewers. Measures had to be kept simple and the variables selected for measurement were confined to rather gross structural characteristics. Informal relations within the organizations were not studied, and the schedule was written in such a way as to avoid topics which were

considered likely to arouse resistance (such as questions about conflicts and power relations within the organizations).

Aside from the obvious benefit of cost reduction, the use of student interviewers simplified the problem of gaining access to the firms. Businessmen in the area seemed to feel strongly that they were misunderstood by many college students and welcomed the opportunity to explain their businesses and activities. Although the writer was rarely refused access to firms selected for study, several of the executives mentioned that they were agreeable only because it was a student project.

THE EFFECTS OF SIZE

Given the dependency built into the usual definitions of size and administrative intensity, some reformulation seemed necessary. In one of the early articles on this subject, Baker and Davis (1954) regressed number of administrators on number of production workers. The theoretical issue addressed by such a procedure is whether a given increase in P is matched by a proportionate increase in A. This may be assessed by seeing whether or not the A-intercept is equal to zero. If it is not, any linear regression indicates a disproportionate relationship.

In this study, production workers (P) are employees who physically change the form or location of materials or products plus maintenance personnel. This definition comes close to those used by Rushing (1967) and Pondy (1969); and, since administrators (A) are all other personnel, it matches fairly well the more generally useful distinction suggested by Haas, Hall and Johnson (1963) between the "direct" and "supportive" components. The correlation between A and P is quite high ($r = .98$) and the A - intercept is close to zero, 3.90. Since some of the organizations had

⁸ Only two firms refused to permit interviewing. Thirteen others were not studied because of limited manpower. Most of these had less than one hundred employees. The three firms added from outside the city limits were large firms. These sampling decisions were made in an effort to reduce the skew on size. Given the nonprobability sampling, and the geographically restricted population, tests of significance were deemed inappropriate.

⁹ The confidence interval for the intercept is ± 32.6 ($p > .95$) which easily encompasses zero. However, the sampling and positive skew for both A and P preclude any rigorous interpretation. A somewhat different procedure is based on the regression of $\log A$ and $\log P$. Since b for this regression is less than zero ($b = .84$), there is also some indication of a disproportionate relationship using this mode of analysis. As with the regression based on untransformed variables, however, the difference would not be statistically significant (.05 level) if the assumptions for such a test were met.

more than 2,000 administrators, the difference seems negligible.⁹ However, it should be remembered that these data are cross-sectional but the theory is dynamic. That is, the theory pertains to organizational growth (and decline). A number of writers have cautioned against inferring growth processes from cross-sectional studies of size (Haire, 1959:292; Meyer, 1972).

This method of analysis does imply a theoretical reformulation. It precludes the possibility that increases in A produce coordination problems which lead to subsequent increases in A, a position which the original hypothesis seems to imply. The obverse, that increases in A lead to economies of scale which reduce the need for administrators, is present (apparently) in hypotheses which predict inverse relationships between administrative intensity and size (for example, Blau, 1970; Indik, 1964). However, such implied feedback loops indicate that the theories cannot be tested as stated with the usual cross-sectional data. We are obliged to ignore them even if some other reformulation is desired.

The conclusion to be derived from this section is that for these data neither economies of scale nor diseconomies of scale are manifested. The general idea suggested by Terrien and Mills (1955) and others remains open to question.

ADDITIVE MODEL

If variations in administrative intensity cannot be attributed to scale factors, how can they be explained? A first step toward an answer may be based on the observation that not all administrators direct their efforts toward securing coordination. And of those who are concerned with it, coordination is rarely *all* they do.

Production Technology

Several researchers have observed that characteristics of the production process are correlated with administrative intensity. Lawler (1947) found that the percent of employees whose principal duties involved record keeping was positively correlated with a technology scale constructed from the following variables: number of product lines or models, number of parts in the finished product, and number of operations in the production pro-

cess.¹⁰ Bendix (1956:223) presented a passage from the German census of 1933 in which higher levels of administrative intensity are attributed to mechanization.¹¹ Chester (1961:62) found that, over time, firms in the more technically advanced chemicals industry displayed greater increases in administrative intensity than the metals fabrication firms.

Woodward (1965) and Zwerman (1970) studied manufacturing establishments. Their most important independent variable was a scale of technical complexity. This scale was broken down into two major divisions: the production of "integral products" (objects) and the production of "dimensional products" (liquid, gaseous and crystalline substances). Within each of these categories, differentiation was based on production characteristics (Woodward, 1965:37-40). Both studies produced positive associations between this scale and administrative intensity.

In the research conducted by the Aston Group (Hickson et al., 1969), a number of findings did not agree with those of Woodward and Zwerman. However, their data support the general argument that more highly complicated production systems have higher levels of administrative intensity. Their scale of workflow integration was comprised by four subscales: automaticity, workflow rigidity, interdependence of workflow segments, and specificity of evaluation of operations (1969:382-3).

Given the empirical support for the proposition, there is a surprising paucity of theory pertaining directly to the relationship between technology and administrative intensity. Perhaps this is because most studies have been directed toward assessing the impact of technology on a diverse set of variables only some of which involve administrative intensity.

We focus here on the mechanization of

¹⁰ Each of these variables figures in the technology variables used by others. Notice the inclusion of *product* as well as *production process* (workflow) characteristics.

Lawler's data also show that his technology index interacts with size. The more complicated the technology, the greater the effects of size (1947:12). Of course, this may be an artifact of the dependency between size and percent record-keeper.

¹¹ Like Lawler, Bendix views product complexity as an important factor apart from characteristics of the production process (1956: 223).

production systems. Variations in mechanization appear to underly Woodward's (1965:3-44) scale of technical complexity (large batch and mass seem more highly mechanized than unit and small batch; process more than large batch). As mechanization increases, long-range planning becomes more important. The increased rate of production it creates and the increased capital investment it represents impose an accelerated rate of cost accrual when planning is inadequate. This planning is performed by administrators. With increases in mechanization, control of the production process becomes less intuitive and more computational, requiring more technical experts (Woodward, 1965:154-81). And as mechanization increases, technical expertise is combined with day-to-day production operations first through more formalized structures (large batch) in which line and staff distinctions figure prominently, and finally through the duplication of skills across major organizational units (process). Woodward describes a fairly clean division of labor between research and production departments in small batch, a subdivision of research and development in large batch with increased problems of coordination, and a breakdown in process such that "pure" research may occur at a remote location while production departments include within their administrative components people with the same kinds of skills as those in the research departments.

One would also expect the number of production workers per unit of output to decline with mechanization. If the volume of production remains constant, or increases but slightly, administrative intensity ought to increase. At one extreme, when the volume of production is constant, the increase in administrative intensity is rather uninteresting since it is almost true by definition (given the assumption). When production levels increase with mechanization, administrative intensity can be expected to increase because much administrative work is based on the flow of materials and orders. Increased production is probably associated with increased selling efforts and more record-keeping. The "average daily patient load" measure of size used by Anderson and Warkov (1961) may be more theoretically relevant to this reasoning than to the size-coordination issue with which they were concerned.

Three variables were used to measure

mechanization. *Substance produced* is whether the organization's products were primarily integral or dimensional substances (coded zero and one, respectively; $r = .39$). *Automation* is whether or not automated production lines were used in the plant ($r = .34$). Finally, *Extent of automation* is the proportion of orders produced at least in part on automated lines ($r = .10$).¹²

Environmental Diversity and Change

If administrators perform other duties than coordination, we may look for variables which operate by affecting the number and variety of administrative functions. We take our lead here from Melman (1956:94) who argued that in the 1935-1948 period, administrative intensity increased in American manufacturing industries as management functions expanded. Bendix (1956:211-12) explained increasing administrative intensity in manufacturing through reference to the delegation of administrative functions by the entrepreneur to managers. And Chester (1961:45) noted that, among other things, "New managers were . . . brought in to supervise the operation of functions not undertaken in the firm before."

If we are concerned with the entire universe of goal-seeking organizations, it is obvious that certain functions are performed in some but not in others. Some conduct research while others do not. Some sell their products while for others output is not problematical. It also seems reasonable to expect variations of this type between manufacturing organizations. Many administrative functions arise from the relations between organization and environment. The more diverse or changeable that environment is, the greater the number of factors requiring administrative action. We may examine segments of the environment in a search for factors affecting the functional load borne by administration.¹³

¹²One of the measures, *substance produced*, is actually a characteristic of the product. However, there seems to be no a priori basis for arguing that liquid, gaseous or crystalline substances are more "complex" than objects. Rather, it is assumed here that such substances are processed through a more highly mechanized system than are objects.

¹³Dill (1958), Burns and Stalker (1961), Lawrence and Lorsch (1967), Perrow (1967), and Hage and Aiken (1970) did not address themselves to the

One such factor is labor unrest. The variable is whether or not the firm had been *struck by labor*. It was positively correlated with A/P ($r = .25$).

Another source of environmental factors which affect the functional load on administration is the market place. The first market variable is taken from Zwerman (1970). It is the *proportion of sales in the local area*. A negative correlation was expected because organizations selling in local markets would be less subject to the disturbing influences of transportation and communication problems. Data were not available for ten of the cases, but a positive correlation was observed ($r = .29$), contrary to expectations. It may be that a selection process operates through which organizations succeed in local markets by supplying services not available from distant competitors. For instance, a local supplier may offer reduced lag between the time at which orders are placed and the time at which they are filled.

A second market variable is the *importance of advertising*. It seemed reasonable to expect organizations operating in a changeable market to attempt to control it by advertising (Galbraith, 1967:198-210). The personnel who manage the organization's advertising are administrators. The results fit this expectation. Using a trichotomy (slight, moderate, crucial), the more important advertising was judged to be, the higher the administrative intensity ($r = .25$; $n = 37$).

Another market variable is the *instigation of sales*, whether or not the selling act was instigated by salesmen. A market which requires the organization to take the initiative by employing salesmen increases administrative intensity by adding to the functional load borne by administration. This was only measured among non-retail manufacturers because the use of whole-salers and marketing organizations might obscure the effects of the market structure. The results fit the expectation. When sales were instigated by salesmen, administration intensity was higher ($r = .20$; $n = 20$).

The final market variable is the presence or absence of *manufacturer's representatives*. Originally, this variable was viewed as a manifestation of the practice of purchasing

administrative services. If one asks, however, why a firm would employ manufacturer's representatives, a reasonable answer is that it would do so when entering a new market as a result of product change or when its sales operations are far-flung, with insufficient volume to justify the support of salesmen in the dispersed territories. The results fit this interpretation. Organizations which employed manufacturer's representatives had higher A/P ratios ($r = .34$; $n = 36$).

Environmental Constraint

At this point, we consider variables which have inverse effects on administrative intensity. We begin by noting that few organizations are perfectly free to vary their structures. In their article on hospitals, Anderson and Warkov (1961:27-8) mentioned the possibility that the disparity between their results and those of Terrien and Mills (1955) may have been due to some "bureaucratic constraint" operating differently on the units studied. Chester (1961:41) noted that general economic conditions may affect the level of administrative intensity. When the economy is contracting, administrative staffs may not be pared back as rapidly as production worker employment. Hawley, Boland and Boland (1965:255) argued that variations in administrative intensity may be due less to variations in structural complexity than to the effects of broader systemic properties such as those describing the flow of information into and out of the organization. We might ask, for instance, whether school districts are organized the way they are, at least in part, because of the requirements of outside agencies regardless of the wishes of local decision-makers.¹⁴ Pondy (1969) argued that top managers may desire the allocation of revenues to the expansion of administrative staff for status reasons. However, owners generally prefer that these revenues be allocated to profits. When owners are also managers, they are in a better position to restrain other managers. Consequently, administrative intensity should be lower. This becomes relevant if a distinction is made between the owner and manager role. The former is not part of the organization if Etzioni's criteria

issue of administrative intensity. However, the position adopted here is derived in large part from their ideas.

¹⁴Students of human ecology will no doubt recognize this as the basis of Hawley's Principle of Isomorphism (Hawley, 1968:334).

for membership are accepted (1969:21). He argued that the boundaries of organizations may usefully be defined in terms of three dimensions: involvement, subordination, and performance. The "owner" role is low on all three. The "manager" role is high on performance; and, in most organizations, it is high on involvement as well. If a person is both an owner and a manager, he occupies two roles, one of which is "in" the organization and one of which is "outside" the organization. His demand for profits is part of the latter role. As such, this demand ought to be considered an environmental constraint.

Zwerman (1970) studied manufacturing establishments, not industries as did Pondy. However, he also found that combined ownership and management tended to be associated with lower levels of administrative intensity (1970:112-14). Although external constraints were not a major subject for Blau and Schoenherr, they did discuss the dependence of employment security agencies on other branches of state government for administrative services (1971:156). They mentioned that machine politics in the northeastern states may permit administrators to expand their empires (157). In addition, the diverse needs of urban centers may require a more highly differentiated structure and shifts in the allocation of time to coordination (226). Legal requirements force agencies in different localities to maintain uniform statistical records (18). And most of the administrative budgets of state agencies are provided from a pool of federal money which is allocated through negotiation.¹⁵

One subset of constraint variables involves the autonomy of the local establishment in its relationship with headquarters.¹⁶ Three of these were scored identically and later combined in a summary measure: *pricing decisions*, the extent to which decisions about pricing were made at the headquarters as opposed to locally; *promotion decisions*, the degree to which decisions about the promotion of middle managers were made at headquarters; and *new product decisions*, the degree to which these decisions were made at headquarters. All three variables were neg-

atively related to A/P as expected: *pricing decisions*, $r = -.22$ ($n = 34$); *promotion decisions*, $r = -.17$ ($n = 34$); *new products decisions*, $r = -.34$ ($n = 33$). These three variables were combined into a summated ratings scale, called *autonomy*. Its correlation with A/P was .23.¹⁷

Another variable reflecting autonomy of the local unit was called *source of funds*. When funds were needed for plant expansion, did the unit gain them entirely through its own devices, entirely through headquarters, or a combination of the two? This variable was also negatively correlated with A/P, as one would expect if headquarters were keeping a tighter rein on local administrators ($r = -.25$; $n = 23$).

The last variable to be discussed here acts as a constraint on the organization by affecting the preferences of what Thompson (1967) has called the "dominant management coalition." Owners often institute management incentive plans to foster a correlation between their motivations and organization goals, or between management motivations and owner motivations. The dichotomy, presence or absence of *management incentive plans* was uncorrelated with administrative intensity, contrary to expectations ($r = .02$; $n = 41$).¹⁸

Since we are dealing here with nineteen variables and only forty-one cases, and the data are not complete for all cases, it is obvious that a meaningful multiple correlation coefficient could not be computed to represent their joint effects. A simple model, encompassing all three additive hypotheses, was developed with primary consideration given to minimizing the loss of cases. In it, A/P was regressed on four variables. For three of these, positive correlations were predicted: a technology variable (*substance produced*), and two environmental diversity-change variables (*manufacturer's representatives* and *importance of advertising*). The fourth independent variable, *management incentive*

¹⁵ See Table 1 for the method of scoring which explains the positive sign for the correlation coefficient.

¹⁶ Two variables, suggested by Pondy's study (1969), were the *form of ownership* (sole proprietorship and partnership vs. corporation) and *owner-management* (whether or not major owners were active in managing the firm). Neither of these was strongly correlated with A/P:

¹⁵ See Klatzky (1970) for a discussion of how variations in resources supplied by the Federal Government affect internal structure.

¹⁶ Whether or not the organization was a subsidiary had very little effect on A/P ($r = .10$; $n = 41$).

Table 1. Correlations between Nineteen Independent Variables and Administrative Intensity (A/P)

Independent Variable	Description	r	n
<u>Mechanization:</u>			
Substance produced	Integral = 0		
	Dimensional = 1	.39	39
Automation	No automated lines = 0		
	Have automated lines = 1	.34	40
Extent of automation	Proportion of orders on automated lines	.10	26
<u>Environmental Diversity and Change:</u>			
Struck by labor	No = 0		
	Yes = 1	.25	41
Proportion of sales in local area		.29	31
Importance of advertising	Slight = 0		
	Moderate = 1		
	Crucial = 2	.25	37
Instigation of sales	Not by salesman = 0		
	By salesman = 1	.20	20
Manufacturer's representative	Used = 0		
	Not used = 1	.34	36
<u>Environmental Constraint:</u>			
Pricing decisions	Locally = 0	-.22	34
Promotion decisions	Both (emph. local) = 1		
	Equally = 2	-.17	34
New product decisions	Both (emph. HQ) = 3		
	Headquarters = 4	-.34	33
Autonomy	12 minus sum of previous three	.23	33
Source of funds	Independent = 0		
	Both = 1		
	Headquarters = 2	-.25	23
Management incentive plans	Absent = 0		
	Present = 1	.02	41

plans, had a negligible zero-order correlation with A/P but its partial correlation was fairly strong and negative, as predicted by the model. For the thirty-three cases which had scores for all four variables, the multiple R was .65. The third-order partial correlation coefficients were: *substance produced*, .35; *manufacturer's representatives*, .47; *importance of advertising*, .45; and *management incentive plans*, -.34.¹⁹

The hypotheses developed in this section posit additive effects. The correlations are weak but they do lend some empirical support to the arguments.

Hypothesis 1. Organizations with more highly mechanized production technologies will have higher levels of administrative intensity.

¹⁹ The reader should notice that with so small a sample, and such crude measurement, the same set of variables cannot be expected to reveal identical results on replication. Many of the independent variables are continua measured as dichotomies. The random measurement error introduced by these

definitions probably weakens the correlations to a considerable degree.

The correlations between size (A + P) and the independent variables in Table 1 were quite low. The largest was with *proportion of sales in local area* ($r = -.23$; $n = 31$).

Hypothesis 2. Organizations operating in more diverse or changeable environments will have higher levels of administrative intensity.

Hypothesis 3. Organizations which are subjected to stronger environmental constraints will have lower levels of administrative intensity.

INTERACTION MODEL

James Thompson (1967:14-24) has argued that organizations seek to insulate their "core technologies" from the uncertainty produced by a diverse or changeable environment. That is, they seek to make possible the application of a closed-system logic within certain organization units. In so doing, they permit the use of "technical rationality" within their technical cores. This means that they try to provide these units with as high a degree of certainty as possible on which calculable decisions can be based. They do this by removing from consideration problems of input and output for the units in the technical core. They create other organizational units which insulate the units in the technical core from environmental variations. These units "buffer" the core technology by building up inventories and by stockpiling raw materials. They attempt to anticipate variations in supply of resources and demand for outputs. And sometimes, they gain control over sources of variation in the environment.

Thompson's insights can be linked to the subject of administrative intensity through the assumption that organizations insulate their core technologies primarily through the efforts of administrators. The result is that *the more organizations seek to provide their core technologies with the stability necessary for a closed-system logic, the more administrative intensity they will display.*

This line of reasoning complements the additive hypothesis concerning environmental diversity and change. In fact, each of the variables studied with reference to that hypothesis may be viewed as a "contingency" or "constraint" in Thompson's terms.

Manufacturing organizations use what Thompson calls "long-linked technologies." Their technical cores are production systems. Woodward noted that as production tech-

nologies become increasingly complex, "it becomes increasingly possible to exercise control over manufacturing operations, the physical limitations of production become better known and understood." Uncertainty in the operations themselves declines and the degree of predictability of results increases (Woodward, 1965:40). Her position suggests an elaboration of Thompson's work. A failure of insulation for some technologies is more disruptive and costly than it is for others. Organizations with more highly complex technologies can be expected to expend more effort in insulating their core technologies. If it is accepted that these insulating efforts are carried out primarily by administrators, then the effects of environmental variability increase with the complexity of the core technology.

This argument is couched in terms of "technical complexity," Woodward's term. It may be developed somewhat by considering mechanization. As the production technologies of manufacturing organizations become more highly mechanized, the tolerance for variations in the timing, quantity, and quality of raw materials declines. While skilled artisans working with hand tools may modify their activities to fit variations in quality, highly mechanized systems do not offer this flexibility. If input or output problems arise, production workers in plants with low levels of mechanization may more easily be shifted to working on different products. On an individual level, each worker can change to a different product more rapidly since set-up time is lower. At an aggregate level, it is less likely that large proportions of the total work force will be working on the same product at one time. So the probability that some disrupting influence will simultaneously affect large proportions of the work force is lower. The production systems in organizations with low levels of mechanization are less likely to produce sequences of operations involving large numbers of workers. For this reason, disruption at one point in the system is less likely to bring about a total stoppage of work in the plant as a whole than when mechanization is high. Furthermore, highly mechanized plants produce output at a much higher rate, and represent a higher level of capital investment. For these reasons, technical rationality is crucial in highly mechanized plants. Mechanization presents a prom-

ise of high productivity. Smooth input and output transactions are essential if this promise is to be realized. So highly mechanized production systems are less flexible and therefore more susceptible to disruption. And if disruption occurs, costs mount more rapidly. Environmental variations which may disrupt *any* manufacturing system are more likely to disrupt highly mechanized ones, and the disruption is likely to be more costly.

Woodward noted that firms in which labor costs were low (the more highly mechanized ones) spent more money on employing specialists in the personnel and labor relations fields. She attributed this correlation to the ability of the more highly mechanized firms to pay rather than to the need for such specialists (Woodward, 1965:55). It was assumed that the need was low because labor relations were generally better. An alternative, suggested by the line of reasoning developed here, is that the workers in highly mechanized plants are in a position to do much more damage should they be dissatisfied. From this perspective the employment of personnel and labor relations specialists is not so much a result of a surplus of funds to be allocated, but a reaction to a potential source of disruption.

Hypothesis 4. When organizations have higher levels of mechanization in their production systems, environmental diversity and change variables will have greater effects on administrative intensity.

The final hypothesis is a straightforward extension of Hypothesis 3. When environmental constraints are high, other causes of administrative intensity can be expected to have less effect than when constraints are low. If the local establishment is perfectly free to adjust its administrative intensity, not only might one expect the level to be higher because top managers have interests in expanding their staffs (Pondy, 1969), but one would expect other variables to have stronger effects. Suppose labor relations become more problematical. If the local establishment is not closely scrutinized by headquarters, it may react with a greater expansion in its personnel management activities than it would if autonomy were low. In other words, any real cause of administrative intensity is less likely

to be used as the basis for an argument to expand administrative staff by local managers. The more closely local managers are watched by headquarters officials, the less effective these arguments will be.

Hypothesis 5. When organizations operate under tighter environmental constraints, other independent variables will have weaker effects on administrative intensity.

This hypothesis has some important implications for organization theory in general. Barnard (1966:97-9) identified the subordination of some organization to others as a problem for researchers in establishing the proper unit of analysis. The unit property is often viewed as less problematic in this area than in others. For instance, deciding who is and who is not a member of General Motors is less difficult than deciding who is and who is not a New Yorker or an American. If an open-systems perspective is adopted, and one admits that some organizations are more highly constrained than others, a different sort of boundary problem is suggested. Is a local manufacturing establishment an organization or is it a part of an organization? One can ask a similar question about the unit properties of governmental organizations like state employment security agencies. Local offices are parts of state agencies which are elements of state governments. But the state agencies are controlled to some degree by the Department of Health, Education and Welfare, part of the U. S. government, which may be viewed as one large organization. If one views organizations as more or less independent of one another, particularly when the subject of interest is joint programs (Thompson and McEwen, 1958; Aiken and Hage, 1968), the unit property itself becomes variable, confounding the important distinction between units of analysis and variables.

Two technical points should be made to clarify Table 2. First, the use of product terms to represent interaction effects is conservative because of the high correlation between one (or both) of the additive terms and the product term, and the fact that the partial r treats all variance which could be attributed to either main or interaction effects as if it were all due to the main effects (Althauser, 1971). Second, the sign of the product term

Table 2. Interaction Effects: Technology and Environmental Constraints with Other Variables on Administrative Intensity (A/P)

Model I: $A/P = a + b_1X_1 + b_2X_2 + e$									
Model II: $A/P = a + b_3X_1 + b_4X_2 + b_5X_1X_2 + e$									
Independent Variables	R_I	R_{II}	Diff. in R^2	b_1	b_2	b_3	S.E. b_5	Partial r for X_1X_2	n
Substance produced (X_1) and:									
Struck by labor (X_2)	.415	.480	.058	.93	.47	-.83	.51	-.264	39
Proportion of sales in local area (X_2)	.513	.522	.010	.59	.004	.005	.009	.118	29
Importance of adv. (X_2)	.450	.493	.040	.29	.11	.40	.31	.225	35
Instigation of sales (X_2)	.202	.541	.121	-.08	.05	.56	.30	.382	24
Manufacturer's rep. (X_2)	.436	.491	.051	-.12	.23	.82	.58	.251	34
Automation (X_1) and:									
Struck by labor (X_2)	.373	.373	.000	.40	.20	.04	.73	.010	40
Proportion of sales in local area (X_2)	.402	.417	.013	.30	-.003	.01	.01	.123	30
Importance of adv. (X_2)	.367	.373	.004	.35	.02	.14	.36	.071	36
Instigation of sales (X_2)	.443	.527	.081	.01	.08	.41	.27	.318	24
Manufacturer's rep. (X_2)	.382	.451	.036	.07	.02	.56	.47	.208	35
Autonomy (X_1) and:									
Struck by labor (X_2)	.347	.426	.061	.02	-.38	.10	.07	.264	33
Proportion of sales in local area (X_2)	.369	.379	.007	.04	.001	.001	.001	.092	23
Importance of adv. (X_2)	.387	.633	.252	-.02	-.65	.11	.03	.544	31
Instigation of sales (X_2)	.326	.327	.000	.02	.22	.001	.05	.007	19
Manufacturer's rep. (X_2)	.454	.476	.021	.02	.07	.05	.07	.163	29
Substance produced (X_2)	.603	.726	.163	.05	-.12	.18	.06	.506	32
Automation (X_2)	.446	.470	.022	.01	.07	.06	.07	.167	32
Extent of automation (X_2)	.327	.413	.064	-.01	-.01	.001	.001	.267	33
Source of funds (X_1) and:									
Struck by labor (X_2)	.393	.613	.221	.06	2.50	-1.16	.44	-.512	24
Proportion of sales in local area (X_2)	.245	.485	.175	.49	.03	-.01	.01	-.431	16
Importance of adv. (X_2)	.373	.606	.228	.10	1.26	-.61	.24	-.515	22
Manufacturer's rep. (X_2)	.506	.704	.240	.10	2.81	-1.18	.42	-.568	21
Substance produced (X_2)	.488	.705	.258	.18	2.74	-1.23	.39	-.582	23
Automation (X_2)	.519	.716	.243	.08	2.76	-1.19	.38	-.576	24
Extent of automation (X_2)	.329	.640	.302	.12	.04	-.02	.01	-.582	24
Management incentive plans (X_1) and:									
Struck by labor (X_2)	.250	.267	.009	-.07	.14	.32	.53	.098	41
Proportion of sales in local area (X_2)	.365	.389	.019	-.07	.02	-.01	.02	-.146	29
Importance of adv. (X_2)	.262	.295	.018	.04	.46	-.30	.37	-.141	37
Instigation of sales (X_2)	.313	.362	.033	.14	.42	-.29	.32	-.192	25
Manufacturer's rep. (X_2)	.393	.409	.014	-.17	.83	-.38	.53	-.126	36
Substance produced (X_2)	.402	.529	.118	.07	2.08	-1.64	.69	-.376	39
Automation (X_2)	.357	.366	.006	-.03	.68	-.24	.48	-.083	40
Extent of automation (X_2)	.219	.419	.127	.34	.01	-.01	.01	-.365	40

regression coefficient refers to the direction of movement in the regression line of A/P on one independent variable as the other increases.

Hypothesis 4 predicts that as mechanization increases, the effects of the environmental diversity-change variables increase. Since each of them had a positive relationship with A/P, the interaction effect should also be

positive. While some effects appear to be negligible, all but one are positive, as predicted.²⁰

²⁰ The third mechanization indicator, *extent of automation*, produced only two interactions worthy of note. One of these, with *struck by labor*, was in the opposite direction from that hypothesized ($r =$

Hypothesis 5 predicts positive interaction effects for *autonomy* since the more autonomous the organization, the less constraint. All the interactions were positive, but many were weak. *Source of funds* was coded in such a way that constraints increased with it. So the interactions should be negative. Once again, all were negative and the partials were quite large. A reasonable interpretation is that the funding pattern is a better measure than the decision variables on which *autonomy* is based because error may be introduced into the decision variables through morale-building fictions (e.g., "we seriously considered your views but decided that the new product would stretch company resources too thin"). Also, a broad range of constraints can be expected to follow the funding pattern but not necessarily the decision variables. It is difficult to believe that funds for plant expansion could come through the headquarters without a prior flow of detailed information on management practices at the local level. On the other hand, the samples were smaller for these interactions than for the others; and one would expect larger partials because of the reduction in degrees of freedom.

The interaction effects involving *management incentive plans* should all be negative. One is not (*struck by labor*) and, once again, several are weak; but the results generally fit the hypothesis.²¹

CONCLUSIONS

Although one would not want to draw firm conclusions from the data analysis reported above, there does seem to be good reason for doubting the basic size-complexity-coordination explanation for variations in administrative intensity. Much of the empirical support for this position was called into question because of the definitional dependencies which pervade the literature. Apart from this, however, it seems obvious that not all mem-

bers of the administrative component spend all or even most of their time securing coordination. To the degree that different administrative personnel do different things, one might well look to the subcomponents for explanation, a position which suggests that Rushing's (1967) lead would be well worth following.

Considerable attention was given to mechanization as a characteristic of production technology. The nature of the product may be of equal importance. A brewery and a chemicals firm may use very similar production systems, but one would expect the chemicals firm to have a wider variety of products, a more diverse set of raw materials, and a more elaborate research and development operation. Administrative intensity may be higher, then, because administrative functions are linked to these product-related variables. Similarly, many of the effects attributed to size may in fact reflect variations in the volume of production and variations in the quantities of materials used. For instance, variations in administrative intensity of school districts may result from the fact that administrators spend much of their time accounting to superordinate agencies in terms of information based upon student enrollments. The number of teachers may be quite irrelevant to these accounting activities. At the same time, however, student-teacher ratios may be maintained at a constant level either because internally-derived policies demand it for pedagogical reasons, or because outside agencies assess quality on this basis. Aside from the point that this latter instance constitutes a kind of environmental constraint, it is important to note that the size of enrollments would produce a spurious correlation between number of faculty and number of administrators. This is a potentially serious criticism of the reformulation suggested for the size-administrative intensity issue.

Although the administrative contributions to organizational control and coordination have not been ignored, emphasis has been placed on other things administrators do. To the degree that administrative apparatuses act as regulators, environmental diversity and change variables may be considered as sources of variety in system inputs. Ashby's Law of Requisite Variety is clearly relevant here. As he puts it, "only variety can destroy variety"

-.23; $n = 40$). The other was with *instigation of sales* ($r = .28$; $n = 24$).

²¹ The variables *form of ownership* and *owner-management* (see footnote 16) produced some fairly strong interactions, but their signs were not consistent in either support or opposition to the hypotheses. For both, the sample sizes tended to be small and measurement is weakened by the fact that some of the organizations were subsidiaries while other were independent.

(Ashby, 1956:207). Variety in administrative subsystems (i.e., complexity) must develop if variety of inputs increases and stability of output is to be maintained. Work-flow technology is often viewed as a "contextual" variable. As such, increasing variety produced by a more complex production technology ought to have similar effects. In addition, this technological complexity produces a lower tolerance for output instability from the "insulating" units identified by Thompson (1967:20-3). If Ashby is correct, this condition should result in growing complexity within these units as well.

If it is true that constraints may exist which limit the response organizations exhibit to increasing variety, they may be forced to risk the consequences of less than optimal regulation. An alternative, however, lies in the heavier reliance on superordinate organizations for regulation. An important implication of this is that what appears to be a lower level of administrative intensity may actually be a transfer of administrative tasks (and manpower resources) to superordinate organizations (e.g., headquarters). In exercising a constraining influence on some other organization, the superordinate agency takes on an additional administrative burden of its own.

If organizations react to environmental disturbances in different ways depending on production technology variables, and if their reaction to most independent variables depends on the degree to which they are constrained by outside agencies, a more careful examination of these and other sources of interaction is of paramount importance. Until these underlying conditions are discovered and understood, replications are not likely to produce consistent results. Such a position would seem to underly the argument that different theories are required to explain structure and process in different kinds of organizations.

REFERENCES

- Alken, Michael, and Jerald Hage.
1968 "Organizational interdependence and intra-organizational structure." *American Sociological Review* 33 (December): 912-30.
- Akers, Ronald, and Frederick L. Campbell.
1970 "Size and the administrative component in occupational associations." *Pacific Sociological Review* 13 (Fall) 241-51.
- Ashby, Robert.
1971 "Multicollinearity and non-additive regression models." Pp. 453-73 in Hubert Blalock, Jr. (ed.), *Causal Models in the Social Sciences*. Chicago: Aldine-Atherton.
- Anderson, Theodore R., and Seymour Warkov.
1961 "Organization size and functional complexity: a study of administration in hospitals." *American Sociological Review* 26 (February): 23-8.
- Ashby, W. Ross.
1956 *An Introduction to Cybernetics*. New York: Science Editions.
- Baker, A. W., and R. E. Davis.
1954 *Ratios of Staff to Line Employees and Stages of Differentiation of Staff Functions*. Columbus, Ohio: Bureau of Business Research, Ohio State University.
- Barnard, Chester I.
1966 *The Functions of the Executive*. Cambridge: Harvard.
- Bendix, Reinhard.
1956 *Work and Authority in Industry*. New York: Wiley.
- Blau, Peter, and Richard A. Schoenherr.
1971 *The Structure of Organizations*. New York: Basic Books.
- Blau, Peter.
1970 "A formal theory of differentiation in organizations." *American Sociological Review* 35 (April): 201-18.
- Blau, Peter, and W. Richard Scott.
1962 *Formal Organizations: A Comparative Approach*. San Francisco: Chandler.
- Bossard, J. H. S.
1945 "The law of family interaction." *American Journal of Sociology* 50 (January): 292-4.
- Burns, Tom, and G. W. Stalker.
1961 *The Management of Innovation*. London: Tavistock.
- Caplow, Theodore.
1957 "Organizational size." *Administrative Science Quarterly* 1 (March): 484-505.
- Chester, T. E.
1961 *A Study of Post-War Growth in Management Organizations*. A report prepared for the European Productivity Agency of the Organization for European Economic Cooperation, E. P. A. Project No. 347.
- Dill, William R.
1958 "Environment as an influence on managerial autonomy." *Administrative Science Quarterly* 2 (March): 409-43.
- Etzioni, Amitai.
1969 *A Comparative Analysis of Complex Organizations*. New York: The Free Press.
- Freeman, John Henry, and Jerrold Kronenfeld.
1973 "Problems of definitional dependency: the case of administrative intensity." *Social Forces* 52 (September): 108-21.
- Fuguitt, Glenn V., and Stanley Lieberman.
1972 "The correlation of ratios or difference scores having common terms." Unpublished.
- Galbraith, John Kenneth.
1967 *The New Industrial State*. Boston: Hough-

- ton Mifflin.
 Graicunas, V. A.
 1933 "Relationship in organization." *Bulletin of the International Management Institute* 7 (March):39-42.
 Haas, Eugene, Richard H. Hall and Norman Johnson.
 1963 "The size of the supportive component in organizations: a multi-organizational analysis." *Social Forces* 42 (October):9-17.
 Hage, Jerald, and Michael Aiken.
 1970 *Social Change in Complex Organizations*. New York: Random House.
 Haire, Mason.
 1959 "Biological models and empirical histories of the growth of organizations." Pp. 272-306 in Mason Haire (ed.), *Modern Organization Theory*. New York: Wiley.
 Hawley, Amos.
 1968 "Human ecology." Pp. 328-37, Vol. 4, in David L. Sills (ed.), *International Encyclopedia of the Social Sciences*. New York: Macmillan.
 Hawley, Amos, Walter Boland and Margaret Boland.
 1965 "Population size and administration in institutions of higher education." *American Sociological Review* 30 (April):252-5.
 Hendershot, Gerry E., and Thomas F. James.
 1972 "Size and growth as determinants of administration-production ratios in organizations." *American Sociological Review* 37 (April):149-53.
 Hickson, D. J., D. S. Pugh, and Diana C. Pheysey.
 1969 "Operations technology and organization structure: an empirical reappraisal." *Administrative Science Quarterly* 14 (September):378-97.
 Holdaway, Edward A., and Thomas A. Blowers.
 1971 "Administrative ratios and organization size: a longitudinal examination." *American Sociological Review* 36 (April):278-86.
 Indik, Bernard P.
 1964 "The relationship between organization size and supervision ratio." *Administrative Science Quarterly* 9 (December):301-12.
 Kephart, William M.
 1950 "A quantitative analysis of intragroup relations." *American Journal of Sociology* 55 (May):544-9.
 Klatzky, Sheila R.
 1970 "Organizational inequality: the case of public employment agencies." *American Journal of Sociology* 76 (November):474-91.
 Lawler, P. F.
 1947 *Records for the Control of Growing Manufacturing Enterprises*. Cambridge: Harvard.
 Lawrence, Paul R., and Jay W. Lorsch.
 1967 *Organization and Environment: Managing Differentiation and Integration*. Cambridge: Harvard.
 Melman, Seymour.
 1951 "The rise of administrative overhead in the manufacturing industries of the United States 1889-1947." *Oxford Economic Papers* 3:62-112.
 1956 *Dynamic Factors in Industrial Productivity*. New York: Wiley.
 Meyer, Marshall W.
 1972 "Size and the structure of organizations: a causal analysis." *American Sociological Review* 37 (August):434-40.
 Parkinson, C. Northcote.
 1957 *Parkinson's Law*. Boston: Houghton Mifflin.
 Perrow, Charles.
 1967 "A framework for the comparative analysis of organizations." *American Sociological Review* 32 (April):194-208.
 Pondy, Louis.
 1969 "The effects of size, complexity, and ownership on administrative intensity." *Administrative Science Quarterly* 14 (March):47-60.
 Raphael, Edna.
 1965 "The Anderson-Warkov hypothesis in local unions: a comparative study." *American Sociological Review* 32 (October):768-76.
 Rushing, William.
 1967 "The effects of industry size and division of labor on administration." *Administrative Science Quarterly* 12 (September):273-95.
 Schuessler, Karl.
 1973 "Ratio variables and path models." In A. S. Goldberger and O. D. Duncan (eds.), *Structural Equation Models in Social Science*. New York: Seminar Press.
 Starbuck, William.
 1965 "Organizational growth and development." Pp. 451-533 in James G. March (ed.), *Handbook of Organizations*. Chicago: Rand McNally.
 Terrien, Frederick L., and Donald L. Mills.
 1955 "The effect of changing size upon the internal structure of organizations." *American Sociological Review* 20 (February):11-13.
 Thompson, James D.
 1967 *Organizations in Action*. New York: McGraw-Hill.
 Thompson, James D., and William J. McEwen.
 1958 "Organizational goals and environment: goal-setting as an interaction process." *American Sociological Review* 23 (February):23-31.
 Tsouderos, John E.
 1955 "Organizational change in terms of a number of selected variables." *American Sociological Review* 20 (April):206-10.
 Woodward, Joan.
 1965 *Industrial Organization: Theory and Practice*. London: Oxford.
 Zwerman, William L.
 1970 *New Perspectives on Organization Theory*. Westport, Conn.: Greenwood.

TWO SOURCES OF ERROR IN ECOLOGICAL CORRELATIONS*

John L. Hammond

Columbia University and Center for Policy Research

American Sociological Review 1973, Vol. 38 (December):764-77

The discrepancy between individual and ecological correlations has often been noted, but its sources have not been understood. The discrepancy can arise from two quite distinct sources, both of which can be explained sociologically. The two sources have opposite implications for the possibility of inference to individual relationships from aggregate data. When individuals are grouped into neighborhoods on the basis of their homogeneity on an independent variable, the ecological correlation will necessarily be larger than the individual correlation; but the regression equation of the aggregate variables provides an unbiased estimate of the individual regression. Aggregation bias arises when the independent variable has a contextual effect, or when individuals are grouped into neighborhoods on the basis of their similarity on the dependent variable. If aggregation bias is present, no inference about the individual relationship can be drawn from aggregate data. An investigator's knowledge of the social processes operating in the situation he is examining will often enable him to estimate whether his data incorporate aggregation bias; if they do not, he can draw inferences about the individual relationship from aggregate data.

Under certain conditions it is valid to make "ecological inferences"; the relations between variables for individuals can be correctly inferred from the relations between those variables for aggregates. These conditions can be understood as the consequences of social processes, the same processes that are usually the object of the investigator's research. A knowledge of the social processes giving rise to aggregations can, therefore, inform an analyst about the propriety of drawing inferences about individuals from aggregate data.

Understanding the dependence of aggregation relations on social processes can have at least three beneficial consequences: first, it will enable a researcher to use his knowledge of the social processes he is investigating to justify (or, if unjustifiable, to reject) the use of aggregate data for inference to individuals. Second, it will make aggregation relations more comprehensible to sociologists who understand social reality better than they understand mathematical deduction. Third, knowledge that some aggregation relations do entail biases can motivate research to estimate the degree of bias, permitting reasonable inferences even in those cases.

Since Robinson's article (1950) it has become a commonplace that "ecological cor-

relations are not generally equal to individual correlations," or, more precisely, that the value of a correlation coefficient between two variables for which individuals are the unit of analysis is not in general the same as the correlation coefficient between summary measures of those same variables for aggregates into which the individuals are grouped. While this phenomenon has been generally recognized, its causes and consequences are not so well understood. There are different kinds of aggregation relations,¹ which have very different implications for the possibility of making inferences about individual relations from

¹The phrase "aggregation relations" is used throughout this paper to refer to the presence or absence of congruence between relations at the individual level and relations between the analogous aggregate variables. I therefore depart from the usage of Hannan (1970:21) and others, for whom an aggregation relation refers to the way an aggregate-level variable is derived from the individual values for its constituent members.

Throughout the paper, I freely use the words "individual" and "aggregate" as a shorthand for a more precise but also more cumbersome description of data, variables, relationships, etc. for which the unit of analysis is, respectively, an individual or an aggregate of individuals. An aggregate relation, then, is different from an aggregation relation: the former is a relation between variables measured for aggregates, while the latter, as defined in this note, is the relation between an individual relation and an aggregate relation.

I retain the customary usage "ecological inference" to refer to the inference of individual relations from the corresponding aggregate relations.

*I am indebted to Peter M. Blau, Jonathan R. Cole, Jonathan Kelley, and Donald J. Treiman for their comments on an earlier version of this paper.

aggregate data. Discussions of the sources of bias in aggregation have usually described those sources in statistical formulae, however (cf. notably Goodman, 1959; Duncan et al., 1961: 60-80), without demonstrating that identifiable social processes are responsible for the statistical relations.

Probably the most frequent use of aggregate data by sociologists is as a substitute for unavailable individual data. The sociologist wishes to estimate the relationships he would find if the individual data were available. Under what conditions will the relations he observes in his data be reasonable estimates of the relations he would have found if he had had the individual data, and under what conditions will they contain bias? When, in other words, will he be a victim of the ecological fallacy?

Briefly, the two sources of error in ecological correlations are homogeneous grouping and aggregation bias. If individuals are grouped homogeneously (on the independent variable), a measure of correlation between two variables will necessarily be larger for aggregates than for individuals. However, this error is readily overcome, because the relation between the two aggregate variables expressed by a regression equation will be an unbiased estimate of the regression equation relating the individual variables. Aggregation bias, on the other hand, arises when the relation between the two individual variables is systematically different in different units of aggregation. In such cases, the relation in aggregate data cannot be used to estimate the individual relation without further knowledge or assumptions. In this paper I will explain, both in terms of social and of statistical processes, the conditions under which aggregation bias prevents the straightforward use of aggregate regressions to estimate individual relations.

AGGREGATION AS A SOCIAL PROCESS

It is perhaps the fundamental sociological truth, and truism, that people do not group themselves randomly into aggregates, but unite with others on the basis of similarity or complementarity. From the phenomenon of homogeneous grouping into aggregates derives the most important aggregation relation responsible for Robinson's dictum about "ecological correlations." In the absence of aggrega-

tion bias (to be defined precisely below), aggregate variables under homogeneous grouping necessarily have higher correlations (in absolute value) than the corresponding individual variables. Regression coefficients, on the other hand, have the same expected values for aggregates as for individual variables.

Typically one is investigating the relation between two variables, which can divide all the individuals in a sample into discrete categories or range them along a continuum,² and which are correlated with each other to an unknown degree. The sample members' residences (for example) can be grouped into areal units (census tracts, wards, counties, regions, etc; the general term "tracts" will be used here.)³ A summary measure such as a mean or a proportion will reveal the average level of each variable in each tract. To understand how an investigator can work "downward" from aggregate to individual relations, one must first examine how the process works "upwards"—that is, for a specified relation in individual data, what will be the relation in aggregate data?

Assume that we are interested in the relationship between two variables X and Y which, for a population, have a correlation of r_{XY} at the individual level. Assume further, for simplicity, that the individuals reside in m tracts, all of size two. The aggregate variables used in computing the "ecological correlation" between X and Y are the means of X and Y for the residents in each tract. For any tract j , the tract values are

$$\bar{X}_j = \frac{X_{j1} + X_{j2}}{2} ; \quad \bar{Y}_j = \frac{Y_{j1} + Y_{j2}}{2}$$

² As Cohen (1968), Fennessey (1968), and others have shown, there is no formal difference between the treatment of discrete and continuous variables. A discrete variable can be treated as a dummy variable (if dichotomous) or a set of dummy variables, for which each individual is scored 1 if he possesses the attribute and 0 if he does not. The proportion of the sample possessing the attribute is then equal to the mean of the dummy variable.

³ Throughout this paper I assume that the individuals are human beings and that the aggregates are geographically defined. But in fact the conclusions hold for other dimensions of aggregation, such as time periods (cf. Lee et al., 1970), organizations, and occupational or other classifications. Moreover, the "individual level" can refer to the lower in any pair of levels of analysis.

Tract means are computed for each of the m tracts, and these means \bar{X}_j and \bar{Y}_j are the summary measures from which the aggregate correlation is computed.

The Appendix to this paper demonstrates that, for tracts of size two, the ecological correlation is

$$\bar{r}_{X_j Y_j} = \frac{r_{X_{ji} Y_{ji}} + r_{X_{ji} Y_{jk}}}{\sqrt{(1 + r_{X_{j1} X_{j2}})(1 + r_{Y_{j1} Y_{j2}})}} \quad (1)$$

In this expression, $r_{X_{ji} Y_{ji}}$ is the correlation between an individual's X and his Y , and is thus equal to the correlation r_{XY} for the entire sample of individuals. The other terms in equation (1) are the correlations between the values of "neighbors." $r_{X_{j1} X_{j2}}$ is the correlation between the X values of two people residing in the same tract; $r_{Y_{j1} Y_{j2}}$ is the correlation between the Y values of two people residing in the same tract; and $r_{X_{ji} Y_{jk}}$ is the correlation between the X of one tract resident and the Y of his neighbor. The relation between the individual correlation and the aggregate correlation clearly depends on the size of these terms.

One case of the relationship between individual and aggregate correlations is that of random or nonhomogeneous grouping. It is probably a null case for any variable of sociological interest, but an examination of its implications will facilitate the exposition of other kinds of aggregation relations. If individuals are grouped randomly into tracts, there should be no correlation between an individual's value and his neighbor's value on either X or Y . Thus all the terms on the right side of equation (1) become zero except $r_{X_{ji} Y_{ji}}$, and the equation reduces to

$$\bar{r}_{X_j Y_j} = r_{X_{ji} Y_{ji}} = r_{XY} \quad (2)$$

So, when a population is grouped randomly, the aggregate-level correlation between two variables is precisely the same (in expected value) as the individual-level correlation.

The Appendix also shows that, when individuals are grouped randomly, the regression coefficient of Y on X for individuals is the same in expected value as the regression coefficient of \bar{Y}_j on \bar{X}_j for aggregate units.⁴

⁴Blalock, performing a random grouping into units, demonstrates this empirically (Blalock, 1964:102-5).

The derivation readily generalizes to tracts of greater size. If the tracts are unequal in size, the same result can be derived for both correlation and regression coefficients, but the coefficients have a larger sampling variance.⁵

HOMOGENEOUS GROUPING

In the real world, people do not gather into residential units randomly, any more than they gather randomly into other categories. Nevertheless, if there is no aggregation bias, there is a simple correspondence between the individual relations and the aggregate relations: the aggregate correlation coefficient is larger than the individual correlation coefficient, but the regression coefficients are identical.

If there is no aggregation bias, the relation between independent and dependent variables is the same (or, more precisely, its expected value is the same) in each aggregate unit. This is equivalent to the condition that all the homogeneity in the tract is due to the independent variable, or, in other words, that any relation between an individual's Y -value and his neighbor's Y -value is due entirely to their similarity on X .

The Appendix demonstrates that when individuals are so grouped (and all tracts have a population of two), the aggregate correlation is

$$\bar{r}_{X_j Y_j} = r_{XY} \sqrt{\frac{(1 + r_{X_{j1} X_{j2}})}{(1 + r_{XY}^2 r_{X_{j1} X_{j2}})}} \quad (3)$$

which must be greater than r_{XY} because the radical sign contains an expression whose numerator is necessarily greater than its denominator. So the "ecological" correlation is necessarily larger than the individual one, as has often been noted empirically.

But the Appendix also demonstrates that, in the case of homogeneous grouping, the regression coefficients (slope and intercept) derived from aggregate data are identical in

⁵It should be noted that, were random grouping an empirical possibility, the standard deviations of \bar{X}_j and \bar{Y}_j would become very small as tract size increased, so that the estimates of individual relations would be very unstable.

expected value to those derived from individual data. This may seem counterintuitive, but it should be remembered that, as I will explain in more detail below, the regression coefficients define a straight line through a scatterplot. If the points in the scatterplot are divided into subgroups, the means of those subgroups will be represented by points which cluster around the same straight line (leaving the regression coefficients unchanged), but the points will be less widely dispersed around the line (increasing the correlation coefficient).

Thus, while homogeneous grouping introduces a bias into the correlation coefficient, it does not introduce any bias into the regression coefficients. Under the condition of homogeneous grouping without aggregation bias, therefore, the regression of aggregate variables is identical (in expected value) to the regression of individual variables. To draw inferences about individual relations from aggregate data, then, one must examine not the correlation between aggregate variables but the regression of the dependent variable on the independent variable.

How are the results of an aggregate regression to be interpreted? The operational interpretation depends on whether the individual variables are discrete or continuous (although the derivations presented in the Appendix are not affected by the level of measurement). If both independent and dependent variables are dichotomous, the regression coefficients can be transformed into the percentages in a fourfold table, as the following example demonstrates.

Dichotomous individual variables. Table 1 presents hypothetical data on the relation between religion and vote in a particular election, where both variables have been dichotomized. The relationship presented in this table can also be expressed in a regression equation, in which the two variables are expressed as dummy variables. Let X_i be a dummy variable which takes the value of 1 if individual i is Catholic, 0 if he is not; and Y_i be a dummy variable which takes the value 1 if he voted Democratic, 0 if he did not. Then the regression equation computed for the sample described in Table 1 will have the solution

$$Y_i = .45 + .16 X_i \quad (4)$$

and the predicted values of Y_i can be interpreted as proportions: of non-Catholics ($X_i = 0$), 45% voted Democratic, while the proportion of Catholics voting Democratic was $45 + 16 = 61\%$. Thus the regression coefficients can be adjusted to provide values equivalent to the cell values in the first row of Table 1 (and the second row can of course be computed by subtracting the values in the first row from 100). In general, if p is the proportion possessing trait Y among those who possess trait X , and q is the proportion possessing trait Y among those who do not possess trait X , the solution to the regression equation

$$Y_i = a + bX_i \quad (5)$$

will bear the following simple relationship to the within-cell rates:

$$a = q; a + b = p \quad (6)$$

Thus, in this hypothetical case, p is .61 and q is .45.

Table 1. Vote by Religion
(Hypothetical Data)

Vote	Religion	
	Catholic	Other
Democrat	61%	45%
Republican	39%	55%
	100%	100%

Now let us consider the relation between religion and vote at the aggregate level (assuming the election to be a citywide one and the areal units to be wards). To say that there is no aggregation bias is to say that the relation between religion and vote is the same in every ward, i.e. that the proportion of non-Catholics voting Democratic is the same in each ward (namely, 45%) and the proportion of Catholics voting Democratic is the same in each ward (namely, 61%). (As we will see below, this unlikely assumption need not be met precisely; it is sufficient that the proportion of Catholics or of non-Catholics

voting Democratic not be dependent on the density of Catholics in the ward.) If this assumption is met, the proportion voting Democratic in each ward is an exact function of the proportion Catholic in the ward, namely

$$\bar{Y}_j = .45 + .16\bar{X}_j \quad (7)$$

where \bar{X}_j is the proportion Catholic in the ward, or the aggregate variable derived from the individual dummy variable X , and \bar{Y}_j is similarly the proportion voting Democratic in the ward.

If the proportion voting Democratic in a ward is an exact function of the proportion Catholic, then the means for each ward will, in a scatterplot, fall exactly on a straight line. Similarly, if these means are substituted in equation (5) for the corresponding individual variables, the regression equation derived from them will be precisely equation (7), which of course also defines the straight line observed in the scatterplot. (In the more likely case that the within-cell proportions vary slightly from ward to ward, the points in the scatterplot will be scattered around this line rather than falling exactly on it.) The within-cell proportions can then be derived from the aggregate regression coefficients by equation (6). The correlation between the aggregate variables may, because the points lie close to the line, be higher than the association between individual variables, but the regression equation yields estimates of the values which would be observed in the table of individual variables (and any individual-level measure of association can, if desired, be computed from them).

Continuous individual variables. In the case of continuous variables, the aggregate variable is a tract mean rather than a tract proportion, and the assumption that the relation between independent and dependent variables is identical in all tracts is translated into the assumption that the same regression line describes that relation for all tracts, even though different tracts occupy different parts of the range of that line.⁶ It can readily be shown that if the same regression line describes the relation

between two variables in each tract, then all the tract means fall exactly on that regression line, and the line can be estimated from the tract means. Once again, if individuals are grouped homogeneously, equation (3) applies to continuous variables as it does to dichotomous variables. So the aggregate correlation will again be larger than the individual correlation, but the true relation is represented by the regression equation.

If the individual data were available, the correlation coefficient would very likely be the summary measure used to describe the relationship. The discrepancy between individual and aggregate correlations is therefore especially misleading for continuous variables. However, if there is a clearly understood relation of cause and effect between the two variables, the regression equation provides a more informative description of the relationship (Bohrnstedt and Carter, 1971: 118-20; Cain and Watts, 1970: 229), and it can be inferred from the aggregate data without the bias that the correlation coefficient incorporates.

Robinson's finding about ecological correlations can thus be understood as a consequence of the unsurprising fact that people who become neighbors are likely to resemble each other in characteristics that sociologists investigate. These similarities, though they affect statistical relations, do not make it impossible to draw inferences from aggregate data when the statistical relations are understood.

The individual relationship (as measured by the regression coefficients) can be inferred directly from the aggregate relationship if the relationship between variables is identical in all tracts. That requirement is far too rigid, however, and unlikely to be met in the real world. We must consider two ways in which the relationship between variables can vary from tract to tract. The first, random error, does not seriously affect the applicability of all the foregoing. The second, aggregation bias, is more serious: under many circumstances the relation between independent and dependent variable will vary systematically across tracts. This variation in the relationship, however, can also be understood as a consequence of social processes, and the analyst's knowledge of those processes can often tell him whether aggregation bias is likely to be great enough to destroy the

⁶It is further assumed that the relations are linear. If individual relations are not linear, or if the summary measures for units are not linearly related to the individual values, there will necessarily be bias in the aggregation relations (Hannan, 1971:497).

validity of ecological inferences.

The case of random error has so far only been mentioned in passing, but it can be disposed of quickly. In the absence of random error, the scatterplot of the relation between two aggregate variables will form a perfect straight line and all the points will fall exactly on it. This condition is highly unlikely, as anyone who has dealt with aggregate data knows. A less than perfect correlation at the individual level permits the grouping process to form units in which the regression lines differ by chance from the regression line for the whole population. But, as Goodman (1959: 612) has shown, the demand for identical within-tract regression lines can be replaced by a less stringent one: that the deviations of the within-unit regression lines from the population regression line not be associated with the independent variables; or, in the case of discrete variables, that the expected value of the within-unit rates not be associated with the independent variables.

The extension to the multivariate case is straightforward, for if the same regression plane (or hyperplane) describes the whole population, then the point representing the within-unit mean of every unit must fall on the plane, and one can use these means to derive the population regression coefficients. The same allowance for random error must be made.

A frequently encountered case implying a multivariate model arises when the independent variable is discrete and polytomous at the individual level. In that case, the aggregate variables are the proportions falling into each category. They can jointly be entered as independent variables in a regression equation (omitting one to avoid a linear dependency), and within-group rates can be derived from their regression coefficients as in equation (6).

AGGREGATION BIAS

Aggregation bias is present in a set of data if the aggregate proportions or rates vary systematically with the tract values of the independent variables. For example, in the two-by-two case described in Table 1, aggregation bias would be present if either of the group-specific rates p and q varied systematically with the tract marginal X_j . This would produce a nonlinear relationship over tracts;

in the case of continuous variables, aggregation bias would be present if the within-unit intercept or regression coefficients were associated with any of the independent variables. When aggregation bias is present (this, indeed, is the precise definition of aggregation bias), the relations in aggregate data differ from the relations in individual data.

There are two circumstances of sociological significance that create aggregation bias. The first of these is the phenomenon generally described as contextual effects, when the relationship between an independent and a dependent variable in a unit is a function of the level of the independent variable in the unit. The second can be described as "selection by the dependent variable," when the dimension of similarity governing the grouping of members into units is the dependent variable in a relationship being examined, or is more closely associated with the dependent variable than would be expected from the independent variable alone.⁷

Let us consider the way in which an aggregate relation arises from an individual relation under the condition of aggregation bias. The statistical relations, whether they arise from contextual effects or from selection by the dependent variable (or from any other form of aggregation bias⁸) are formally identical; they are best described by analysis of covariance with interaction (as has often been pointed out both for contextual effects and for aggregate data analysis generally; see Schuessler, 1969; Fennessey, 1968; Hauser, 1972: 11-26). The relation between X and Y can be expressed in a regression equation which takes account of tract membership (the notation closely follows that of Fennessey, 1968:20-1):

$$Y_{ji} = a + b_1t_j + b_2X_{ji} + b_3t_jX_{ji} \quad (8)$$

where Y_{ji} and X_{ji} are individual variables and t_j is a dummy variable for the tract in which

⁷These correspond roughly to what Boudon calls, respectively, "effects of structure" and "effects of frequency" (i.e., effects of the distribution of the dependent variable) (Boudon, 1963:292-3).

⁸That is, from any source of aggregation bias in which the aggregate variables are linear functions of the individual variables. Tract means land proportions, the kinds of tract summary measures whose use is discussed here, are examples of such linear functions.

individual i resides. Since t_j is a dummy variable which takes on the value of 1 for one and only one j for each individual, equation (8) can be rewritten as

$$Y_{ji} = (a + b_{1j}) + (b_{2j} + b_{3j})X_{ji} \quad (9)$$

the term within the first set of parentheses representing the intercept, unique for each tract, and the term within the second set of parentheses representing the slope, also unique for each tract.

The assumption of no aggregation bias is that the regression line is identical in all tracts, or that $b_{1j} = 0$ and $b_{3j} = 0$ for all j (or that their expected values are zero for all X_j). If there is no aggregation bias, then, there is no relation between tract and the dependent variable except to the extent that there is selection by the independent variable. Equation (9) then reduces to

$$Y_{ji} = a + b_2X_{ji} \quad (10)$$

This is again the case of homogeneous grouping: the relation between X and Y is identical in all tracts, the mean in every tract falls (within the deviation allowed by chance) on the regression line, and the regression line derived from the means or aggregate variables is an unbiased estimate of the regression line that would be derived from the individual data.

If there is selection by the dependent variable, or if there is a contextual effect of X_j , either or both of b_{1j} and b_{3j} will be nonzero for some tracts. If so, the relation between tract means \bar{X}_j and \bar{Y}_j is not due only to the individual-level relation between X_{ji} and Y_{ji} ; it arises in part from the effects of tract membership. Since tract membership, therefore, creates a "spurious" relation between aggregate variables, they do not provide unbiased estimates of the relations that would be found in individual variables.

Contextual effects. In the case of contextual effects, it is obvious that by definition the condition of no aggregation bias is not met, for the relation between variables is a function of the level of the independent variable. Contextual effects can change either or both of these coefficients—either the intercept or the slope or both is a function of the average level of X in the tract (cf. Davis et al., 1961).

A clear example of such a contextual effect is one which Robinson provided in his article

warning about the dangers of inference from aggregate data. He demonstrated that the relation between race and illiteracy at the aggregate level was far closer than their relation at the individual level. He did not show, however, that the increase in the correlation arose from two quite distinct sources: first, since the racial distribution is quite uneven through the country (and was even more so in 1940, the year of the census data he analyzed), one would expect the aggregate correlation to be larger than the individual correlation as a consequence of homogeneity on the independent variable. If one could assume that the rates of illiteracy for blacks or whites did not vary systematically with racial composition, the regression of the area proportions could be used to estimate the proportion of illiterates in each race.

However, illiteracy rates in the United States, for both blacks and whites, have historically been higher in that part of the country which has the heaviest concentration of blacks. Key's interpretation of southern politics (Key, 1949) argues that the illiteracy rate is causally determined by the contextual effect of the proportion black. For, Key argues, the large black population led to the perpetuation of a one-party system and non-competitive politics, which in turn led to a low provision of public services, especially education, for the whole population of the southern states.

Consider this relationship in terms of equation (9), letting X_{ji} be a dummy variable which is 1 if the individual is black and 0 if he is white; Y_{ji} be a dummy variable which is 1 if the individual is illiterate and 0 if he is literate. Then \bar{X}_j is the proportion of the tract's (state's, region's) population which is black, and \bar{Y}_j is the proportion which is illiterate. In tract j $(a + b_{1j})$ is the proportion of whites who are illiterate, and $(a + b_{1j} + b_2 + b_{3j})$ is the proportion of blacks who are illiterate. Both b_{1j} and b_{3j} vary directly with \bar{X}_j ; both are larger if there is a larger black population in the tract. Thus, illiteracy is a function not only of an individual's race, but also of the racial composition of the state or region. The aggregate data by themselves do not permit one to estimate the proportion of illiterates in either race for the whole population.

More straightforward contextual effects

occur when the concentration of members of a particular category affects their own behavior, which may be called the "class-consciousness effect," or affects the behavior of members of the opposite category. In the first case, the "class-consciousness effect" (cf. Przeworski and Soares, 1971), b_{3j} is large where \bar{X}_j is large; in the second case (e.g. that whites are more likely to be Klansmen where there is a large black population), b_{1j} is large where \bar{X}_j is large⁹ (for another example, see MacRae, 1955).

Another form of contextual effect, which Blau calls an "inverse" effect, appears at first glance to be quite different but falls readily into the same framework. An inverse contextual effect arises when a variable has a positive effect at the individual level, but a negative effect at the aggregate level. Blau finds that competitiveness makes an individual more productive than the other members of his work group, but a high average level of competitiveness in the group decreases the group's overall productivity (Blau, 1968:77-8). In terms of equation (9), b_2 is positive, reflecting the direct effect of competitiveness on an individual's productivity, but b_{1j} is negative where \bar{X}_j is high. In such cases the aggregate correlation (and the aggregate regression) would imply a lower relation (and quite possibly a negative one) between the two variables, and would lead to an inaccurate estimation of the individual relationship if the differences between aggregate units were not taken into account.

Selection by the dependent variable. The second sociological process that can give rise to aggregation bias is selection into a tract by the dependent variable. If people tend to group themselves homogeneously with respect to their values on the dependent rather than an independent variable (I refer, of course, to independent and dependent variables in the relationship one is examining), the condition of identical relations within tracts will not be met. This phenomenon has often been described by saying that a variable independent at the individual level may be dependent at the aggregate level.

For example, race has a direct effect on income (Siegel, 1965). But income affects

one's ability to choose one's residence, so mean income is a determinant of the racial composition of a neighborhood. If residential segregation by race is only a consequence of de facto economic segregation rather than of overt racial discrimination, people of all races in a tract will tend to have similar incomes. In such a case, the coefficients of equation (11) would take the following form: b_{1j} would vary widely among tracts and $(b_2 + b_{3j})$ would be near zero, so that individuals in the same tract would tend to have similar incomes whatever their race. Again, the aggregate variables by themselves would be insufficient to estimate the individual-level relation between race and income. (If, on the other hand, overt racial discrimination rather than economic segregation is the primary mechanism assigning people to residences, the aggregate relation will provide an unbiased estimate of the individual relation.)

In either the case of a contextual effect or the case of selection by the dependent variable, failure to take account of aggregation bias amounts to a specification error, preventing accurate estimation of relationships because terms associated with area are omitted from the estimating equation.¹⁰ If either or both of b_{1j} and b_{3j} are related to the average level of X and an investigator attempts to estimate the effect of X from aggregate data, he will under- or overestimate its effect because variables correlated with it have been omitted from the model.¹¹ The error due to the use of aggregate data in such cases is no different in principle from error due to any other misspecified relation.¹²

¹⁰ Selection by dependent variable and contextual effects can of course occur in combination with each other and/or with selection by the independent variable. As Hannan (1970:62) points out, simultaneous selection by the independent and dependent variables can create a "spurious" association between two variables at the aggregate level as can be readily demonstrated in the analysis of covariance framework.

¹¹ It is possible that some b_{1j} are not zero, but that, across tracts, b_{1j} and \bar{X}_j are independent of each other. If so, and if all b_{3j} are zero, the aggregate regression will give an unbiased estimate of b_1 .

¹² It might be pointed out that other forms of specification error can occur in the analysis of aggregate data just as they can in the analysis of individual data: whenever a variable which is correlated with the independent variable being analyzed and which has an effect on the dependent variable is omitted from consideration, the effect of the included variable will be misestimated. The remedy is

⁹ And, in compensation, b_{3j} is equally large, but negative, since the slope for (in this example) blacks contains the sum of b_{1j} and b_{3j} .

JUDGING AGGREGATION BIAS

The researcher's primary concern, of course, is to decide in a given case whether his aggregate data are affected by aggregation bias. If he judges that they are not, he can reasonably use them for inferences to individual-level relations. There are several tests for aggregation bias, though none can conclusively establish its absence.

If the relation between variables is the same in every tract — that is, if there is no aggregation bias — the relation among tract measures is necessarily linear. A curvilinear relationship is in itself evidence of either a contextual effect or selection by the dependent variable.¹³ To determine whether the relation is a linear one, a scatterplot can be examined or a test of linearity can be applied (such as that in Walker and Lev, 1953:245-6; this test is strictly applicable only if the independent variable is intervally scaled and has a small number of values, but it can be applied to a continuous variable by grouping its values into discrete categories).

Another test is proposed by Duncan et al. (1961:71). Ordinarily, the means of the independent and dependent variables for the whole sample will be known. If the sample mean of the independent variable is substituted into the prediction function derived from aggregate data, the predicted value should approximate the sample mean of the dependent variable.

Finally, if the independent variable is discrete, the regression coefficients can be transformed by equation (6) to estimate within-group rates. Rates necessarily must fall between zero and one, but if aggregation bias is present, the estimated "rates" will often fall outside that range. Any rates significantly less than zero or greater than one can be taken as *prima facie* evidence of aggregation bias.

These statistical tests are weak tests: they can conclusively prove that aggregation bias is present, but opposite findings do not prove its absence. Equally important, therefore, is the kind of evidence provided by sociological theory. Aggregation bias depends on social

processes, and when it is so understood the investigator will often be able to determine its presence or absence on the basis of theory even when data are available only for the aggregate level. On the basis of such theory the investigator can determine whether or not to make an ecological inference.

In many sociological investigations, it seems plausible that selection by the independent variable is far more important than selection by the dependent variable. Voting is perhaps the most common subject of aggregate-data studies; with a few exceptions such as university towns (for another exception, see Rosskam, 1972), it seems likely that peoples' residence is determined by the same factors that determine their politics rather than by their politics directly.

The ecological analysis of mental disorders, on the other hand, is probably contaminated by selection by the dependent variable. The mentally ill may be concentrated in certain areas of cities because the characteristics of these areas produce mental disorders or because the mentally ill from other areas drift into them (Faris and Dunham, 1939: 163-77; Dohrenwend, 1966:15). As long as that debate remains unresolved, aggregate data cannot be used to determine the etiology of mental illness.

Another case of selection by the dependent variable is described by Orwell, who claims that poor but talented boys like himself were admitted to British prep schools so that their headmasters could entice the sons of the rich with reports of scholarships won to Eton (Orwell, 1953: 18-19). If his case is typical, data on schools will show a closer relationship between parents' status and students' performance than would data on the students themselves.

Demonstrations of the effect of context in the education process (e.g. Sewell and Armer, 1966) offer a second reason for believing that aggregate data cannot be relied on to estimate the effects of background on students' academic performance. Similarly, while voting studies are probably free of the problem of selection by the dependent variable, voting is more likely to be affected by context (cf., e.g., Allardt and Pesonen, 1967:343; Stokes, 1969). If these effects are significant, party preferences among different groups of the population cannot be determined from aggregate data.

of course to undertake a multivariate analysis, in which the omitted variable is included.

¹³ Under certain rather restrictive assumptions, a curvilinear model may be fitted to the data to estimate the parameters of the individual-level relationship. See Boudon, 1963; Przeworski and Soares, 1971.

In all of these cases, the decision for or against ecological inference should be based on an informed assessment of the importance of the causal process being investigated relative to the biasing effects of context and of selection by the dependent variable.

DISCUSSION

It has long been recognized that data-gathering is itself a social process, and that, for example, the interaction between interviewer and respondent in a sample survey can affect the responses (Payne, 1951; Cicourel, 1964). Naturally occurring social processes not in the control of the investigator can also affect (if not precisely "contaminate") the manner in which phenomena are translated into data. For example, police practices affect crime statistics and criminal records so that the full incidence of criminal acts is not recorded (Black, 1970). In this paper I have demonstrated the effect of social processes on aggregation relations—that is, on the correspondence, or lack of correspondence, between relationships in individual-level data and those in aggregate-level data. While the statistical formulations presented here are not original, the recognition of their roots in social process provides the researcher with a means for estimating their parameters.

To recapitulate, if individuals are grouped into tracts according to an independent variable, the correlation between tract-level measures will expectably be higher than the correlation between individual-level measures, but the regression will not be affected, so that aggregate data can be used to estimate the individual relation.

If the relation between the two variables incorporates a contextual effect, or if individuals are selected into tracts by the dependent variable rather than the independent variable, the correlation coefficient can change in either direction and in any degree. Moreover, the linear regression of tract means will bear no necessary relation to the regression of individual values (and if there is an appreciable contextual effect, one would not wish to estimate the relation between individual values without taking account of it anyway). Thus, if the second source of error is present, inferences cannot justifiably be drawn from aggregate to individual relations. If only the first source is present, however, such inferences can be drawn.

I should point out that this paper's title, referring to two sources of "error" in ecological correlations, is perhaps misleading. These are sources of error only insofar as one wishes to make inferences across levels. The aggregate correlations are correct as descriptions of the relationships between the tract values of the variables (Goodman, 1959: 611; Menzel, 1950:674), and it is entirely proper to use them in investigations in which the tract is the fundamental unit of analysis.

Cross-level inference need not be, as Stokes calls it, a "game against nature" (Stokes, 1969); it can instead be confidently based on the applicability of social theory to the situation one is investigating. Our understanding of aggregation relations will be further enriched by more studies like Slatin's (1971) and Stokes's own, both of which compare data on the same phenomenon at different levels of analysis. Such studies will not only warn us against straightforward ecological inference where it is fallacious. They can also provide us with expectations about the size of terms representing contextual effects or selection by a dependent variable, ultimately enabling us to estimate the missing terms in analyses for which data are available at only one level. We will then be able to make reasonable "ecological inferences" even when we know or suspect that aggregate relations do not correspond to individual ones. Such inferences must of course be grounded not only in empirical evidence from studies like these but also in an understanding of the social processes that determine aggregation relations.

REFERENCES

- Allardt, Erik and Pertti Pesonen
1967 Cleavages in Finnish Politics. Pp. 325-66 in Seymour M. Lipset and Stein Rokkan, ed., *Party Systems and Voter Alignments: Cross-National Perspectives*. New York: Free Press.
- Black, Donald J.
1970 "Production of crime rates." *American Sociological Review* 35 (August):733-48.
- Blalock, Hubert M., Jr.
1964 *Causal Inferences in Nonexperimental Research*. Chapel Hill: University of North Carolina Press.
- Blau, Peter M.
1963 *The Dynamics of Bureaucracy: A Study of Interpersonal Relations in Two Government Agencies*. Chicago: University of Chicago Press, second edition.

- Bohmstedt, George W. and T. Michael Carter
1971 "Robustness in regression analysis." Pp. 118-46 in Herbert L. Costner, ed., *Sociological Methodology 1971*. San Francisco: Jossey-Bass.
- Boudon, Raymond
1963 Propriétés individuelles et propriétés collectives: un problème d'analyse écologique." *Revue Française de Sociologie* 4 (July-September): 275-99.
- Cain, Glen G. and Harold W. Watts
1970 "Problems in making policy inferences from the Coleman report." *American Sociological Review* 35 (April): 228-42.
- Cicourel, Aaron V.
1964 *Method and Measurement in Sociology*. New York: Free Press.
- Cohen, Jacob.
1968 "Multiple regression as a general data-analytic system." *Psychological Bulletin* 70:426-43.
- Davis, James A. et al.
1961 "A technique for analyzing the effects of group composition." *American Sociological Review* 26(April): 215-25.
- Dohrenwend, Bruce P.
1966 "Social status and psychological disorder: an issue of substance and an issue of method." *American Sociological Review* 31(February): 14-35.
- Duncan, Otis Dudley, Ray P. Cuzzort, and Beverley Duncan
1961 *Statistical Geography: Problems in Analyzing Areal Data*. Glencoe: Free Press.
- Faris, Robert E. L. and H. Warren Dunham
1939 *Mental Disorders in Urban Areas: An Ecological Study of Schizophrenia and Other Psychoses*. Chicago: University of Chicago Press.
- Fennessey, James
1968 "The general linear model: a new perspective on some familiar topics." *American Journal of Sociology* 74(July): 1-27.
- Goodman, Leo A.
1959 "Some alternatives to ecological correlation." *American Journal of Sociology* 64 (May): 610-25.
- Hauser, Robert Mason
1972 *Socioeconomic Background and Educational Performance*. Washington: American Sociological Association.
- Hannan, Michael T.
1970 *Problems of Aggregation and Disaggregation in Sociological Research*. Chapel Hill: University of North Carolina Institute for Research in Social Science.
1971 "Problems of aggregation." Pp. 473-508 in Hubert M. Blalock, Jr., ed., *Causal Models in the Social Sciences*. Chicago: Aldine Atherton.
- Key, V. O., Jr.
1949 *Southern Politics in State and Nation*. New York: Vintage Books.
- Lee, T.C., G. G. Judge, and A. Zellner
1970 *Estimating the Parameters of the Markov Probability Model from Aggregate Time Series Data*. Amsterdam: North-Holland Publishing Company.
- MacRae, Duncan, Jr.
1955 "Occupations and the congressional vote, 1940-1950." *American Sociological Review* 20 (June): 332-40.
- Menzel, Herbert
1950 "Communication on Robinson's 'Ecological Correlations and the Behavior of Individuals.'" *American Sociological Review* 15(October): 674.
- Orwell, George
1953 *Such, Such Were the Joys*. New York: Harcourt, Brace and Company.
- Payne, Stanley L.
1951 *The Art of Asking Questions*. Princeton: Princeton University Press.
- Przeworski, Adam and Glauco A. D. Soares
1971 "Theories in search of a curve: a contextual interpretation of left vote." *American Political Science Review* 65 (March): 51-68.
- Robinson, W.S.
1950 "Ecological correlations and the behavior of individuals." *American Sociological Review* 15(June): 351-7.
- Roskam, Edwin
1972 *Roosevelt, New Jersey: Big Dreams in a Small Town and What Time Did To Them*. New York: Grossman.
- Schuessler, Karl.
1969 "Covariance analysis in sociological research." Pp. 219-44 in Edgar F. Borgatta, ed., *Sociological Methodology, 1969*. San Francisco: Jossey-Bass.
- Sewell, William H. and J. Michael Armer.
1966 "Neighborhood context and college plans." *American Sociological Review* 31(April): 159-69.
- Siegel, Paul M.
1965 "On the cost of being a Negro." *Sociological Inquiry* 35 (Winter): 41-57.
- Slatin, Gerald T.
1969 "Ecological analysis of delinquency: aggregation effects." *American Sociological Review* 34(December): 894-907.
- Stokes, Donald E.
1969 "Cross-level inference as a game against nature." Pp. 62-83 in Joseph L. Bernd, ed., *Mathematical Applications in Political Science*. Vol. 4. Charlottesville: University Press of Virginia.
- Walker, Helen M. and Joseph Lev
1953 *Statistical Inference*. New York: Holt, Rinehart and Winston.

APPENDIX

I show the relation between individual and aggregate correlations, and between individual and aggregate regressions, first for the general case and then for the cases of random and of homogeneous grouping. Throughout it is assumed that individuals reside in m tracts, all of size two. Lower-case x and y represent variables expressed in standard form, so that their means, for individuals, are both 0, and their standard deviations are both 1. (Standardizing variables has no effect on the correlation between them.)

The aggregate variables are

$$\bar{x}_j = \frac{x_{j1} + x_{j2}}{2} ; \quad \bar{y}_j = \frac{y_{j1} + y_{j2}}{2}$$

The standard deviation of the aggregate variable \bar{x}_j is

$$\sigma_{\bar{x}_j} = \frac{\sigma_{x_{j1} + x_{j2}}}{2} = \sqrt{\frac{1}{4} \left(\sigma_{x_{j1}}^2 + \sigma_{x_{j2}}^2 + 2r_{x_{j1}x_{j2}} \sigma_{x_{j1}} \sigma_{x_{j2}} \right)}$$

Since x is standardized, $\sigma_{x_{j1}}^2 = \sigma_{x_{j2}}^2 = 1$

$$\text{and } \sigma_{\bar{x}_j} = \frac{1}{2} \sqrt{2(1 + r_{x_{j1}x_{j2}})} \quad (11)$$

$$\text{and, similarly, } \sigma_{\bar{y}_j} = \frac{1}{2} \sqrt{2(1 + r_{y_{j1}y_{j2}})} \quad (12)$$

Then the "ecological correlation"

$$r_{\bar{x}_j \bar{y}_j} = \frac{\frac{1}{m} \sum_{j=1}^m \left(\frac{x_{j1} + x_{j2}}{2} \right) \left(\frac{y_{j1} + y_{j2}}{2} \right)}{\sigma_{\bar{x}_j} \sigma_{\bar{y}_j}}$$

$$\begin{aligned} &= \frac{\frac{1}{4} \frac{1}{m} \sum_{j=1}^m (x_{j1}y_{j1} + x_{j2}y_{j2} + x_{j1}y_{j2} + x_{j2}y_{j1})}{\frac{1}{4} \sqrt{4(1 + r_{x_{j1}x_{j2}})(1 + r_{y_{j1}y_{j2}})}} \\ &= \frac{r_{x_{j1}y_{j1}} + r_{x_{j2}y_{j2}} + r_{x_{j1}y_{j2}} + r_{x_{j2}y_{j1}}}{2 \sqrt{(1 + r_{x_{j1}x_{j2}})(1 + r_{y_{j1}y_{j2}})}} \\ &= \frac{2(r_{x_{ji}y_{ji}} + r_{x_{ji}y_{jk}})}{2 \sqrt{(1 + r_{x_{j1}x_{j2}})(1 + r_{y_{j1}y_{j2}})}} \\ &= \frac{r_{x_{ji}y_{ji}} + r_{x_{ji}y_{jk}}}{\sqrt{(1 + r_{x_{j1}x_{j2}})(1 + r_{y_{j1}y_{j2}})}} \quad (13) \end{aligned}$$

In this expression, $r_{x_{j1}x_{j2}}$, $r_{y_{j1}y_{j2}}$, and $r_{x_{ji}y_{jk}}$ are the correlations between the values of "neighbors," and $r_{x_{ji}y_{ji}}$ is the correlation between an individual's x and his y , and is thus equal to the correlation r_{xy} for the sample. This result appears above as equation (1).

From the definition of random grouping,

$$r_{x_{j1}x_{j2}} = r_{y_{j1}y_{j2}} = r_{x_{ji}y_{jk}} = 0 \quad (14)$$

so that in the case of random grouping, the aggregate correlation is

$$r_{\bar{x}_j \bar{y}_j} = r_{x_{ji}y_{ji}} = r_{xy} \quad (15)$$

This result appears above as equation (2).

From the following we see that the regression slope is also identical for individual and aggregate variables if individuals are grouped randomly. If X and Y are in raw form, the individual regression slope is

$$b_{YX} = r_{xy} \frac{\sigma_y}{\sigma_x} \quad (16)$$

The same coefficient is derived from the aggregate variables: with X and Y in raw form, the standard deviation of the aggregate variable \bar{X}_j is

$$\sigma_{\bar{X}_j} = \sqrt{\frac{1}{4}(\sigma_{X_{j1}}^2 + \sigma_{X_{j2}}^2 + 2r_{X_{j1}X_{j2}}\sigma_{X_{j1}}\sigma_{X_{j2}})}$$

and $\sigma_{\bar{Y}_j}$ is similarly defined. Because the individuals are randomly grouped, the last

term in parentheses is zero.

Then

$$\begin{aligned} b_{\bar{Y}_j\bar{X}_j} &= r_{\bar{X}_j\bar{Y}_j} \frac{\sigma_{\bar{Y}_j}}{\sigma_{\bar{X}_j}} = r_{\bar{X}_j\bar{Y}_j} \frac{\sqrt{\frac{1}{4}(\sigma_{Y_{j1}}^2 + \sigma_{Y_{j2}}^2)}}{\sqrt{\frac{1}{4}(\sigma_{X_{j1}}^2 + \sigma_{X_{j2}}^2)}} \\ &= r_{\bar{X}_j\bar{Y}_j} \frac{\sqrt{2\sigma_{Y_{ji}}^2}}{\sqrt{2\sigma_{X_{ji}}^2}} = r_{xy} \frac{\sigma_{Y_{ji}}}{\sigma_{X_{ji}}} \\ &= r_{xy} \frac{\sigma_Y}{\sigma_X} = b_{YX} \end{aligned} \quad (17)$$

A similar derivation shows that the intercept term for the regression of aggregate variables is equal to the intercept for the corresponding individual regression.

Homogeneous grouping without aggregation bias implies that $r_{x_{j1}x_{j2}} > 0$, but that the relation between x and y is identical in every tract. This means that $r_{y_{j1}y_{j2}}$ differs from zero only to the extent expected on the basis of the correlation between x_{j1} and x_{j2} , i.e. that $r_{y_{j1}y_{j2}} = r_{xy}^2 r_{x_{j1}x_{j2}}$ and $r_{x_{ji}y_{jk}} = r_{xy} r_{x_{j1}x_{j2}}$. (It will be noted that these identities follow from the assumption that the partial correlations between neighbors' values, other than between x_{j1} and x_{j2} , are zero). Then, by substitution in equation (11), the aggregate correlation is

$$\begin{aligned} r_{\bar{X}_j\bar{Y}_j} &= \frac{r_{xy} + r_{xy}r_{x_{j1}x_{j2}}}{\sqrt{(1+r_{x_{j1}x_{j2}})(1+r_{y_{j1}y_{j2}})}} \\ &= r_{xy} \frac{(1+r_{x_{j1}x_{j2}})}{\sqrt{(1+r_{x_{j1}x_{j2}})(1+r_{xy}^2 r_{x_{j1}x_{j2}})}} \\ &= r_{xy} \sqrt{\frac{(1+r_{x_{j1}x_{j2}})}{(1+r_{xy}^2 r_{x_{j1}x_{j2}})}} \end{aligned} \quad (18)$$

which, as stated above, must be greater than r_{xy} because the radical sign contains an expression whose numerator is necessarily greater than its denominator. This result appears as equation (3) above.

The individual and aggregate regression slopes, however, are still identical if individuals are grouped homogeneously. As in equation (17),

$$b_{\bar{Y}_j\bar{X}_j} = r_{\bar{X}_j\bar{Y}_j} \frac{\sigma_{\bar{Y}_j}}{\sigma_{\bar{X}_j}} \quad (19)$$

But X_1 and X_2 are now correlated, and Y_1 and Y_2 are correlated, so

$$\begin{aligned} b_{\bar{Y}_j\bar{X}_j} &= r_{\bar{X}_j\bar{Y}_j} \\ &= \frac{\sqrt{\frac{1}{2}(\sigma_{Y_{j1}}^2 + \sigma_{Y_{j2}}^2 + 2r_{Y_{j1}Y_{j2}}\sigma_{Y_{j1}}\sigma_{Y_{j2}})}}{\sqrt{\frac{1}{2}(\sigma_{X_{j1}}^2 + \sigma_{X_{j2}}^2 + 2r_{X_{j1}X_{j2}}\sigma_{X_{j1}}\sigma_{X_{j2}})}} \\ &= r_{\bar{X}_j\bar{Y}_j} \sqrt{\frac{2\sigma_Y^2 + 2\sigma_Y^2 r_{Y_{j1}Y_{j2}}}{2\sigma_X^2 + 2\sigma_X^2 r_{X_{j1}X_{j2}}}} \end{aligned}$$

$$= \frac{\sigma_Y}{\sigma_X} r_{X_j Y_j} \sqrt{\frac{1 + r_{XY}^2 r_{X_{j1} X_{j2}}}{2}} \quad (20)$$

$$\text{So } b_{Y_j X_j} = r_{XY} \frac{\sigma_Y}{\sigma_X} = b_{YX} \quad (21)$$

It can again be shown that the intercept term for the aggregate regression is also equal to the intercept for the corresponding individual regression.

It should be emphasized that equations (16) through (21) apply to regression coefficients for variables in raw form only. Standardized coefficients (beta weights or path coefficients) are affected by aggregation, just as correlation coefficients are.

From equation (18), the above expression is

$$\text{equal to } r_{XY} \frac{\sigma_Y}{\sigma_X}$$

MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

NON-RANDOM EXOGENOUS VARIABLES IN PATH ANALYSIS*

DAVID L. KLEMMACK, THOMAS A. LEGGETTE AND LAWRENCE S. MAYER

Virginia Polytechnic Institute and State University

American Sociological Review 1973, Vol. 38 (December):778-84

Path analysis requires that exogenous and residual variables not be correlated. An alternative to assuming the two to be independent is to treat the exogenous variables as fixed or non-random. Examination of path models, however, reveals that the usual standardized path coefficients are biased and inconsistent when exogenous variables are considered fixed. At the same time, the path coefficient divided by the residual term is a large-sample unbiased estimate of the true coefficient. Variants of this basic transformation are discussed and an empirical example is included.

THE use of path analytic procedures is, as is any statistically based technique, bound by a set of assumptions. Violation of such assumptions leads to incorrect and, on occasion, potentially misleading results.¹ One assumption, critical in the use of path analysis, is that exogenous variables may not be correlated with the residual terms incorporated in the model (Duncan, 1966; Heise, 1968; Land, 1968; Blalock et al., 1970). Blalock et al. (1970) contend, to meet this assumption, the sociologist is in fact assuming that the exogenous variables are not themselves a function of some third, unspecified set of factors which also relate to the endogenous variables. Another approach to the problem largely ignored in the literature, is to treat the input or exogenous variables as fixed or non-random. This paper explores the consequences of adopting this second approach.

Although research always involves the use of certain simplifying assumptions, the specification of the assumptions to be used when analyzing any set of data is not to be taken lightly. While sociologists commonly assume exogenous variables to be non-correlated with residuals, such a practice often appears un-

justified, particularly when the residuals are large.² Were this assumption necessary for analyzing the data, the position taken by sociologists would be understandable if not entirely defensible. Such, however, is not the case.

Classically, regression analysis, the procedure on which path analysis is based, was developed under the assumption that the input or exogenous variables are fixed or non-random. In such a case, the question of whether an exogenous variable is correlated with a residual is irrelevant since fixed variables add no variance to the models they belong to (Johnston, 1963; Goldberger, 1964; Wonnacott and Wonnacott, 1970). This fact is reflected in the assumed form of the data in the population if the input variables are fixed:

$$(1) \quad Y_i = \alpha + \beta_{YX}x_i + \epsilon_i$$

where Y_i , $i = 1, \dots, n$, is the dependent variable which has mean $\alpha + \beta_{YX}x_i$ and variance σ^2 , ϵ_i , $i = 1, \dots, n$, is the error component with mean 0 and variance σ^2 , and x_i , $i = 1, \dots, n$, is the fixed exogenous variable.³ Note, in particular, that "... the dis-

* The authors would like to acknowledge the partial support of the Department of Agriculture and a National Institute of Mental Health grant, No. 1 RO3 MH 23177 01.

¹ Violation of assumptions always leads to strictly incorrect results. The bias may, however, be so minimal that the results, although incorrect, are not misleading.

² Since residual terms are defined so as to include all predictor variables not explicitly included in the model, a large residual implies much has been omitted. Under such conditions, it would appear likely that a third variable which is correlated with both exogenous and endogenous variables exists.

³ The multiple exogenous variable analog of equation (1) expressed in vector notation is

$$Y = X\beta + \epsilon$$

where $Y = (Y_1, \dots, Y_n)'$, $\epsilon = (\epsilon_1, \dots, \epsilon_n)'$, $\beta = (\beta_1, \dots, \beta_p)'$, $X = (x_{ij})$ $i = 1, \dots, n$, $j = 1, \dots, p$.

tribution of ϵ is just the distribution of Y translated onto a zero mean (Wonnacott and Wonnacott, 1970:17).³ The question of whether the exogenous variable is correlated with the residual does not become an issue in model (1) since the exogenous variable is not treated as random (Wonnacott and Wonnacott, 1970:38).

Treatment of exogenous variables appearing in a given regression model as fixed has several consequences not all of which are favorable. On the positive side, the problem of correlated residual and exogenous variables is avoided. Thus, $\hat{\beta}_{yx}$, the least squares estimator of β_{yx} , [see equation (1)] is (provided the remaining assumptions are met) consistent and unbiased. On the negative side, the generalizability of any findings is somewhat limited and the consideration of measurement error in the exogenous variables is explicitly excluded.⁴ Finally, if the least squares (regression) estimator is an excellent estimator of the non-standardized path coefficient regardless of whether the exogenous variables in the model are fixed or stochastic but uncorrelated with certain residual terms. For the standardized path model we present several competing estimators including the usual beta coefficient. Then, relying on the work of Younger (1972), we show that the appropriate estimator of a standardized path coefficient depends on which of the exogenous variables are assumed to be fixed. We then present methods for computing the appropriate estimators by exploring a sociological example. model is in standardized form, it can be shown that the usual estimator of the standardized regression coefficient is biased, inconsistent, and, to make matters worse, has an extremely complex distribution.

These features will be considered in the context of the sociological path model which relies heavily on regression theory. First we will review the non-standardized and standardized path models. We will then introduce estimators of the non-standardized and standardized path coefficients. The usual

ALTERNATIVE ESTIMATORS OF STANDARDIZED PATH COEFFICIENTS

Path analysis techniques were initially designed to be used in situations (1) where the data are subject to minimal sampling and measurement error and (2) where the causal ordering of variables is relatively certain (Wright, 1921, 1934; Heise, 1968). Usually, however, sample rather than population data are available necessitating a distinction between estimating (sample-based) and structural (population-based) equations as well as creating problems for efficiently estimating path coefficients. Furthermore, since data may often be explained by alternative models, the relative sizes of individual path coefficients (compared with their standard errors) are often used to help determine the model's non-zero paths. Hence, knowledge of the expected values and distribution characteristics of path coefficient estimators becomes useful.

In addition to assumptions regarding the nature of the distribution of residual variables, one other condition influences the properties of path coefficient estimators. It is a fact that estimating equations are typically constructed in terms of standardized variables since this facilitates calculations and enables a ready determination of the relative contribution of exogenous variables on a single endogenous variable.⁵ The sociologist must be aware that the parameters to be estimated are different in the standardized than in the non-standardized case. The structural equation for the simple path diagram shown in Figure 1, when expressed in

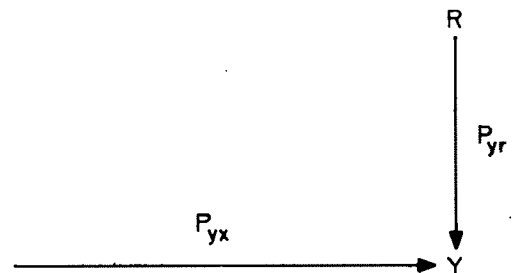


FIGURE 1. SIMPLE PATH DIAGRAM SHOWING A SINGLE ENDOGENOUS VARIABLE AS A FUNCTION OF AN EXOGENOUS VARIABLE X AND A RESIDUAL TERM R .

³ n and where it is assumed $E(\epsilon) = 0$, $\text{cov}(\epsilon) = \sigma^2 I$, $E(Y) = X\beta$, and $\text{cov}(Y) = \sigma^2 I$ (Wonnacott and Wonnacott, 1970:2423).

⁴ Measurement error in exogenous variables is, of course, a problem whether the exogenous variables are fixed or random (Blalock et al., 1970).

⁵ The issue of whether variables "should" be standardized has been considered by many others (Tukey, 1954; Wright, 1960; Blalock, 1971).

non-standardized form, is:

$$(2) \quad Y = P_{yx}x + P_{yr}r$$

where P_{yx} and P_{yr} are the non-standardized path (regression) coefficients, x is the exogenous variable expressed in deviation form, r is the residual variable expressed in deviation form, and Y is the endogenous variable.⁶ The standardized form of the same structural equation is

$$(3) \quad z_y = \frac{\sigma_y}{\sigma_x} P_{yx} z_x + \frac{\sigma_x}{\sigma_y} P_{yr} z_r$$

or

$$(3') \quad z_y = p_{yx} z_x + p_{yr} z_r$$

where z_x , z_y , and z_r are x , y , and r expressed in standardized form; i.e., with mean 0 and standard deviation 1, and σ_x , σ_y , and σ_r represent the true standard deviations for the non-standardized exogenous, endogenous, and residual variables respectively.

Examination of equation (3) may suggest that a natural estimator of p_{yx} , the stan-

dardized path coefficient, is $\hat{b}_{yx} = \frac{s_x}{s_y} \hat{\beta}_{yx}$, the

beta coefficient presented in many standard texts (Blalock, 1972; Hays, 1963). Although it can be shown that \hat{b}_{yx} is a "good" estimator of p_{yx} if x is random, such is not the case if x is fixed. The difficulty, if the exogenous variable is fixed, is that s_y is a biased and inconsistent estimator of σ_y . Examination of the non-standardized regression equation (1) suggests that the sample variance of y can be written:

$$(4) \quad s_y^2 = s_x^2 \hat{\beta}_{yx}^2 + s_{y \cdot x}^2$$

an equation appearing in many texts (Hays, 1963; Blalock, 1972; Wonnacott and Wonnacott, 1970).⁸ However, if the exogenous

variable is fixed, then

$$(5) \quad E(s_y^2) = E(s_x^2 \hat{\beta}_{yx}^2 + s_{y \cdot x}^2) = s_x^2 \beta_{yx}^2 + \sigma_y^2,$$

a value clearly larger than σ_y^2 . Therefore, if the exogenous variable is fixed, the sample variance σ_y^2 is not an unbiased estimator of the true variance σ_y^2 . Furthermore, if the sample size is large then:

$$(6) \quad E(b_{yx}) = E\left(\frac{s_x}{s_y} \hat{\beta}_{yx}\right) \approx \frac{s_x}{\sqrt{s_x^2 \beta_{yx}^2 + \sigma_y^2}} \beta_{yx},$$

a parameter clearly different from p_{yx} .

Equation (5) does, however, suggest an alternative estimator of the standardized path coefficient if the exogenous variable is fixed. Since $E(s_{y \cdot x}^2) = \sigma_y^2$ and since the parameter to be estimated is:

$$(7) \quad p_{yx} = \frac{\sigma_x}{\sigma_y} \hat{\beta}_{yx},$$

substitution suggests defining a new estimator:

$$(8) \quad b^*_{yx} = \frac{s_x}{s_{y \cdot x}} \beta_{yx}.$$

Younger (1972) and Mayer and Younger (1972) have demonstrated that b^*_{yx} is, in fact, an asymptotically unbiased, consistent estimator of p_{yx} when the exogenous variable is fixed. Furthermore, they show $t = (n-2)^{1/2} b^*_{yx}$ is distributed as a t -distribution with $n-1$ degrees of freedom under the null hypothesis $H_0: p_{yx} = 0$ making hypothesis testing relatively simple.¹⁰

An alternative estimator of p_{yx} particularly attractive if only a small sample is available is:

$$(9) \quad b_{yx} = \gamma(p, n) b^*_{yx}$$

where $\gamma(p, n) = 2^{1/2} \Gamma[(n-p)/2] / (n-p)^{1/2} \Gamma[(n-p-1)/2]$, $\Gamma(\alpha)$ is the usual gamma function, and p is the number of regressor variables. If α is a positive integer, then $\Gamma(\alpha) = (\alpha-1)!$. Mayer and Younger (1972) show that b_{yx} is a best estimator of p_{yx} in

⁹ This equation follows since x is non-random, and thus s_x^2 is constant in the model implying $E(s_x^2) = s_x^2$.

¹⁰ The distributional statements require that the residual terms be normally distributed; a requirement also found in the non-standardized regression model.

⁶ We assume that the exogenous variable is either non-random or random and not correlated with the residual term, and the other conditions underlying regression analysis.

⁷ Since σ_x^2 , the population variance of the exogenous variable, is not defined when x is fixed, an alternative value must be used. The natural indicator would be $\frac{\sum(x_i - \bar{x})^2}{n}$, the average squared deviation about the mean of the x scores. Since, computationally, this is s_x^2 if x were random, we employ the label s_x^2 .

⁸ $s_{y \cdot x}^2$ is the sample residual variance and is defined as $(1-R^2)s_y^2$, where R^2 is the multiple correlation squared and s_y^2 is the sample variance of the endogenous variable.

the sense of being minimum variance unbiased. However, for samples of reasonable size ($n/p \geq 25$) b^*_{yx} and \tilde{b}_{xy} are almost identical and thus we recommend b^*_{yx} since it is easier to compute.

AN EMPIRICAL EXAMPLE

Consider a simple model with two exogenous variables determining an endogenous variable, with these three then determining yet another endogenous variable (see Figure 2). The endogenous variables, Y_1 and Y_2 , are treated as random; and the residual variables, R_u and R_v , are assumed to be uncorrelated (1) with each other and (2) with Y_2 and Y_1 respectively. The exogenous variables, X_a and X_b , may be treated as either random or fixed. If they are considered random, however, we must further assume that each is uncorrelated with the residual terms.¹¹ Whether the exogenous variables are treated as fixed or random, the structural equations for this example, expressed in non-standardized form, are:

$$(10) \quad Y_1 = P_{1a}x_a + P_{1b}x_b + P_{1u}r_u$$

$$(11) \quad Y_2 = P_{2a}x_a + P_{2b}x_b + P_{21}Y_1 + P_{2v}r_v$$

where x_a , x_b , r_u , r_v , and y_1 are the deviation forms of X_a , X_b , R_u , R_v , and Y_1 respectively. Furthermore, if the exogenous variables are

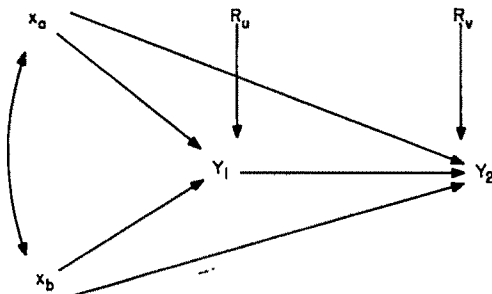


FIGURE 2. PATH DIAGRAM SHOWING Y_1 (SOCIAL PARTICIPATION) TO BE A FUNCTION OF TWO EXOGENOUS VARIABLES, x_a (EDUCATIONAL ATTAINMENT) AND x_b (OCCUPATIONAL PRESTIGE). A SECOND ENDOGENOUS VARIABLE, Y_2 (SOCIAL ISOLATION), IS ASSUMED TO BE A FUNCTION OF x_a , x_b , AND Y_1 . R_u AND R_v ARE RESIDUAL TERMS.

¹¹ If the predictors and residual terms are correlated, then the usual methods of path analysis are inappropriate, since econometric estimators and not the usual least squares estimators must be used to guarantee that the estimators of the non-standardized regression coefficients are consistent (Goldberger, 1964; Johnston, 1963).

treated as random, the estimating equations, expressed in standardized form, are:

$$(10') \quad z_1 = \frac{s_a}{s_1} \hat{\beta}_{1a} z_a + \frac{s_b}{s_1} \hat{\beta}_{1b} z_b + \frac{s_u}{s_1} \hat{\beta}_{1u} z_u$$

$$(11')$$

$$z_2 = \frac{s_a}{s_2} \hat{\beta}_{2a} z_a + \frac{s_b}{s_2} \hat{\beta}_{2b} z_b + \frac{s_1}{s_2} \hat{\beta}_{21} z_1 + \frac{s_v}{s_2} \hat{\beta}_{2v} z_v.$$

The usual path estimating procedures may, providing all underlying assumptions are met, be used to obtain estimates of the various path coefficients under any of the following three sets of assumptions: (1) the exogenous variables are fixed and the model is in non-standardized form, (2) the exogenous variables are random and the model is in non-standardized form, and (3) the exogenous variables are random and the model is in standardized form (see Table 1). In fact, the estimates of the non-standardized or non-random path coefficients are identical whether the exogenous variables are random.

The usual path coefficient estimates are inadequate only under the fourth set of assumptions; i.e., when the exogenous variables are fixed and the model is in standardized form. In this case, the appropriate estimating equations are:

$$(10'')$$

$$z_1 = \frac{s_a}{s_{1 \cdot ab}} \hat{\beta}_{1a} z_a + \frac{s_b}{s_{1 \cdot ab}} \hat{\beta}_{1b} z_b + \frac{s_u}{s_{1 \cdot ab}} \hat{\beta}_{1u} z_u$$

$$(11'')$$

$$z_2 = \frac{s_a}{s_{2 \cdot ab}} \hat{\beta}_{2a} z_a + \frac{s_b}{s_{2 \cdot ab}} \hat{\beta}_{2b} z_b + \frac{s_{1 \cdot ab}}{s_{2 \cdot ab}} \hat{\beta}_{21} z_1 + \frac{s_v}{s_{2 \cdot ab}} \hat{\beta}_{2v} z_v.$$

The denominators are different in (10'') and (11'') than in (10') and (11') since the unbiased estimator of the variance of y is different. In particular, s_1 in equation (10') becomes $s_{1 \cdot ab}$ in equation (10'') and s_2 in equation (11') becomes $s_{2 \cdot ab}$ in equation (11''). More generally, the variance of an endogenous variable in the population is unbiasedly estimated by the variance of an endogenous variable in the sample after control for all fixed predictor variables.¹² Thus,

¹² Specifically, if x_a, x_b, \dots, x_n are fixed exogenous predictors of Y and x_1, x_2, \dots, x_1 are random

Table 1. Comparison of Alternative Path Coefficient Estimators for Sample Data^a

Predictor Variables ^b	$\hat{\beta}$	b	b*	\tilde{b}	t ^d
EQ. 10 x_a : Educational Attainment	.0731	.4015	.4627	.4627	10.40
x_b : Occupational Prestige	.0055	.1717	.1978	.1978	4.46
R_u : Residual-Participation	.6106	.8678	.8678 ^e		
Multiple Correlation	.4970				
EQ. 11 x_a : Educational Attainment	.4548	.4013	.4682 ^c	.4682	10.52
x_b : Occupational Prestige	.0190	.0949	.1107 ^c	.1107	2.49
y_1 : Social Participation	8.0422	.1352	.1369	.1369	
R_y : Residual-Isolation	3.7169	.8490	.8490 ^e		
Multiple Correlation	.5284				

^aThe data were obtained from a quota sample of 507 adults aged 45 and over residing in Southwest Virginia. The variables employed are: x_a : number of years of school completed by the head of the household ($\bar{x} = 9.27$, $sd = 3.863$); x_b : Duncan scale score of the current occupation of the head of the household ($\bar{x} = 20.21$, $sd = 21.891$); y_1 : modified Chapin scale score ($\bar{x} = 0.477$, $sd = 0.704$); y_2 : modified Dean social isolation scale score ($\bar{x} = 33.26$, $sd = 4.378$).

^bThe first set of variables is associated with equation (10) and the second set with equation (11).

^cThe multiple correlation of isolation with education and occupation is .5152.

^d $t = b^*(n-2)^{1/2} = b^*(505)^{1/2} = 22.47b^*$.

^eIt can be easily shown that the estimators of the residual path coefficients are the same whether the predictor variables are fixed or random.

the appropriate (unbiased estimator of σ^2 in equation (11'') is $s_{2.ab}$, not s_2 or $s_{2.1ab}$.

Since most computer programs yield, minimally, the usual beta coefficients and the multiple correlation, several shortcut calculational methods are available. The simplest procedure for obtaining b^* is to divide the beta coefficient b by the value of the residual path, i.e., $(1 - R^2)^{1/2}$. This works since:

$$(12) \quad b^* = \hat{b} \frac{1}{\sqrt{1 - R^2}} = \hat{\beta} \frac{s_x}{s_y \sqrt{1 - R^2}} = \hat{\beta} \frac{s_x}{s_y \sqrt{1 - R^2}} = \hat{\beta} \frac{s_x}{s_{y \cdot x}} \cdot 18$$

predictors of Y , then

$$E(S_{y.ab}, \dots, n) = \sigma_y.$$

This case is necessary in path models that include intervening variables since the intervening variable must be assumed to be random.

¹⁸ In general, $s_{yx} = (1 - r^2)^{1/2} s_y$ where R is the

Thus, in our data (see Table 1),

$$b^*_{1a} = .4627 = .4015 / .8678 = .0731 \frac{(3.863)}{(.6106)}$$

and

$$b^*_{2a} = .1978 = .1717 / .8678 = .0055 \frac{(21.891)}{(.6106)}.$$

Equation (11''), on the other hand, poses more difficulties since it is necessary to obtain the multiple correlation when predicting Y_2 from X_a and X_b . This would almost always involve the computation of a separate regression equation involving only the fixed predictors. The resulting coefficients are:

$$b^*_{2a} = .4682 = .4013 / .8571 = .4548 \frac{(3.7169)}{(3.863)}$$

multiple correlation and s_y is the sample standard deviation of the endogenous variable (Hays, 1963).

and

$$b^*_{2b} = .1107 = .0949 / .8571 = .0190$$

$$\frac{(21.891)}{(3.7169)}$$

The third coefficient in equation (11''), b^*_{21} , represents yet a different case since an unbiased estimator of σ_1 is $s_{1 \cdot ab}$ rather than s_1 . In this case, the simplest calculating equation is:

$$(13) \quad b^*_{21} = b_{21} \frac{\sqrt{1 - R^2_{2 \cdot ab}}}{\sqrt{1 - R^2_{1 \cdot ab}}}$$

where $R_{1 \cdot ab}$ is the multiple correlation obtained by regressing y_1 on x_a and x_b . Equation (13) follows since

$$(14) \quad b^*_{21} = b_{21} \frac{\sqrt{1 - R^2_{1 \cdot ab}}}{\sqrt{1 - R^2_{2 \cdot ab}}} = \hat{\beta}_{21} \frac{s_1 \sqrt{1 - R^2_{1 \cdot ab}}}{s_2 \sqrt{1 - R^2_{2 \cdot ab}}}$$

$$= \hat{\beta}_{21} \frac{s_{1 \cdot ab}}{s_{2 \cdot ab}}$$

Numerically, this coefficient is:

$$b^*_{21} = .1369 = .1352 \frac{(.8678)}{(.8571)}$$

$$= 8.0422 \frac{(.6106)}{(3.7169)}$$

To briefly summarize, there are four distinct rules for calculating the proposed estimators of the standardized path coefficients.

(a) If all predictors of a given endogenous variable are treated as random, standard path analysis procedures may be used to obtain path coefficient estimates.

(b) If all predictors of a given endogenous variable are treated as fixed, divide the usual path coefficient estimates by the residual term of the endogenous variable $(1 - R^2)^{1/2}$ to obtain the path coefficient estimates.

(c) If some of the predictors of a given endogenous variable are treated as fixed and others as random, the path coefficient for fixed predictors is the usual beta coefficient divided by $(1 - R^2)^{1/2}$ where R is the multiple correlation when predicting the endogenous variable from the set of fixed exogenous variables.

(d) If some predictors of a given endogenous variable are treated as fixed and others as random, the path coefficient for random pre-

dictors is the usual path coefficient estimator multiplied by the ratio.

$$\frac{\sqrt{1 - R_1^2}}{\sqrt{1 - R_2^2}}$$

where R_1 is the multiple correlation when predicting the random predictor with prior fixed exogenous variables (if any) and R_2 is defined as in (c). [Note, the result reduces to (c) if there are no prior predictors.]¹⁴

We repeat that the coefficients obtained by using these rules have many desirable statistical properties as estimators of the standardized path coefficients displayed in (3). Furthermore, use of these rules leads to simple procedures for testing the hypothesis that a given path should be removed from the model. Finally, we note that these rules lead to estimators which are either identical to or larger than (in absolute value) the usual standardized estimators.

SUMMARY AND CONCLUSIONS

One of the critical concerns in using path analytic or, for that matter, regression procedures is that exogenous variables may not be correlated with residual variables. This problem has, at least to date, been dealt with by the expedient procedure of assumption, i.e., by assuming (wishing) the two types of variables to be uncorrelated. Of course, it is well known that if our hopes are incorrect, the resulting estimates are both biased and inconsistent. Another approach to this problem is to treat the exogenous variables as fixed or predetermined. Such a procedure is always valid, even if the data were initially sampled at random.¹⁵ The

¹⁴ The reader should note the alternative estimators introduced are always larger in absolute value than the usual estimator. This discrepancy, increases as the multiple correlation increases since the ratio of the proposed estimator to the usual estimator is $(1 - R^2)^{-1/2}$ which is greater than 1 (unless $R=0$). Since the proposed estimators are unbiased for large samples, the usual estimators tend to underestimate the true standardized path coefficients. Somewhat paradoxically, the usual estimates become more biased as the predictability of the endogenous variable increases. Thus the inadequacy of the usual estimators is most notable when dealing with highly related variables.

¹⁵ Treating random exogenous variables as fixed involves a change in the underlying model similar to the change between random and fixed effects analysis of variance. While a random variable may always be treated as if it were fixed, such treatment

primary advantage to this approach is that no assumptions need be made regarding the nature of the relationship between exogenous and residual terms; by definition the correlation is zero. The primary disadvantage of this approach is that the statistical generalizability of the results is limited to the values of the exogenous variables actually included in the study. Since, however, the primary mode of generalization in sociology appears to be logical rather than statistical, such a limitation seems minimal.

We do not mean to imply that treating random exogenous variables as fixed is a methodological panacea. Any method of sampling and model specification has its advantages and disadvantages. Fixing, *a priori*, exogenous values and then sampling is an excellent design for controlling for multicollinearity but forfeits inferences about predictor variables. Randomly sampling units and, as a consequence, viewing variables as random has the advantage of yielding information about the population's behavior with respect to the predictor variables. At the same time, this approach is extremely sensitive to multicollinearity and forces concern about correlated exogenous and residual terms. The method suggested in this paper, treating exogenous variables as fixed however they are obtained, neither controls multicollinearity nor permits inferences about predictor variables. It does, however, eliminate the problem of correlated predictor and residual terms.

Careful examination of the structural equations incorporating fixed exogenous variables reveals that, if data are standardized, the usual estimators of path coefficients are biased and inconsistent. Within this context, an alternative estimator of the standardized path coefficient that is asymptotically unbiased ($b^*_{yx} = \frac{s_x}{s_{y \cdot x}} \hat{\beta}_{yx}$) is developed.

In addition to being asymptotically unbiased, the alternative estimator facilitates hypothesis testing and is larger (in absolute value) than the coefficients typically used.

eliminates inferences with respect to that dimension. Of course, the ultimate in such treatment is considering all variables to be fixed, in which case no statistical inferences are possible.

REFERENCES

- Blalock, H. M.
1971 "Causal inferences, closed populations, and measures of association." Pp. 139-51 in H. M. Blalock (ed.), *Causal Models in the Social Sciences* Chicago: Aldine.
1972 *Social Statistics*. New York: McGraw-Hill.
- Blalock, H. M., C. S. Wells and L. F. Carter
1970 "Statistical estimation with random measurement error." Pp. 75-103 in E. F. Borgotta and G. W. Bornstedt (eds.), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Duncan, O. D.
1966 "Path analysis: sociological examples." *American Journal of Sociology* 72(July): 1-16.
- Goldberger, A. S.
1964 *Econometric Theory*. New York: Wiley.
- Hays, W. L.
1963 *Statistics for Psychologists*. New York: Holt.
- Heise, D. R.
1968 "Problems in path analysis and causal inference." Pp. 38-73 in E. F. Borgotta and G. W. Bornstedt (eds.), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Johnston, J.
1963 *Econometric Methods*. New York: McGraw-Hill.
- Land, K. C.
1968 "Principles of path analysis." Pp. 3-37 in E. F. Borgotta and G. W. Bornstedt (eds.), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Mayer, L. S. and M. S. Younger
1972 "Procedures for estimating standard regression coefficients from sample data." Unpublished manuscript.
- Tukey, J. W.
1954 "Causation, regression, and path analysis." Pp. 35-66 in O. Kempthorne et al. (eds.), *Statistics and Mathematics in Biology*. Ames, Iowa: Iowa State College.
- Wonnacott, R. J. and T. H. Wonnacott
1970 *Econometrics*. New York: Wiley.
- Wright, S.
1921 "Correlation and causation." *Journal of Agricultural Research* 20(January):557-85.
1934 "The method of path coefficients." *Annals of Mathematical Statistics* 5(September): 161-215.
1960 "Path coefficients and path regressions: alternative or complementary concepts?" *Biometrika* 16(June):189-202.
- Younger, M. S.
1972 *Invariant Estimation with Application to Linear Models*. Unpublished Doctoral Dissertation at Virginia Polytechnic Institute and State University.

COMMENTS

COMMENTS ON MODELS FOR THE SOCIOECONOMIC CAREER*

Professor Kelley's paper (Kelley, 1973) demonstrates the utility of specifying causal models which incorporate hypotheses regarding measurement errors. In Kelley's case, he has assumed errors in respondent's own education, occupation, and income statuses to be unequal and uncorrelated at any one time of measurement, errors in the same status at different points in time to be equal and uncorrelated (even in the case where reports are made retrospectively), and errors in proxy reports (e.g. son's report of father's occupation) to be larger than errors in the respondent's reports (say, of his occupation) and also to be uncorrelated with errors in the respondent's reports. While these specifications are not explicit in his causal diagrams, they are represented formally in his use of different reliability coefficients (which are not estimated from his model but are taken from external sources) and his technique of correcting for attenuation. The latter method is the standard formulation:

$$r_{11} = \frac{r_{11}}{\sqrt{r_{11}r_{11}}},$$

which assumes that errors in measuring X_i are random with regard to those in X_j . If one accepts this logic, and Kelley himself is not of a single mind about uncorrelated errors in these data, then there is some support for Kelley's conclusions, although problematic estimates remain in the Princeton Fertility Study (PFS) data for the modified causal chain.

Kelley seems not to recognize that theories, framed so that their credibilities can be assessed in some set of data, embody substantive hypotheses about relationships between variables of interest, which include assumptions (usually not explicit in the formal specification, as in Kelley's case) about errors of measurement. In judging

the theory's credibility, the analyst must consider the plausibility of error estimates as well as of estimates of substantive relationships in the model; for the former clearly affect the latter. Given that Kelley's modified causal chain model implies a number of complex assumptions about error relationships, it is not altogether clear that his theory is more parsimonious than the so-called "historical" model, even though the latter also contains assumptions about error (e.g. those explicit in ordinary least-squares estimation). Since assumptions about error are part of Kelley's theory, I shall discuss them in the context of a general critique of his model. I find quite plausible the notion that errors in current statuses (e.g. occupation, income) are random with respect to each other, especially in data collected by the Bureau of the Census. The rigor of data collection associated with the Census [and this is the source of the Hodge and Siegel (1968) estimates which Kelley uses] is not necessarily duplicated in all data files. Kelley is inclined to doubt the independence of occupation and income reports, especially when made retrospectively. His footnote 9 alleges that income reported for the time of marriage is biased by income and occupation statuses at the time of the first interview. It is unclear how he arrives at this conclusion except by assumption and in what sense this bias "offsets" simple attenuation in correlations. In any event, either by assumption or fact, such bias implies that the model in his Figure 2 is mis-specified and that the estimates for the modified causal chain are inaccurate. Indeed there is evidence (presented subsequently) that a model applied to the PFS data which embodies Kelley's assumptions about error may be mis-specified.

Contrary to Kelley's assertion that "unbiased estimates can be obtained simply by correcting each correlation for attenuation and then proceeding as usual," specifying errors in causal models requires careful thought and explicit documentation of the means by which estimates of error are obtained and the manner in which errors affect other estimates. Such documentation is lacking in Kelley's paper, especially with regard to error in the proxy report of father's

*I thank the Editor for his invitation to comment on Professor Kelley's paper. I appreciate Kelley's pointing out the arithmetic and copying errors in my earlier papers.

occupation.¹ This demand I am imposing on Kelley's work may seem arbitrary and pedantic, but the issues of error have meaning for substantive conclusions, as Kelley recognizes. Bowles (1972) has challenged Blau and Duncan's conclusions (1967) about the relative importance of educational achievements versus family socioeconomic characteristics in explaining inequalities in socioeconomic attainments. In part, Bowles' conclusions derive from his assumptions about errors and their correlations, which diverge from those in Blau and Duncan. Whether one finds greater credibility in one analysis or the other depends largely on the quality of information about errors. Since one could be led to quite different policy inferences in each case, the issues which I am raising appear more than academic.

I take issue with Kelley's theory on more substantive grounds as well. Kelley asserts that the "historical" model is atheoretical because it fails to find a substantive basis for the presence of time-two lagged (historical) affects. Yet Kelley's "strong theory" lacks a basis (other than empirical) for the expectation that father's occupation will continue to affect the occupation, and not the income, of the son beyond the early stages of the latter's work career. If paternal occupation were a proxy for family income, then one might expect father's occupation to affect son's later earnings rather than son's occupation (Sewell and Hauser, 1972).

Furthermore, why should Kelley expect time-one lagged effects of income (earnings) on occupation (in the metric of prestige) to be zero at all points in the life cycle? Occupation-specific earnings can be taken as a measure of socioeconomic variation over jobs within detailed occupation titles which share the same level of prestige. Several occupation titles, involving

somewhat different types of work, can have the same prestige score. Moreover, within any one occupation there are a number of jobs and each can have a different average earnings. Is it not logical that an effect of income at time 1 on occupation at time 2 taps vertical movements of men between jobs where such movement involves a change of occupation prestige level as well? This movement can represent a net shift, holding constant occupation (prestige score) at time 1, since the effect of occupation at time 1 on occupation at time 2 estimates the extent of inter-temporal stability of prestige levels. Kelley states that "income in part measures need, ability, motivation and other individual characteristics which are relatively persistent over time." Is it not possible that a man whose earnings are lower than the average for his occupation (prestige) level might become motivated (the situation instigating a persistent tendency to achieve) to change jobs and in so doing raise his prestige level? If this is possible, and I think it is in the American stratification system where the correlations among status dimensions (education, occupation prestige and income) are rather low when calculated over individuals, then such inter-status mobility would be reflected in a negative net coefficient for the effect of income on occupation. This relationship is implied for the PFS data in Kelley's footnote 10, which should not surprise the careful reader of my analyses of these data.²

In principle this logic could be extended to justify an historical (time-two lagged) effect, on the argument that early job takers often have had to interrupt their schooling; they start not only at a lower prestige level but at characteristically lower paying jobs within an occupation. Some minority of such men resume their educations, and they experience considerable subsequent upward mobility (occupational prestige) (Duncan, Featherman, and Duncan, 1972:210-24). In the PFS data, statuses at marriage could not be ordered unambiguously with regard to the completion of schooling. It would not be surprising to find (and the data which follow bear on this point) a (negative) effect of income or occupation at marriage on some time-two lagged occupation in the PFS data, reflecting the ascension of the early-to-marry, whose mobility was postponed by delays in completing schooling or by delayed accumulation of economic resources necessary for mobility (cf. Coombs et al., 1970).

¹ Data on errors of measurement of socioeconomic statuses are not extensive. The most systematic examination of errors in reports to Census and CPS surveys is Hodge and Siegel's (1968). Bowles (1972) and Jencks (1972:Appendix B) treat the topic of proxy reports of parental statuses by their adult offspring, and Kerckhoff et al. (1973); and Kayser and Summers (1973) report the accuracy of children's estimates of parental statuses. Estimates of errors differ in these sources, reflecting alternative assumptions in the absence of sufficient data; they also reflect different populations of reference. As part of the design of the 1973 replication of the Blau and Duncan (1967) OCG mobility survey (Featherman and Hauser, 1973), special studies are planned to collect data on reliability and validity of reports (for self and by proxy for parent) which will supply some of the missing information.

² Occupation had been grouped originally by five-unit intervals of a prestige scale and then recoded by me according to the prestige average for each interval. This procedure leaves great latitude for job and occupation mobility within the prestige categories.

To summarize this critique of the theory, I argue that Kelley has not specified correctly the model of the process as he himself thinks it works; errors in measurement are mis-specified, and insufficient justification is offered for the appropriateness of the estimates of error he has used. With so many over-identifying restrictions in the dual-chain model, it would be prudent for Kelley to estimate errors and their correlation from the model itself before deciding to accept the credibility of his (mis-specified) formulation.³ Second, elements of Kelley's theory are not grounded in substance. Third, post-factum explanations for the presence of certain time-one and time-two lagged effects can be offered for the PFS data; Kelley argues that these are zero and have no basis in substance. Taken together these observations imply that Kelley's work is suggestive but no more definitive than my early research note (Featherman, 1971).

Let me manipulate some data to support my reservations about Kelley's model. Table 1 has been computed for PFS data corrected for attenuation, using the same procedures as Kelley. The regression equations can be understood as representing a fully-recursive model, including status information from the second PFS panel which Kelley chose to ignore in his reanalysis. Table 1 permits a test of Kelley's theory, if one imposes the following expectations for non-significant (absolute value of coefficient less than twice its standard error) or zero-value effects:

³ Figure 2 in Kelley's paper could be re-specified so that estimates of reliability derive from the structural equations. These equations also would represent formally one's assumptions about correlations among errors in variables, and they could (assuming the model is not then under-identified) allow estimates of error correlations to be problematic. The merit of this approach is that it does not force the analyst to accept the implicit assumptions which inhere in estimates of measurement error taken from other samples, other populations, and other classifications of relevant variables and applied to one's data. In Kelley's case, he accepts the Hodge and Siegel (1968) estimates of reliability as the estimates appropriate to the PFS population. Moreover, he accepts as equivalent the Hodge and Siegel estimate of error in reporting occupation at the Census major occupation group level, with data coded in Duncan's socioeconomic indexes (1961) for major occupation groups and the *unknown* estimate of error in reporting occupation in the PFS data, which were aggregated on the basis of intervals of occupational prestige. Since neither the classification of occupation nor the metric of scaling is the same in the PFS data as in Hodge and Siegel's case, the application of the Hodge and Siegel estimates is questionable and not necessary.

1) all time-two lagged (historical) effects, except for paternal occupation and respondent's education, 2) all time-one lagged effects of income on occupation. These expectations constitute the null hypotheses.⁴ Although variables are somewhat differently labelled, the partial regression coefficients in the first four columns of Table 1 are the same as columns 1, 3, 5, and 7 of Kelley's Table 3. In both tables two coefficients which are expected to be zero (non-significant) are not. There is a net negative effect of income at marriage on occupation at the first interview, or panel I (-.097). My earlier comments about early job takers or the early-to-marry seem pertinent. I have no satisfying post-factum explanations for the positive net effect of occupation at marriage on income at panel I (.134), which Kelley ignores, despite its statistical significance. In the absence of an explanation for these violations of the theory, the results should warn the analyst that the fully-recursive model, and attendant assumptions about errors, may be mis-specified. Since data at panel I and reports about statuses at marriage were collected within the same interview, errors in variables may not be uncorrelated; apparently Kelley suspects this to be the case. But if such correlations obtain, then the estimates in Table 1 and Kelley's Table 3 cannot support his position or any other.

In the equation for occupation at panel II (Table 1, col. 5), the coefficient for the effect of occupation at marriage (a time-two lagged effect) is negative and non-zero (-.087). I could argue that this is the delayed rise in occupation prestige for men who were married early, interrupted schooling (perhaps), entered a lower status occupation, and later accumulated sufficient resources (educational or other) to rise. Finally, in the equation for income at panel III, there are two non-zero coefficients which Kelley's theory would have as non-significant. I hesitate to speculate about these values, partly because of the non-random allocation of respondents to shorter or longer intervals between panels II and III (cf. Featherman, 1969). As mentioned previously, these violations of Kelley's theory suggest alternative specifications, possibly in the treatment of errors in variables.

I conclude from these results that the full

⁴ The reader should recognize that the "historical" model (or Kelley's model) can be subjected to determinate statistical tests by this means, using the significance criterion cited in the text or a t-test for each coefficient in a regression equation. Kelley's method of "testing" his model by using its over-identifying restrictions to calculate differences between observed and expected (calculated from the model) correlations is descriptive and not determinate.

Table 1.--Partial Regression Coefficients in Standard Form, PFS Data Corrected for Attenuation

Independent Variables		Dependent Variables (see stub)							
		W	IM	Y1	I1	Y2	I2	Y3	I3
Father's Occupation	X	.113	.036*	.081	-.012*	-.019*	-.017*	.107	-.003*
Education	U	.618	-.029*	.236	.108	.191	.113	.056*	.142
Occ. Marriage	W	--	.264	.685	.134	-.087	.074*	-.002*	-.085*
Inc. Marriage	IM	--	--	-.097	.331	-.020*	-.012*	.015*	.054*
Occ Panel I	Y1	--	--	--	.168	.842	-.035*	.095*	.132*
Inc Panel I	I1	--	--	--	--	.018*	.604	-.047*	.057
Occ Panel II	Y2	--	--	--	--	--	.185	.688	-.182
Inc Panel II	I2	--	--	--	--	--	--	.037*	.359
Occ Panel III	Y3	--	--	--	--	--	--	--	.423
	R ²	.453	.068	.769	.294	.829	.614	.750	.518

*Absolute size of coefficient less than twice its standard error.
(Source: Featherman, 1971)

PFS data file presents less support for Kelley's model than he would have the reader believe. If one interprets substantively the non-zero coefficients which are problematic for Kelley's model, then one sees the possibility that there are small effects early in the work career, some of which are historical, which would argue that the timing and place (status) of entry into the labor force have more than immediate consequence for later socioeconomic achievements. This interpretation is viable only if the reader subscribes to the implicit assumptions in these estimates about the size and workings of errors of measurement and about the appropriateness of the Hodge and Siegel (1968) estimates of measurement error to the PFS case.

Since there are peculiarities in the PFS data, especially in the panel III information, I am not inclined to make much of the estimates beyond panel II, at least for the purposes at hand. To explore Kelley's thesis further, I have obtained data from the Six-Cities Study of Labor Mobility and the 1970 Detroit Area Study.⁵ My purpose in analyzing other data is obvious: If a model is valid, it can be imposed on different samples.

⁵I thank Professor Robert Cole, principal investigator for the 1970 Detroit Area Study, Mr. Paul Voss who participated in that study and has analyzed some of its data, and Professor Seymour Spilerman, from whom I obtained a portion of the Six-Cities data file, for their kind assistance in my hasty search for additional information. They in no way are responsible for any misuse or misinterpretation of these data on my part.

What follows pertains only to the modified causal chain model for occupations, since income data were not readily available for Kelley's full model.

Tables 2 and 3 contain standardized partial regression coefficients for different populations, as described in the table titles.⁶ The Detroit sample is much smaller (N=129) than the Six-Cities sample (N=2054). However, the data on occupation apply to men at roughly equivalent stages in their life cycles. In Detroit the men were aged 26-35 in 1946, 34-43 in 1954, 42-51 in 1962, and 50-59 in 1970. In Six Cities, men were aged 24-34 in 1940, 29-38 in 1945, 33-42 in 1949, and 35-44 in 1951. In some broad sense, these are men from the same birth cohort whose occupation statuses were ascertained at roughly overlapping periods in the life cycle (although the period was more extended in Detroit by using the technique of retrospective life-history interviews for men aged 50-59 at the time of the cross-section study). Although the variables' labels are not equivalent, data for variables X_1 . . . X_8 in Tables 2 and 3 cover a similar interval of the life cycle for both samples. However, the *Beta* coefficients in Tables 2 and 3 are not directly comparable, owing to different estimates of variances in the two popu-

⁶The product-moment correlations and other summary statistics on which the regressions in Tables 2 and 3 are based were not included for reasons of space limitations. They are available on request.

Table 2.--Partial Regression Coefficients in Standard Form
for Non-Negro Men Aged 35-44 in the Experienced
Civilian Labor Force in 1951, Six-Cities Labor
Mobility Study

Independent Variables	Dependent Variables (see Stub)						
		X_3	X_3^*	X_4	X_4^*	X_5	X_5^*
Father's Occupation [†]	X_1	.127	.152	.031 ^a	.011 ^a	.025 ^a	.019 ^a
Education	X_2	.489	.528	.179	.114	.138	.095
Occupation 1940	X_3	--	--	.655	.811	.197	-.005 ^a
Occupation 1945	X_4	--	--	--	--	.555	.854
Occupation 1949	X_5	--	--	--	--	--	--
	R^2	.303	.378	.605	.790	.660	.844

[†]All occupation variables scaled in metric of socioeconomic status (Duncan, 1961).

^aAbsolute size of coefficient less than twice its standard error.

*Equation based on data corrected for attenuation.

Table 3.--Partial Regression Coefficients in Standard Form
for Nonfarm Men Aged 50-59 in 1970, Detroit Area

Independent Variables		Dependent Variables (see stub)							
		X_3	X_3^*	X_4	X_4^*	X_5	X_5^*	X_6	X_7
Father's Occupation [†]	X_1	-.029 ^a	-.057 ^a	.037 ^a	.043 ^a	-.075 ^a	-.128 ^a	-.051 ^a	-.037 ^a
Education	X_2	.299	.349	.205	.206	.266	.301	.142	.077 ^a
First Job	X_3	--	--	.352	.407	-.124 ^a	-.216	-.075 ^a	-.036 ^a
Occupation 1946	X_4	--	--	--	--	.672	.828	.084 ^a	.096 ^a
Occupation 1954	X_5	--	--	--	--	--	--	.767	.125 ^a
Occupation 1962	X_6	--	--	--	--	--	--	--	.647
Occupation 1970	X_7	--	--	--	--	--	--	--	--
	R^2	.083	.105	.217	.277	.539	.730	.756	.719

[†]All occupation variables scaled in metric of prestige (Siegel, 1970).

^aAbsolute size of coefficient less than twice its standard error.

*Equation based on data corrected for attenuation.

(Source: Uncorrected correlations drawn from Voss, 1971.)

lations;⁷ data were corrected for attenuation according to Kelley's assumptions.

In Six-Cities (Table 2) there are no significant time-two lagged effects in the equation for X^*_s , which is consistent with Kelley's assertions about the consequence of correcting for attenuated variance* (compare the equation for X_s with that for X^*_s). For men aged 33-42 in these data, there are no historical effects. Notice, however, that the expectation by the modified causal chain of lagged effects of father's occupation are not observed; there are no direct effects of paternal or filial occupation statuses beyond the age interval 24-34, or early career.

Data for Detroit (Table 3) demonstrate no direct effect of father's occupation on any of son's occupation statuses, not even first job. There is a negative and non-zero "historical" coefficient for the effect of first job on X^*_s , or occupation status at age 34-43. For men aged 42-51 and 50-59, equations for X_s and X^*_s , respectively, there appear to be no lagged effects of earlier occupations, even before correcting for attenuation. Indeed, at this point in the life cycle it would appear that inter-temporal status shifts are so rare that the process may be strictly Markovian or a simple causal chain (note the absence of any non-zero coefficient except for the effect of time-one lagged occupation, .647).

I would conclude from these data that occupation careers, as these are summarized by scores of prestige or socioeconomic status, cannot be estimated from simple causal chains except perhaps for the period late in the life cycle, say after age 50. Modified causal chains seem to offer reasonably good approximations to the process in the middle of the work career, say ages 35 to 50. Education appears to exert some lagged influence in the middle of the career, but the evidence for a corresponding effect of paternal occupation is lacking. In the early stages of occupation careers, up to age 35, there may be small lagged effects of status and timing of entry into the full-time labor force which modified causal chains do not represent. Where data are available for earnings as well as for occupation, there are suggestions of lagged relationships between income and occupation which Kelley's dual modified chain would mis-specify. In short, the socioeconomic life cycle may not be homogeneous in the sense that one model can be applied to it irrespective of the age of the cohort

or stage in the career. Corrections for errors in measurement are always desirable, provided one offers a logically consistent rationale for explicit assumptions about errors in terms of the model which is adduced, the data source, and the estimates of the model's structure. I think that Kelley's paper is an important statement in the early stages of specifying models of the socioeconomic career which take errors as problematic. Future treatments of this topic should focus their attention on the data from life-history studies, such as the 1970 Detroit Area Study, which were conducted in the last few years.

Finally, interpretations by Kelley, myself and others of the data for occupation careers has been incorrect, I think. Often, we have spoken of occupation prestige or socioeconomic status as indexing tenure rights, experience, skills and knowledge, or contacts, for example. The extent to which occupation, industry and class of worker information from surveys tap these items in any direct way is small. Hence, discussions of time-one or time-two lagged occupation effects ought not to stress issues of tenure or experience, when occupation at time one and two are coded in scales of prestige or socioeconomic status. Since several occupations can share the same score, inter-temporal stability is not inconsistent with occupational changes, or alterations in experiences and tenure rights. What these data do measure is the stability of status levels, whether engendered by tenure and experience considerations or by (possibly) concerns for status consistency or senses of relative deprivation or whatever. My point is to give greater credence to "historical" effects than are apparent in discussions of models of *status* stability and change, phrased as if these models were of *job* stability and change. The latter models are quite different from the former in that issues of job tenure rights, job experience, skill and the like are very pertinent. Such models can be argued successfully along the lines of Kelley's section, "Models for the Occupational Career"; perhaps arguing for historical effects makes little sense when discussing job stability. Again, data from life-history studies (e.g. Blum et al., 1969) are useful in distinguishing job changes from occupation status shifts (cf. Voss, 1971). As we move toward a better understanding of how socioeconomic careers develop, we will want to distinguish models of job mobility from those of status mobility; but ultimately the two kinds of models ought to be synthesized (e.g. Sørensen, 1972).

DAVID L. FEATHERMAN

University of Wisconsin

* Standard deviations of variables X_1 . . . X_s , respectively, are as follows: (Six-Cities) 20.87, 1.54, 21.99, 21.76, 22.45; (Detroit) 11.49, 1.20, 10.47, 11.02, 10.59.

REFERENCES

- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: Wiley.
- Blum, Zahava D., N. Karweit and A. Sørensen
1969 *A Method for the Collection and Analysis of Retrospective Life Histories*. Center for the Study of Social Organization of Schools Report No. 0048. Baltimore, Maryland: The Johns Hopkins University.
- Bowles, Samuel
1972 "Schooling and inequality from generation to generation." *Journal of Political Economy* 80(May/June):S219-S251.
- Coombs, L. C., R. Freedman, J. Friedman and W. F. Pratt
1970 "Premarital pregnancy and status before and after marriage." *American Journal of Sociology* 75(March):800-20.
- Duncan, Otis Dudley
1961 "A socioeconomic index for all occupations." Pp. 109-38 in A. J. Reiss and others, *Occupations and Social Status*. New York: Free Press.
- Duncan, Otis Dudley, D. L. Featherman and Beverly Duncan
1972 *Socioeconomic Background and Achievement*. New York: Seminar.
- Featherman, David L.
1969 *The Socioeconomic Achievement of White Married Males in the United States: 1957-1967*. Unpublished doctoral dissertation, University of Michigan.
- Featherman, David L.
1971 "A research note: a social structural model for the socioeconomic career." *American Journal of Sociology* 77(September):293-304.
- Featherman, David L. and Robert M. Hauser
Forthcoming "Design for a replicate study of social mobility in the United States." In K. Land and S. Spilerman (eds.), *Social Indicator Models*. New York: Russell Sage.
- Hodge, Robert and Paul M. Siegel
1968 "A causal approach to the study of measurement error." Pp. 28-59 in H. M. Blalock, Jr. and A. B. Blalock (eds.) *Methodology in Social Research*. New York: McGraw-Hill.
- Jencks, Christopher
1972 *Inequality*. New York: Basic.
- Kayser, Brian and Gene F. Summers
1973 "The adequacy of student reports of parental SES characteristics." *Sociological Methods and Research* 1(February):303-15.
- Kelley, Jonathan
1973 "Causal chain models for the socio-economic career." *American Sociological Review* 38(August):-481-93.
- Kerckhoff, Alan C., William M. Mason and Sharon S. Poss
1973 "On the accuracy of children's reports of family social status." *Sociology of Education* 46(Spring):219-47.
- Sewell, William H. and Robert M. Hauser
1972 "Causes and consequences of higher education: models of the status attainment process." *American Journal of Agricultural Economics* 23(December):851-61.
- Siegel, Paul M.
1970 *The American occupational prestige structure*. Unpublished doctoral dissertation, University of Chicago.
- Sørensen, Aage B.
1972 "A model for occupational careers." Paper presented at the annual meetings of the American Sociological Association, New Orleans, La.
- Voss, Paul R.
1971 "Occupational mobility of a cohort of Detroit workers." Unpublished manuscript, Department of Sociology, University of Michigan.

HISTORY, CAUSAL CHAINS AND CAREERS: A REPLY*

Featherman's valuable comment raises three main points. First, he presents new data that strongly support the causal chain models. Second, he raises questions about random measurement error, particularly the reliability of father's occupation. But the quantitative results are, I will show, wholly unaffected by reasonable assumptions about that. Finally, he raises problems involving correlated measurement error. But the same assumptions are implicit in both our models and reasonable assumptions about their value do not affect the conclusions. In short, the causal chain models remain clearly preferable to Featherman's historical models.

New Data

Featherman presents new data which strongly support Blau and Duncan's (1967:177-88) causal chain model of the occupational career. These important data are from the large (N=2054) and careful Six-Cities Study of Labor Mobility (Palmer, 1954). The crucial question is whether occupation in the first time period has an independent effect on occupation in the third period; Featherman (1971:299) originally rejected the causal chain model on the ground that this historical effect exists. But when corrected for attenuation, the Six-Cities data offer absolutely no evidence for an historical effect. The path is a miniscule -.005 and the increment in variance explained, .000, can be ignored with

* I thank John L. Hammond and Donald J. Treiman for their comments and David L. Featherman for his courtesy throughout this exchange.

some safety. The more parsimonious causal chain model fits the data astonishingly well.¹ That the causal chain model fits both this and the PFS data is persuasive evidence for it.²

Measurement Error

There is some confusion about measurement error in Featherman's account. 1) Estimating path models conventionally without correcting for attenuation is plainly and simply wrong; the resulting estimates are known to be biased (e.g. Johnston, 1963:148-50; Wonnacott and Wonnacott, 1970:164-8) and the biases can be expected to be large (Bohrstedt and Carter, 1971:130-40; Bowles, 1972). Moreover, since indirect effects are especially vulnerable to random measurement error (Blalock, 1961:146-50; Kelley, 1973), bad statistics can easily lead to bad theory by wrongly suggesting the existence of direct (e.g. historical) effects when a more parsimonious theory would suffice. 2) The conventional procedure is mathematically identical to the procedure I have used except for the (unrealistic) specification that all variables are measured without error. In particular, the two procedures make exactly the same assumptions about correlated error (Johnson, 1963: Ch. 6; Wonnacott and Wonnacott, 1970: Ch. 7). The only difference is that the conventional procedure implausibly assumes there is no measurement error. 3) In a later paper, Featherman (1972) himself corrects the PFS data for attenuation using exactly the same procedures that I have used. He finds none of the assumptions sufficiently unusual or problematic to discuss. In that he is, I think, correct. In short, correcting for attenuation is the procedure of choice;

the conventional procedures have no advantages and clear disadvantages. Ignoring measurement error doesn't make it go away.

Random Measurement Error. Except for father's occupation, getting good reliability estimates for the PFS data is no problem. 1) Siegel and Hodge (1968) provide good estimates for reports of own occupation, education, and income. The PFS data are carefully collected so the estimates are appropriate; Featherman (1972) himself uses them without qualification. 2) Father's occupation, however, is a problem since there is as yet no persuasive direct evidence on its reliability in a general population. Featherman (1972) uses the reliability for son's current occupation, but this estimate is surely too high; in addition to the ambiguities in describing and coding any occupation, a son's report of his father's occupation refers to activities of another person which may have taken place years before. The estimate I originally used (Treiman and Hauser, 1970) is much lower and, I think, more plausible. It assumes that the true correlation between occupation and education has not changed over time and that measurement error accounts for the difference between the correlation observed for son's characteristics and the lower correlation observed for reports of father's characteristics. But the estimate can certainly be questioned.

Fortunately, this uncertainty about father's occupation is of no practical importance since quite different assumptions give results virtually identical to those I originally reported. I experimented with four different reliabilities. The highest was the reliability of son's own occupation, .861; this figure is surely an upper bound. The second was halfway between this and the third, the Treiman-Hauser estimate. The lowest, .646, was an equal step down and is surely a reasonable lower bound. The true figure doubtless lies somewhere within this wide range. Only the results using the two extreme reliabilities are shown, since the other two consistently give intermediate results. The true figure does not matter since all assumptions lead to virtually identical results. Table A-1 gives the evidence.³ No matter what one thinks

¹ The Six-Cities data differ from the PFS results in that father's occupation has no significant direct effect on son's occupation in the second and third time periods. This argues against both the causal chain and the historical models since both posit a direct effect. But it seems likely that the Six-Cities data, based on an urban sample, are atypical on this point since Blau and Duncan's (1967:178) excellent national sample indicates a higher correlation between father's and son's occupation for the nation as a whole. For men approximately 35-44 we have, correcting for attenuation,

Blau and Duncan	$r = .509$	$p = .178$
PFS	$r = .434$	$p = .190$
Six-Cities	$r = .397$	$p = .133$

where the path, p , is computed for a reduced model with only father's occupation and son's education as independent variables.

² Featherman's analysis of the 1970 Detroit Area Study also supports the causal chain model. Five of the six possible historical effects are small and insignificant. The results are, however, based on too small a sample ($N = 129$) to be taken seriously.

³ Here and in my original paper, I have omitted Featherman's Panel II. It is only three years after Panel I, unlike the approximately eight years separating the other panels. Omitting it doesn't matter (compare Table 1 in Featherman's comment with Table 3 in my original article) and makes the analysis easier to interpret since the intervals between periods are then roughly equal. There are also theoretical reasons why the causal chain model might not cover very short intervals—specialized skills, contacts, knowledge, tenure rights and the like might persist for a few years.

about the reliability of father's occupation, the causal chain model is clearly superior to the historical model.

Correlated Measurement Error. There may be correlated measurement error in the retrospective reports of income at time 1. But that problem is probably not serious with these data and for the issues at hand, although it is difficult to say anything definitive without direct data on the errors. As I indicated in the original article, some respondents' reports of their income at time 1 might be influenced by their occupation at the time or by their income at time 2. Featherman makes much of this. While it does suggest that estimates involving income at time 1 should be treated more cautiously than other estimates, I doubt that the bias is large enough to make any real difference. In his

own analysis, Featherman (1971, 1972) found the possibility not worth mentioning. Without persuasive data on the existence and magnitude of correlated errors, it seems wiser to stay with conventional and familiar assumptions; an element of judgment is involved, but the risk in the present case seems small. In any event, the problem is not peculiar to the causal chain model but applies equally to the historical model and to most other models. While there is some risk in ignoring correlated errors, it seems well worth taking in order to make some use of the data.

Furthermore, there is evidence that the bias introduced in this way is probably neither large nor of substantive importance. Table A-2 gives some typical results using different assumptions about correlated error. 1) The results are given

Table A-1. Effect of Different Assumptions about the Reliability of Father's Occupation: Standardized Partial Regression Coefficients for the Dual Causal Chain Model (I) and the Historical Model (II). Corrected for Attenuation; Decimals Omitted.

Independent Vars and Reliability of Father's Occ:	Dependent Variables:											
	Occ1		I1		Occ2		I2		Occ3		I3	
	I, II	I	II	I	II	I	II	I	II	I	II	
Fs Occ: Father's Occ												
Reliability = .861	10	0	03*	07	07	0	-01*	08	08	0	01*	
Reliability = .646	12	0	04*	08	09	0	-01*	10	10	0	01*	
Ed: Own Education												
Reliability = .861	63	-02*	-03*	24	24	12	11	19	20	16	16	
Reliability = .646	61	-02*	-03*	24	23	12	11	18	19	16	16	
Occ1: Occ at time 1												
Reliability = .861		27	26	66	69	0	13	0	-06*	0	-05*	
Reliability = .646		27	26	66	68	0	13	0	-06*	0	-05*	
I1: Income at time 1												
Reliability = .861				0	-10	35	33	0	-00*	0	06*	
Reliability = .646				0	-10	35	33	0	-01*	0	06*	
Occ2: Occ at time 2												
Reliability = .861						27	17	64	69	0	04*	
Reliability = .646						27	17	63	68	0	04*	
I2: Income at time 2												
Reliability = .861								0	-01*	29	27	
Reliability = .646								0	-01*	29	27	
Occ3: Occ at time 3												
Reliability = .861										40	40	
Reliability = .646										40	40	
Difference in R²												
Reliability = .861			.001		.009 ^a	.005		.001		.003		
Reliability = .646			.001		.009 ^a	.005		.001		.003		

* Coefficient less than twice its standard error.

^a Significant (F test) at $p < .01$.

Table A-2. Effect of Correlated Error: Standardized Partial Regression Coefficients for the Dual Causal Chain Model (I) and the Historical Model (II) Assuming that Errors in Reporting I1 are Correlated with Errors in Reporting Occ1 and I2. Details in Text. Corrected for Attenuation; Decimals Omitted.

Independent Vars and Correlated Measurement Error:	Dependent Variables:					
	Occ1	I1		Occ2	I2	
	I, II	I	II	I II	I II	I II
Fs Occ: Father's Occ						
Corr. error = .1	11	0	04*	08 08	0 -01*	09 09
Corr. error = .3	11	0	05*	08 08	0 -01*	09 09
Ed: Own Education						
Corr. error = .1	62	00*	-01*	24 24	12 11	18 19
Corr. error = .3	62	04*	03*	24 24	13 11	18 19
Occ1: Occ at time 1						
Corr. error = .1		24	23	66 68	0 15	0 -06*
Corr. error = .3		18	17	66 67	0 15	0 -06*
I1: Income at time 1						
Corr. error = .1				0 -09	33 31	0 -01*
Corr. error = .3				0 -06	30 28	0 -01*
Occ2: Occ at time 2						
Corr. error = .1				27 15	63 68	0 04*
Corr. error = .3				27 15	63 68	0 02*
I2: Income at time 2						
Corr. error = .1						0 -01*
Corr. error = .3						0 -01*
Occ3: Occ at time 3						
Corr. error = .1						40 40
Corr. error = .3						40 40
Difference in R²						
Corr. error = .1		.001		.007 ^a	.006	.001
Corr. error = .3		.002		.004 ^a	.006	.002

* Coefficient less than twice its standard error.

^a Significant (F test) at $p < .01$.

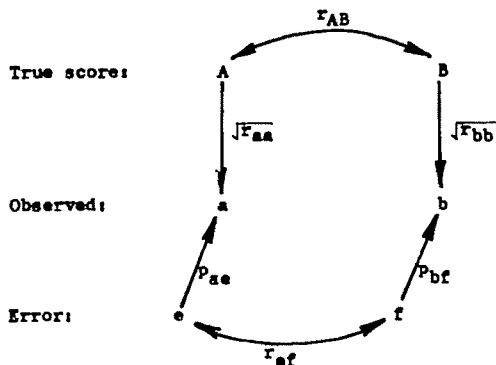
for correlated errors of .100 and .300 (.200 gives intermediate results). In comparison, the error that originally led Featherman to reject the causal chain model was .071 (Kelley, 1973: fn. 7). For simplicity's sake, I have assumed that the correlation between errors of income at time 1 with occupation at time 1 and income at time 1 with income at time 2 are the same; various other assumptions give similar results. 2) The corrected correlations are given by:

$$r_{AB} = \frac{r_{ab} - r_{ef} (\sqrt{1 - r_{aa}} \sqrt{1 - r_{bb}})}{\sqrt{r_{aa} r_{bb}}}$$

where r_{AB} is the corrected correlation, r_{ab} the observed correlation, r_{ef} the correlation between errors of measurement in a and b, and r_{aa} and

r_{bb} are the reliabilities.⁴ The regressions are

⁴ The model is:



then estimated from the corrected correlations. This procedure is identical to the usual errors in variables model in econometrics (Bowles, 1972:228-9, 240-1). 3) The substantive results in this case are insensitive to the precise assumption made about correlated error. The path from income at time 1 to occupation at time 2—the most likely candidate for an historical effect—in fact approaches zero under increasingly extreme assumptions about error; if anything, this slightly weakens the case for the historical model. However, the only other real candidate for an historical effect, the path from occupation at time 1 to income at time 2, gets larger. Other potential historical effects are essentially unchanged, some getting slightly smaller and others slightly larger. And the increment in variance explained by the historical model's eleven additional paths remains miniscule, averaging just over one third of one percent. The same results are produced by additional calculations which assume both correlated error and reliabilities at time 1 ten percent lower than the corresponding figures for later periods. On the whole then, these results offer no added support for the historical model.⁵

Other Matters

1) The link between income at time 1 and occupation at time 2 is the most likely candidate for an historical effect in the PFS data. For this Featherman suggests a status discrepancy

mechanism—men with lower than average income might become motivated to achieve higher status jobs. But while this is possible, other evidence strongly suggests that status discrepancies have little discernible effect (e.g. Jackson and Curtis, 1972). Furthermore, the PFS results would be more persuasive if a similar effect appeared in subsequent time periods; a theory applying only to one time period is not unduly parsimonious. 2) The statistically strongest historical effect is from occupation at time 1 to income at time 2. But neither Featherman nor I can justify it theoretically. Nor is there a corresponding effect at time 3. On balance and pending further evidence, I am inclined to dismiss it. 3) Other than this, there is little statistical justification for any historical effect either in the PFS data or in the Six-Cities data, certainly none with strong theoretical justification.

Measurement. Featherman's comments on the timing of education and of entry into the labor force suggest that these could usefully be included in models of the socioeconomic career. Featherman is also quite right to suggest measuring job characteristics other than status; both of our theoretical analyses use such variables. The measurement of income also deserves further attention. Economists make a strong case for using the log of income rather than dollars, thereby focusing on percentage rather than dollar changes. Also, early in life much investment in education is not from earned income but potential income forgone; a direct measure of that would be highly relevant.

Conclusions. There is strong evidence that Blau and Duncan's causal chain model describes occupational attainment through most of the life cycle. It fits both the PFS and the Six-Cities data remarkably well. Neither different assumptions about the reliability of father's occupation nor assumptions about correlated error undermine this conclusion.

There is good evidence that my causal chain model correctly describes the attainment of occupation and income. Father's occupational status clearly has no direct effect on son's income. And different assumptions about reliability or correlated measurement error in no way alter that finding. There is no good evidence for historical effects on occupation or income in the middle of the life cycle. The results for men between roughly thirty-five and fifty are clear and unaffected by assumptions about reliability or correlated error. Only the earlier part of the life cycle raises a question. But even there the causal chain model seems, on balance, preferable. In all, the two causal chain models have substantial theoretical justi-

By the usual path analysis procedures, the residuals are $p_{ee} = \sqrt{1 - r_{aa}}$ and $p_{er} = \sqrt{1 - r_{bb}}$ and the correlation between observed scores is:

$$r_{ab} = \sqrt{r_{aa}} r_{AB} \sqrt{r_{bb}} + (\sqrt{1 - r_{aa}}) r_{er} (\sqrt{1 - r_{bb}})$$

The formula used in the text follows directly. This reduces to the usual correction for attenuation when there is no correlated error. This correction was used for $r_{11,12}$ and $r_{11,0-10}$. In addition, I assumed that errors in occupation at time 1 were correlated with those in income at time 2 by the very small amount implied by their respective correlations with errors in income at time 1.

⁵ Featherman suggests that the overidentifying restrictions in the causal chain model be used to compute implied correlations between errors. But to do so would merely be to rearrange figures already presented in the paper showing the difference between observed correlations and those implied by the model. For example, the correlation between errors in occupation at times 1 and 3 would simply be the corresponding error in prediction divided by the residuals for occupation at times 1 and 3. My figures seem at least as easy to interpret and no less appropriate, especially since some of the model's slight errors are presumably caused by specification as well as measurement errors.

fication, fit the data well, and are much more parsimonious than the alternatives.

JONATHAN KELLEY

*Columbia University and
Center for Policy Research*

REFERENCES

- Blalock, Hubert M. Jr.
1961 Causal Inferences in Nonexperimental Research. Chapel Hill: The University of North Carolina Press.
- Blau, Peter M. and Otis Dudley Duncan
1967 The American Occupational Structure. New York: Wiley.
- Bohrnstedt, George W. and T. Michael Carter
1971 "Robustness in regression analysis." Pp. 118-46 in Hubert L. Costner (ed), *Sociological Methodology*. San Francisco: Jossey-Bass.
- Bowles, Samuel
1972 "Schooling and inequality from generation to generation." *Journal of Political Economy* 80(May/June):219-51.
- Featherman, David L.
1972 "Achievement orientations and socioeconomic career attainments." *American Sociological Review* 37(April):131-43.
- 1971 "A research note: a social structural model for the socioeconomic career." *American Journal of Sociology* 77(September):293-304.
- Jackson, Elton F. and Richard F. Curtis
1972 "Effects of vertical mobility and status inconsistency: a body of negative evidence." *American Sociological Review* 37(December):701-13.
- Johnston, J.
1963 *Econometric Methods*. New York: McGraw-Hill.
- Kelley, Jonathan
1973 "Causal chain models for the socioeconomic career." *American Sociological Review* 38(August):481-93.
- Palmer, Gladys
1954 *Labor Mobility in Six Cities*. New York: Social Science Research Council.
- Siegel, Paul M. and Robert W. Hodge
1968 "A causal approach to measurement error." Pp. 28-59 in Hubert M. Blalock, Jr. and Ann B. Blalock (eds), *Methodology in Social Research*. New York: McGraw-Hill.
- Treiman, Donald J. and Robert M. Hauser
1970 "On the intergenerational transmission of income: an exercise in theory construction." Unpublished manuscript. New York: Columbia University.
- Wonnacott, Ronald J. and Thomas H. Wonnacott
1970 *Econometrics*. New York: Wiley.

A NOTE ON SYMBOLIC INTERACTIONISM

"Symbolic Interactionism as a Pragmatic Perspective: The Bias of Emergent Theory," appearing in the April, 1973 issue of the A.S.R., misrepresents the position of pragmatism, the views of George Herbert Mead, and my own views which Professor Huber has addressed. The readers of the ASR are entitled to a correction of the misrepresentations.

The central contentions of Professor Huber are the following. (1) Under the precepts of pragmatism and symbolic interactionism, the act of scientific inquiry is initiated without any theory; thus, the position of the investigator is that of the "atheoretical simplicity of a blank mind" (p. 282). (2) Because of this initial absence of theory the symbolic interactionist investigator is forced back on his own views and those of his informants; thus bias is introduced and objectivity lost: "In the absence of theory, the social givens of the researcher and the participants serve as a theoretical framework, giving the research a bias which reflects the unstated assumptions of the researcher, the climate of opinion in the discipline, and the distribution of power in the interactive setting" (p. 282). I wish to discuss briefly each of these two contentions.

(1) Surely, Professor Huber cannot be serious in asserting that pragmatism and symbolic interactionism treat the act of scientific inquiry as beginning with a "blank mind." Neither Mead nor I ever advanced such an absurd position. Mead's view on the matter is stated unequivocally in his classical article, "Scientific Method and the Individual Thinker": mine is given in the article, "The Methodological Position of Symbolic Interactionism." As a reading of these articles should show beyond question, both Mead and I see the act of scientific inquiry as beginning with a problem. Any reasonable consideration of what is involved in the experience of the investigator when he perceives, poses and addresses a scientific problem should show how ridiculous it is to characterize this experience as starting with a "blank mind." Confronted with a problem, the investigator must note given empirical happenings that give rise

to the problem; he must pay attention to the prevailing generalizations or beliefs being challenged by the noted empirical happenings; he must give shape to the problem as it emerges before him; he must identify an area of inquiry implied by the problem; he must form some idea of the kinds of empirical data relevant to clarifying and possibly resolving the problem; and he must sketch out lines of empirical inquiry. To imply that these vigorous orienting actions, essential to the detection, posing and addressing of the problem, are the actions of a "blank mind" is to do violence to meaningful language.

Obviously, Professor Huber's contention reduces to something else. As I interpret her discussion this "something else" is that the symbolic interactionist approach lacks a "logico-theoretic component" and that this lack is equivalent to an absence of theory. Professor Huber's reasoning on this point is gratuitous and quaint. She apparently identifies the "logico-theoretic component" with the use of Aristotelian logic (p. 278), with the consequence that an approach using a different type of logic is automatically lacking in theory! Professor Huber fails to see that posing, clarifying and addressing a scientific problem constitute theoretical action in its own right. In the effort to *shape the problem* for empirical attack, the investigator has to develop premises as to the nature of the empirical world, cut out relevant empirical objects, impute connections between classes of such objects, and form initial conceptions as to the nature of the relationship of these connected classes. To shunt aside this complex of activity as not being theoretical is unwarranted.

The real issue raised by Professor Huber's charge is not that the symbolic interactionist approach lacks theory but, instead, how the scientific problem is to get its theoretical shape. My impression is that she believes that this shaping must precede empirical observation; in this belief she is definitely in line with the dominant methodological position in our discipline today. My own view is that in

studying human conduct or human group life the theoretical shaping of the problem must be done through an on-going, flexible, shifting examination of the empirical field, itself, in order to set the problem correctly. Charles Darwin is the appropriate model for this type of scientific procedure. This is no place to rehearse the differences between these two types of study. I merely wish to point out that Professor Huber is not justified in contending that under the precepts of pragmatism and symbolic interactionism the act of scientific inquiry begins with a blank mind and is devoid of theory.

(2) Professor Huber's second contention is that scientific theory under the framework of pragmatism or symbolic interactionism is bounded by the views of the investigator and his informants and thus becomes a prey to these views. She holds that (a) the investigator is unable to test his assumptions, (b) there is no way to tell which of the informants has the correct views, and (c) the view that triumphs depends on the relative power position of the given informants or the investigator. I suppose that one could find an instance of symbolic interactionist study that fits what Professor Huber is saying; but if so it would be a caricature of proper procedure. There is no reason why the investigator who follows the symbolic interactionist approach cannot test his assertions and hypotheses about his empirical world by a careful, continuous examination of that world; his position is no different from that of Darwin or scores of competent ethnographers. The investigator who is sincere and sensitive to empirical observation is in the same position to find that his given ideas are untenable (and thus "lose the game") as is the researcher operating with a "prior construction of logically-related propositions" (p.282). I can find no basis for Professor Huber's peculiar assertion of this point. Similarly, why is it not possible to test the respective validity of informant views when such views conflict with each other? There are various ways of approaching such a problem, the chief of which is to have the informants thresh out their difference through joint confrontation and hence renew collective examination of the empirical area in dispute. In no sense is this problem inevitable or insoluble as Professor Huber seems to imply. Finally, the notion that the respective power position of informants or of the investi-

gator determines which views among them emerge as "empirically valid" is not backed up by any evidence submitted by Professor Huber. Any conscientious observer who is aware of what is going on, whatever his philosophical persuasion, would not allow himself to fall into such a trap; and if he is sufficiently intelligent and skilled, he will know what is going on. I must conclude that these three boogeymen that Professor Huber gratuitously sees in symbolic interactionist research are in no sense indigenous to or greater in such research. In my judgment, the real source of social bias (as well as the source of its correction) lies in the procedure by which the scientific problem is constructed. The likelihood of introducing unwitting bias is much less when the problem is developed through a close, flexible and reflective examination of the empirical world than when the problem is formed by using a model not derived through such intimate, empirical examination.

A brief comment is due one other questionable line of treatment in Professor Huber's article. I refer to the theme that the test of truth in pragmatic doctrine is whether the given proposition or hypothesis "works." This hoary characterization of pragmatism easily lends itself to absurd interpretation. One should not assume that this crude notion means that the pragmatist fails to examine meticulously the empirical world. The opposite is the case.

Herbert Blumer
University of California, Berkeley

REPLY TO BLUMER: BUT WHO
WILL SCRUTINIZE THE
SCRUTINIZERS?

In considering Professor Blumer's thoughtful comments, let us begin where he left off, since the final issue needs the fewest words. An earlier ASR article (Huber and Loomis, 1970:309) specifically disclaimed the notion that pragmatism involves only the idea that it is important to see how things work out in practice. I had not thought it necessary to repeat the disclaimer. Apparently I was wrong.

The first issue concerns my reference to the blank mind with which the SI researcher

approaches data. This metaphor, harking back to Locke's *tabula rasa*, describes a mind whose knowledge derives from experience. The founder of empiricism, Locke characterized the mind as a sheet of white paper and asked how it came to be furnished. His answer was that all knowledge derives from experience alone because the mind has no innate ideas. But this view fails to account for formal logic and mathematics, neither of which can be derived from experience. Kant corrected Locke by observing that, although the mind contains no innate ideas, it can generate the categories of logic and mathematics which thus arise neither from the mind alone (as rationalists claimed) nor from experience alone (as empiricists claimed). Current philosophy of science also separates logic from sense data and holds that both are necessary for scientific theory. Although no one disputes that Lockean and SI researchers can *think* about problems, or that thinking and the "vigorous orienting actions" Professor Blumer lists may be theoretical *actions*, they are not theory (Lewis, 1973). The issue is whether scientific knowledge can be produced without a prior logical framework. Professor Blumer (1969:43, 44) feels that the conventional analytical schemes in science force data into an "artificial framework" that seriously impairs "genuine empirical analysis." He recommends instead a procedure whose "essence" is "close, shifting scrutiny." Thus he rejects conventional use of a prior logical structure in favor of empirical procedures used with a "different type of logic" which is not described, so far as I have been able to discover, anywhere in his work. The mind of Blumer's observer, like Locke's, is thus a *tabula rasa* because it is furnished by means of empirical observation.

Because I charged that inadequate testing and replication procedures in the SI model permit the influence of power and personality on findings, the real issue between us concerns the rules for deciding whether a proposition is true. In the initial stages of research, abductive reasoning (including observational statements) helps put a problem into testable form. But what is the purpose of all this "theoretical" activity? At what point do you pin something down precisely so that it can be tested? Precise concepts are unsuitable, Professor Blumer (1954:8, 9) holds, because of the varying nature of reality. Instead,

researchers should rely on "sensitizing concepts" grounded on sense instead of on explicit objective traits; such concepts are formulated and communicated by "... exposition which yields a meaningful picture, abetted by apt illustrations which enable one to grasp the reference in terms of one's own experience." But the rules for deciding whether one picture is more "meaningful" than another are not discussed.

In order to learn how SI research should be tested, let us examine the description of methods that Professor Blumer (1969:1-60) recommends, using his phrasing as much as possible. The page numbers in parentheses will help the reader to assess whether the following summary is accurate.

Since the researcher usually does not know at first hand the sphere of life he wants to study (35), Blumer says, he unwittingly forms a picture of it, using images he already has (36). This is all right if the images are revised and tested by first hand experience, but this motif does not prevail in social science (37). Instead, images substitute for experience; the researcher relies on starting with a theory, an hypothesis, and a mode of inquiry to test it. Thus the protocol of inquiry becomes the unwitting substitute for direct examination of the real world (38).

pin something down precisely so that it can be tested? Precise concepts are unsuitable,

What the study of the ongoing real world requires, Blumer says, is a high order of careful and honest probing, creative yet disciplined imagination, resourcefulness, flexibility, pondering, and a constant readiness to recast one's images (38). This world can best be studied by two modes of naturalistic inquiry: exploration and inspection (40). Exploration is a flexible procedure whose purpose is to lead to a clearer understanding of how to pose the problem (40). Such direct examination sets the need for another procedure, inspection or analysis, in order to cast the problem in theoretical form and formulate theoretical propositions, the aim of empirical science. [In this passage Blumer is insensitive to what Peirce called the ethics of terminology for, as we shall soon see, inspection does not constitute scientific analysis as that phrase is customarily used.]

How is scientific analysis to be done in direct examination of the real world? By inspection, Blumer says, an intensive focused

examination of the empirical content of whatever analytical elements are used (43), such as "integration" and "social mobility" (44), and of the relations between such elements. Inspection proceeds by subjecting such elements to meticulous examination by careful, flexible scrutiny of the empirical instances covered by the analytical element; it is not preset, routinized, or prescribed, for "... it only becomes such when we already know what it is and thus can resort to a specific test, as in the case of a technician (44)." The antithesis of scientific inquiry in the current methodology (45), inspection is appropriate for pinning down and testing the relations between analytical elements by flexible scrutiny of empirical instances (46). Without inspection one is captive to one's prior image of the relationship (46).

Thus SI research should be tested by scrutiny. If the scrutiny is not sufficiently intense and flexible, the researcher presumably remains captive to prior images. How researchers know that they have scrutinized hard enough, Professor Blumer does not say. Although SI researchers may scrutinize harder than others, the injunction to look again is an inadequate prescription for testing or replication. Conspicuously, the people who write about SI research don't do it, and the people who do it do not use the basic SI paradigm; if they are good sociologists, they throw in other components, as appropriate (Bucher, 1973). Logical analysis of the SI model shows that its use gives the researcher no rules to decide whose picture of reality is most meaningful, and hence allows the intrusion of power and personality factors into the picture that is drawn.

The pragmatic model also lacks clear rules for deciding what is a "satisfactory" outcome. Mead (1964:341-2) claims that the pragmatic criterion of truth is the continuance of the common world; the test is the ability to act where the action was formerly stopped. Thus Mead implicitly accepts the given distribution of power. Problems arise, Mead says, because the common world continually breaks down when the natural processes of men are in-

hibited. The solution is found in resolving the inhibitions. Thus Mead sees the ongoing world as unproblematic; the problem arises when the action stops, and the solution is to get it started again. Mead and Dewey avoid the implications of this model by using examples of technological rather than social problems to illustrate their methods.

What is best about the SI or pragmatic formulation is the injunction to inspect the real world; too often students engage in secondary analysis with little first hand knowledge of the situation. But nothing guarantees that sociologists will scrutinize more carefully than other people. A sensitive essay may be a delight to read, but a word picture that can be refuted only by a more "sensitive" word picture leaves unanswered the main question: on what grounds does one choose one picture rather than another? Professor Blumer's suggestion that, in case of disagreement, the informants simply settle the matter by threshing out their views in joint confrontation supports my criticism of the SI model better than anything I could say.

Joan Huber
University of Illinois, Urbana

REFERENCES

- Blumer, Herbert
1954 "What is wrong with social theory," *American Sociological Review* 19 (February):3-10.
1969 *Symbolic Interactionism: Perspective and Method*. Englewood Cliffs, N. J.: Prentice-Hall.
- Bucher, Rue
1973 Personal Communication.
- Huber (Rytina), Joan and Charles P. Loomis
1970 "Marxist dialectic and pragmatism: power as knowledge," *American Sociological Review* 35 (April):308-18.
- Lewis, J. David
1973 "The methodology of Herbert Blumer." Unpublished paper.
- Mead, George Herbert
1964 *Selected Writings*. Edited, with introduction, by Andrew J. Reck. Indianapolis: Bobbs-Merrill.

COMMENT ON
"ROLE DIFFERENTIATION"

Bales and Slater's initial research on role differentiation (1955) was concerned with the origin of differentiated social structures and the development of distinct roles in groups. Small laboratory groups, they thought, would provide insight into "role-differentiation 'in the making' from some minimal level, in the hope that the character of the minimal phenomena may give clues as to very general forms and reasons for development of role differentiation" (p. 260). Following their example a research tradition has developed whose goal is to understand the development of task and social-emotional leadership role differentiation.

Questioning all this research, however, Lewis (1972) has recently asked whether there is any evidence for role differentiation. He argues that there is none, and that past evidence purporting to show differentiation in fact shows integration. Two aspects of Lewis' presentation deserve comment. The first, and most important, is his definition of role differentiation; the second is measurement procedures.

Definition Problems. Lewis' definition is probabilistic. He suggests that if we adopt "the convention that holding a role is equivalent to being the member most closely associated with a role [as done in past research], it seems reasonable to define a pair of roles as (1) *differentiated*, if knowing that a certain member holds one role decreases the probability that he holds the other role, [and] (2) *integrated*, if knowing that a certain member holds one role increases the probability that he also holds the other role . . ." (p. 427). To assess this probability or tendency, Lewis calculates the relative frequency across groups with which the two roles (task and social-emotional) are held by different persons. If this frequency is higher than chance expectation, role differentiation is said to exist; if lower, then role integration.

The problem with Lewis' definition is that role differentiation is not conceptualized as a property of an individual group. Rather, it is seen as a property of (1) a population of groups, or (2) of the general culture in which problem-solving discussion groups are formed. By definition, Lewis' methods do not permit measuring role differentiation for any single group. Yet, Bales and Slater, explicitly and implicitly, conceptualized role differentiation as a property of

individual groups. Bales (1958) writes, for example, that

the question as to whether or not there is role differentiation *within a group* can be reduced in part to whether group members show some consensus that certain members stand higher than others on a given criterion and whether different criteria give different status orders rather than a single status order (p. 440, emphasis added).

Lewis' definition alters the original concept substantially. No longer is the development of differentiated role structures in groups to be explained; rather, Lewis' concern is with populations of groups with differing probabilities for the joint performance of roles. The unit of analysis is not the group but the population or culture. This definition cannot be said to be incorrect or useless, however, for all definitions are in some ways arbitrary; and the phenomenon defined by Lewis may well be worth investigating (cf. Zelditch, 1955). The point is, rather, that his definition is not consistent with much work that has been done, and his criticism of that work, therefore, is not entirely relevant.

The source of Lewis' thinking was perhaps Slater's (1955) suggestion that "a consistent tendency for subjects to rate one man high on one criterion and another man high on a second criterion would constitute *prima facie* evidence for the existence of a set of differentiated roles . . ." (p. 300). On the surface, Slater's notion of a "tendency" and Lewis' notion of a "probability" may seem to make their conceptions of role differentiation very similar. However, Slater's very next statement suggests that his notion of "consistent tendency" refers to repeated assessments over time of the same group and not over different groups at the same time, for he asks "what effect do repeated interactions [over time] have on such discriminations?" (p. 300). Both Bales and he recognized that the differentiated structure is the result of a rather involved process, and, like all structures, in reality, is only a cross section at one point in time of that process. Furthermore, several sampling points in time or a summarizing measure is needed to properly assess the existence of differentiated roles within a group. In short, Bales and Slater (and most researchers following that line of research) conceptualized role differentiation as a property of individual groups and not of populations of groups.

Lewis' definition also has implications for research evaluation. For example, in his concluding remarks (p. 433), he calls for research on role relations in different areas. He suggests that "the kinds of explanations and theories postulated to explain the absence or presence of role *differentiation* are not likely to be the ones most helpful in explaining the presence or absence of role *integration*." He also suggests that "in the search for conditions under which roles are integrated and those under which they are not, the process of search, the places searched, the experiments done, and the questions asked are all likely to differ from those in the past." This view of past research must be seriously questioned, however, if role differentiation is seen as a property of individual groups; for under that definition differentiation and integration are opposite sides of the same continuum: research on one is, by definition, research on the other.

The Measurement Problem. The second aspect of Lewis' work which deserves comment concerns biases in his procedures for measuring role performance. If we begin with Slater's (1955) definition of role as "a more or less coherent and unified *system of items* of behavior" (p. 300), it seems strange that both Bales and he, as well as Lewis now, have measured role performance in essence by a single item of reported behavior: "providing the best ideas" for the task role, and "being best liked" for the social-emotional role. Later factor analyses done on sets of items (e.g., Burke, 1967) have shown that, while "best ideas" may be adequate as a single-item measure of task leadership performance, "best liked" is clearly inadequate. Of all social-emotional items it has the lowest correlation with the social-emotional factor. Furthermore, it is the social-emotional item which is most strongly correlated with the task factor. Use of these two items, therefore, produces a greater correspondence between the task and social-emotional dimensions than in fact exists and, hence, too few groups classified as differentiated.

Halo effects in the use of the rating scales to assess role performance are another source of measurement bias. This bias, which causes people to be rated similarly on both the task and social-emotional dimensions, would also result in underestimating the number of differentiated groups in a sample. Although Lewis, in a footnote (p. 425), acknowledges some measurement problems, he appears not to have realized the gross number of under-estimations that can occur with these measures.

In sum, the definition of task and social-emotional leadership role differentiation given by Lewis differs substantially from that of most

researchers; the result is entirely different foci for analysis. Lewis' definition applies to a culture or population of groups, while most other researchers concern themselves with individual groups. Lewis' concern about the greater than chance likelihood that the task and social roles will be combined, therefore, contributes little to our understanding of group processes which differentiate the two roles. Second, examination of the measurement problems suggests that, contrary to Lewis' suggestion, we can neither affirm nor deny a general tendency for groups to combine the two roles.

PETER J. BURKE

Indiana University

REFERENCES

- Bales, Robert F.
1958 "Task roles and social roles in problem-solving groups." Pp. 437-47 in Maccoby, Newcomb, and Hartley (eds.), *Readings in Social Psychology*. New York: Holt, Rinehart and Winston, Inc.
- Bales, Robert F. and Phillip E. Slater
1955 "Role differentiation in small decision-making groups." Chapter V in Talcott Parsons et al. (eds.), *Family, Socialization, and Interaction Process*. Glencoe, Illinois: Free Press.
- Burke, Peter J.
1967 "The development of task and social-emotional role differentiation." *Sociometry* (December):379-92.
- Lewis, Gordon
1972 "Role differentiation." *American Sociological Review* 37(August):424-34.
- Slater, Phillip E.
1955 "Role differentiation in small groups." *American Sociological Review* 20(June):300-10.
- Zelditch, Morris, Jr.
1955 "Role differentiation in the nuclear family: a comparative study." Chapter VI in Talcott Parsons et al. (eds.), *Family, Socialization, and Interaction Process*. Glencoe, Illinois: Free Press.

REPLY TO BURKE

In "Comment on 'Role Differentiation'," Burke discusses what he considers to be two problems in reassessing the evidence for role differentiation (Lewis, 1972). These were (1) that the definition of role differentiation used in the reassessment changed significantly the unit of analysis, from individual groups to populations of groups, and (2) that there were measurement problems which probably resulted in

underestimating the extent of role differentiation. Both points merit reply.

Definitional Problems. Although the paper in question discussed several definitions of role differentiation, Burke's concern is the definition which says that a pair of roles (R_i , R_j) is differentiated if

$$\Pr(R_i = A \text{ and } R_j = A) < \Pr(R_i = A) \cdot \Pr(R_j = A), \quad (1)$$

where " $R_i = A$ " means that R_i is held by person A. Burke's complaint is that such a definition departs from the traditional concern of role differentiation, since the definition is not oriented to individual groups.

If a researcher knows that a given person of a group is the task leader, would the researcher be more likely or less likely to predict that that person would also be the social-emotional leader? Or would it make no difference? It seems clear to me that Bales' (1953) comments about the incompatibility of these roles is equivalent to saying that one should be less likely to so predict, and the prediction applies clearly for a single group. The reanalysis did deal primarily with sets of groups, but the definition of role differentiation is relevant to individual groups.

Still, I do not expect this argument to satisfy Burke entirely since I think his real concern is somewhat different. That the unit of analysis can hardly be the real problem is clear from examining the analysis in question (Lewis, 1972) and the more "traditional analyses." In the paper in question, each group was classified according to whether the task and social roles were jointly held or not, and the number of such groups was reported. If one takes Burke's analyses as well as some of Bales and Slater's, the data are often reported in ways which provide no information about individual groups. Although Burke (1967, 1968, 1969) used a measure of role definition defined on individual groups, he always reports correlations between it and other measures, correlations over the set of all groups. Other analyses, which Burke presumably views as treating role differentiation in an appropriate manner, have reported such things as the mean curve on ideas and liking (Bales, 1953:147, Chart 2) and the mean interaction profile (Bales and Slater, 1955). Obviously, none of these is directly related to the phenomenon within a particular group.

I suspect that the real problem is not the definition of role differentiation, but the interpretations which often accompany it. As far as I can tell, Bales, Burke and I are basically in agreement about whether roles are jointly held or not, and in agreement that role differentiation has not occurred where the roles are jointly held. The problem arises in interpreting the significance of roles which are not jointly held, or as

Burke prefers to put it, those situations where different criteria give different status orders.

The prevailing interpretation has been that task and social roles are incompatible. Another interpretation is that they are simply unrelated. Whatever it is that causes one person to be selected as task leader may not be related to the factors which enter into selection of a social-emotional leader.

The null model used in the reanalysis assumes that the task and social-emotional leaders are selected independently and with equal probability. Null models of this type are often straw men put up to be knocked down when better models could (and should) be posited. The merit of the model, however, is that it allows one to calculate the expected number of groups in which the roles will be jointly held and the number in which they will not. The fact that there are more groups in which the roles are not jointly held than groups in which they are is a phenomenon predicted by the model. If something about these roles makes them incompatible, we would expect to see even fewer cases of joint role occurrence than the number predicted by the null model. In the present case, the null model is not knocked down. In fact more instances of role integration are found than expected.

Measurement Problems. Burke's second concern was that in establishing the social-emotional leader the analysis used questions about affect rather than questions about social-emotional leadership. In 1967 Burke demonstrated that these are not equivalent, a fact which was pointed out (Lewis, 1972:425). Why then were the questions on liking used? The point of the paper was to assess the evidence for role differentiation, including the original evidence adduced by Bales and Slater. Consequently, it seemed fitting to use the operational definitions of Bales and Slater. After all, it was their findings which set in motion the rather extensive research into role differentiation, including Burke's own work. If the original data showed weak support for role differentiation, then the later work would at best have been on the right track for the wrong reason.

Burke expresses his conviction that using the question on liking will result in "too few groups classified as differentiated" (Burke, 1973). Fortunately, one of the studies (Bonacich, 1968) used a set of questions reasonably comparable to Burke's. The questions dealing with task leadership were:

1. Who had the most ability in constructing the crossword puzzles? Please rank the members in order. . . Include yourself. . .
2. Who did the most to guide the group in constructing the crossword puzzles and keeping

the discussion moving? Please rank the group members. . . Include yourself.

The following questions concerned social-emotional leadership:

9. Who do you think showed the greatest concern for the feelings of the other group members and generally tried to keep the members together as a unit? Rank the group members. Include yourself.
11. For each of the other group members would you please indicate your agreement or disagreement with each of the following statements:
 1. I would enjoy an animated discussion with him.
 2. I would want to have him in the same seminar.
 3. I would want him to come to me with his problems.
 4. I would discuss important personal problems with him.
 5. I would enjoy talking with him.
 6. I would like to see him around school sometime.
 7. I would like to discuss serious general problems with him.

The first and second questions are most comparable to Burke's questions 11 and 2. Questions 9 and 11 are most comparable to Burke's questions 10 and 8 (Burke, 1967:384).

The report under question (Lewis, 1972) used question 1 to identify the task leader and question 11 to establish the social-emotional leader. Since Burke is concerned about underestimating role differentiation by incorrectly identifying task and social-emotional leaders using single items and questions about liking (and presumably social distance questions in general), I have recalculated the task and social-emotional leaders for the Harvard data and tabulated the joint occurrence of task and social-emotional roles.

In combining the two questions with regard to each function, I used the weights reported by Burke (1967:384), which appeared to be stable across two studies (Burke, 1967, 1968), as we did in the analysis of function specialization (Bonacich and Lewis, 1973). The weights were .85 and .83 for questions 1 and 2 and .85 and .61 for questions 9 and 11. To obtain the ranking of the individuals in a given group on the task dimension, the weighted sum of ranks on question 1 was added to the weighted sum of ranks on question 2 for each individual. To obtain the ranking of individuals on the social-emotional dimension in each group, the "positive" responses by person *i* towards person *j* on question 11 were summed, weighted, and added to the weighted sum of ranks for the person on question 9. (Further details about these calculations can be found in Bonacich and Lewis, 1973:35.)

Table 1 shows the results. The results are substantially the same whether one uses the single questionnaire items to establish the task and social-emotional leaders or whether he uses Burke's preferred method of weighting the questions more specifically oriented to how the person conducted himself in the task and social-emotional area.

Finally, another claim in Burke's comment deserves clarification. He asserts that since "role differentiation and role integration are opposite sides (sic) of the same continuum, research on one is, by definition, research on the other." Research on one end of a continuum is not necessarily equivalent to research on the other. Knowing about the process of socialization in children may or may not illuminate socialization processes among elderly, though young and old are at opposite ends of the age continuum.

Table 1. Frequency of Joint Occurrence of Ability and Liking or Social-emotional Roles, by Type of Group and Session, Harvard Data

Sex	Session	N	Expected Occurrence	Observed Occurrences	
				Prior Analysis*	New Analysis
Females	1	16	3.2	5	4
	3	16	3.2	8	8
Males	1	8	1.6	5	5
	3	8	1.6	1	3
Total	--	--	9.6	19	20

*Lewis (1972:428)

Research always stems from conceptualizing how things are; and if one assumes that role differentiation is more prevalent or worthy of understanding than role integration, he is likely to plan his research accordingly.

The purpose of the review was not to claim that role differentiation always occurs or never occurs. It was to demonstrate that role differentiation is not as securely grounded as many sociologists believe. The final point of the review was the need for research into why roles are related in different ways, which is precisely the area in which Burke's work has its greatest merit.

GORDON H. LEWIS

Carnegie-Mellon University

REFERENCES

- Bales, Robert F.
1953 "The equilibrium problem in small groups." Pp. 111-61 in T. Parsons, R. F. Bales, and E. A. Shils, Working Papers in the Theory of Action. New York: Free Press.
- Bales, Robert F. and Philip E. Slater
1955 "Role differentiation in small decision-making groups." Chapter V in Talcott Parsons et al. (eds.), Family Socialization and Interaction Process. Glencoe, Illinois: Free Press.
- Bonadich, Phillip
1968 Specialization and Differentiation in Small Laboratory Groups. Ph.D. Dissertation, Harvard University.
- Bonadich, Phillip and Gordon H. Lewis
1973 "Function specialization and sociometric judgments." Sociometry 36(March):31-41.
- Burke, Peter J.
1967 "The development of task and social-emotional role differentiation." Sociometry 30 (December):379-92.
1968 "Role differentiation and the legitimation of task activity." Sociometry 32(December):404-11.
1969 "Scapegoating: an alternative to role differentiation." Sociometry 32(June):159-68.
- Lewis, G.
1972 "Role differentiation." American Sociological Review 37(August):424-34.

PATTERNS OF FEMALE INTERGENERATIONAL OCCUPATIONAL MOBILITY:
A COMMENT

The recent exchange of letters between critics and authors of the article "Patterns of Female Intergenerational Occupational Mobility . . ." (DeJong et al., 1972; Havens and Tully, 1972) prompted me to read the original piece to confirm or disconfirm certain suspicions aroused by reading the correspondence. Even my most unfavorable guesses turned out to be so mild in comparison with reality that I feel impelled to join the debate.

DeJong, Brawer, and Robin, the authors of the article, (DeJong et al., 1971) apparently intended to say something of value and interest concerning the occupational status and mobility of women in contemporary American society. They present results of a secondary analysis of six NORC surveys whose background data included sex, occupation (or former occupation for those not at work) and father's occupation. I have not been in a position to follow the recent American studies on American women's participation in the occupational sphere, but I venture the guess that DeJong et al. probably are among the few students of this question to show any interest in the effects of diversity in social origins (father's occupation) on women's occupational endeavors and achievements. So there is a certain degree of novelty and originality in the design of their investigation which might have increased our understanding of the status of women. The research design might have supplemented what we know about the effects of women's multiple contemporary statuses on their occupational activities, by adding to these the effects of women's diverse status biographies.

What are the crucial aspects of women's occupational participation in contemporary society? First, women are less likely to be gainfully employed than men; second, those who are so earn fewer occupational rewards than men, and third, (combining the first two) their occupational careers show more interruptions and lower promotion rates than men's. DeJong et al. have managed to eliminate all these features of women's occupational performance in their study. They do away with the first by considering only those women in the NORC material who are or have been at work (and take these together). They eliminate

the second and third by withholding all information about the occupational distributions of the women who are (or have been) at work.

What is left? DeJong et al. claim to have been inspired by the work of Blau and Duncan (1967) and to have modelled their analysis after the work of these authors. Now the Blau and Duncan work consists of diverse observations and techniques for casting light on the occupational structure. The article at issue is modelled on the first analytical chapter in Blau and Duncan, which is concerned with the following problem: the division of labor expresses itself in part in the frequency distribution of the members of the labor force over the set of occupations making up the occupational structure. This distribution changes as an expression of changes in the economic and technological systems. Given such changes, how do recruits to the labor force, coming as they do from occupational origins so distributed as to have been adjusted to the occupational structure of a previous time, enter into the new division of labor?

I have worked with this set of questions myself, and used approximately the same techniques of analysis as Blau and Duncan (Rogoff, 1953, Rogoff Ramsøy, 1965, 1966); and I have never been satisfied either with the formulation of the problem or with the methods used to examine it. For one thing, we assume that the occupational distribution observed at a given point in time is fully explained by the state of technology and by the economic forces operating within a self-contained labor market—that there are no lags, maladjustments, or institutionalized features at work other than technological or economic. Then, turning our back on all these assumptions, we declare that it is the social stratification system that accounts for the movement out of occupational origins and into occupational destinations. It seems to me that we should be asking: what would the occupational distribution be like if there were indeed a perfect adjustment, a true equilibrium between supply and demand in the labor market? And what would the occupational distribution be like if the forces of social stratification were given free play, left uncorrected by other institutionalized orders? [Raymond Boudon addresses himself to the

last question in recent work. See Boudon (1970)]. Instead, we allow both of our sets of assumptions to go untested, and thereby turn both the techno-economic order and the stratification order into sealed black boxes. If we knew what the occupational distributions would be like under the two sets of "pure" conditions, then we could, by comparing these with the observed distribution, make far more interesting and adequate statements about the relative strength of the forces at work on the division of labor.

DeJong et al. have simply increased the number of black boxes by one—the institutionalized order of inequality of status between the sexes. Inequality in sexual status obviously affects the distribution of men and women in the occupational structure—a phenomenon which the authors have covered over and confounded with the effects of the stratification system. Since they claim to have used my work as a source, I may be permitted to mention that had they turned a few more pages, they would have seen my attempt to evaluate the effects on occupational "in"- and "outflows" of the institutionalized order of inequality of status between blacks and whites. I did this by the simplest possible method: I used the distribution of the occupational destinations of white males as the expected distribution for black males. The same could have been done by DeJong et al., using the male distribution as the expected values for females. This would have brought out the specific effect of inequality in status between men and women on female in- and outflows.

Even then, however, it would have been less than satisfactory to use, as the authors did, the ten broad occupational groupings of the Bureau of the Census. Blau and Duncan used seventeen; but the current authors could not, because they worked with only 2,371 observations on females and—given the fact that the seventeen categories take their point of departure from the occupational distribution of males—some of the categories would have been as good as empty for a female sample. Their decision to use ten categories simply flies in the face of common sense knowledge of the kinds of occupations women are engaged in, in contrast to men. How can they talk about inheritance of "professional" occupations, when almost two-thirds of women professionals are either nurses or teachers, in contrast to one-tenth of male professionals? This is a perfect example of how they confuse the study of supply and demand in the labor force with the workings of the stratification order, and com-

pound the confusion by mixing in the effects of sexual status inequalities as well.

NATALIE ROGOFF RAMSØY

*Institute of Applied
Social Research, Oslo*

REFERENCES

- Blau, Peter M. and Otis Dudley Duncan
1967 *The American Occupational Structure*. New York: John Wiley and Sons, Inc.
- Boudon, Raymond
1970 "Essai sur la mobilité sociale en Utopie." *Quality and Quantity* 4(December):213-41.
- DeJong, Peter Y., Milton J. Brawer and Stanley S. Robin
1971 "Patterns of female intergenerational occupational mobility: a comparison with male patterns of intergenerational occupational mobility." *American Sociological Review* 36(December):1033-42.
- 1972 "Reply to Havens and Tully." *American Sociological Review* 37(December):777-9.
- Havens, Elizabeth M. and Judy Corder Tully
1972 "Female intergenerational occupation mobility: comparison of patterns?" *American Sociological Review* 37(December):774-7.
- Rogoff, Natalie
1953 *Recent Trends in Occupational Mobility*. Glencoe: The Free Press.
- Rogoff Ramsøy, Natalie
1965 "On the flow of talent in society." *Acta Sociologica* 9:152-74.
- 1966 "Changes in rates and forms of mobility." Pp. 213-34 in Neil J. Smelser and S. M. Lipset (eds.), *Social Structure and Mobility in Economic Development*. Chicago: Aldine Publishing Co.

PATTERNS OF FEMALE INTERGENERATIONAL OCCUPATIONAL MOBILITY: RESPONSE TO RAMSØY

Some readers of our article (DeJong, et al., 1971) have been disconcerted because our findings failed to fulfill their expectations. We share this experience with them, since at the initiation of our study, we too expected the occupational mobility patterns of women to differ markedly from the patterns for men. We were surprised to discover that in each of our male-female comparisons, there were no major differences in the *patterns* of mobility. The findings have been disturbing to us, to Professors Havens and Tully, and now to Professor Ramsøy.

Our initial response was identical to that of our critics: to examine the methods and logic

of the research. Obviously, if a logical or methodological flaw could be found then the disturbing findings could be rejected. Professor Ramsøy issues the third challenge to our methods and logic. We issued the first challenge in the article itself (see paragraph two of Conclusion, DeJong, et al., 1971:1040); Professors Havens and Tully issued the second (see Havens and Tully, 1972). A review of Professor Ramsøy's criticisms enables us to clarify further. To the extent that our critics seek answers to the same questions we posed, our logic and methods seem sufficient. To the extent that the findings raise additional and perplexing questions, we share our critics' concerns. But when our critics scold us for not asking questions they would ask, we are being unreasonably held accountable.

Reviewing our findings, Professor Ramsøy now asks, "What are the significant and crucial aspects of women's occupational participation in contemporary society?" She then offers these answers. 1) Women are less likely to be gainfully employed than men, 2) they earn fewer occupational rewards, and 3) their careers show more interruption and lower promotion rates than men. She then concludes critically that "DeJong, et al. have managed to *eliminate* all of these features of women's occupational performance in their study."

These same questions could have been applied to past research on male intergenerational occupational mobility which used the same methods. One could ask, "What are the significant and crucial aspects of *men's* occupational participation in contemporary society?" 1) Men are more likely to be gainfully employed than women, 2) they earn more occupational rewards, and 3) their careers show fewer interruptions and higher promotion rates than women. Hence, one might conclude that the works of Blau and Duncan, Rogoff and myriad others have eliminated all these features of men's occupational performance.

We do not intend to question past research on male occupational mobility on these grounds. Such research was not directed at these questions. So too, Ramsøy's criticisms are either inappropriate to such mobility research, or misdirected when aimed at the article that has initiated a comparison of male-female occupational mobility patterns. We fully recognize that significant differences

exist between males and females in the work force. Neither the research nor its findings deny these differences, but the research is confined to mobility patterns not mobility per se.

The last sentence of Professors Ramsøy's second paragraph reads, "The research design was such that it might have supplemented what we know about the effects of women's multiple *contemporary* statuses on her occupational activities, by adding to these the effects of women's diverse status biographies." It puts us in mind of C. Wright Mill's claim that "The sociological imagination enables us to grasp history and biography and the relations between the two in society It is the capacity to range from the most impersonal and remote transformations to the most intimate features of the human self - and to see the relations between the two" (Mills, 1959:6-7). An intergenerational mobility table is a start at relating history and biography, but used alone leaves us far removed from "the most intimate features of the human self." We regret the pragmatic limits in scope to any investigation, especially one based on secondary analysis, but we will not permit the absence of what might have been done to diminish the import or validity of that which has been done.

Professor Ramsøy also takes us to task because one article is not a book. Neither is it a library. We are engaged in additional research. At present, some of her questions are treated in DeJong (1972).

Professor Ramsøy indicates her own dissatisfactions over the years "with both the formulation of the problem or with the methods used to work with it." Subsequently, we are criticized for not solving many current difficulties in studying mobility, stratification and other institutionalized orders in society. She charges that we "have simply increased the number of black boxes by one - the institutionalized order of inequality of status between the sexes." To the contrary, our efforts were to illuminate the social fact of sexual inequality by the use of a standardized technique which should have reflected this inequality *if it functioned as theoretically conceived*. That the analysis failed to yield anticipated findings seems to us instructive rather than obscuring.

Her concern about the use of ten broad occupational categories has been raised be-

fore. We have answered it as best we can in footnote 12 in the original article, and by examining the occupational data when measured by status scores (DeJong, et al., 1972). We were limited to the data available to us. Almost two-thirds of our occupational data for women were classified *solely* by the ten occupational categories used. It must be stressed that our use of ten rather than seventeen occupational categories was forced on us by the data, and did not result from Professor Ramsø's allegation that "some of the categories would have been as good as empty for a female sample."

Overall, we are gratified by the burdens Professor Ramsø places on our article. It is rewarding that this work has induced her to pose such basic challenges to the theoretical and methodological approaches used in this area. That we share many of her concerns, can be seen by a careful, objective reading of the "Conclusion" of the article and our response to Havens and Tully.

Her reference to her analysis for comparing the mobility of blacks and whites is instructive. We did not think of applying this analysis to our data comparing males and females. Such an analysis would address important but different questions than those considered in our research. We thank Professor Ramsø for the reference.

A final word should be unnecessary, but seems to be required. In presenting our findings we were not advocating any ideology. We expect that colleagues will further research these findings. Such research will expand the inquiry into the areas suggested by Professor Ramsø. At present, however, it would be more fruitful to explain the findings rather than explain them away.

Peter Y. DeJong
Calvin College

Milton J. Brawer
Western Michigan University

Stanley S. Robin
Western Michigan University

REFERENCES

- DeJong, Peter Y.
1972 Factors Instrumental in Female Occupational Status: A Comparison to Factors Instrumental in Male Occupational Status.

Doctoral Dissertation, Western Michigan University.

DeJong, Peter Y., Milton J. Brawer and Stanley S. Robin

1971 "Patterns of female intergenerational occupational mobility: a comparison with male patterns of intergenerational occupational mobility." *American Sociological Review* 36 (December):1033-42.

1972 "Reply to Havens and Tully." *American Sociological Review* 37 (December):777-9.

Havens, Elizabeth N. and Judy Corder Tully

1972 "Female intergenerational occupational mobility: comparison of patterns." *American Sociological Review* 37 (December):774-7.

Mills, C. Wright

1959 *The Sociological Imagination*. New York: Basic Books.

REPLY TO SIMPSON*

Professor Simpson's paper contains several useful observations on the study of the relations between social or occupational mobility and fertility. He obscures the value of these observations, however, by presenting them as if they are criticisms of my paper "Social Mobility and Fertility" (Hope, 1971). In what follows I dispute his comments only in so far as they are presented as if they invalidated the contents of that paper.

In an academic dispute one may always doubt the pretensions of a critic to have adequately characterized his opponent's position unless he offers quotations from the work he is attacking, giving especially the writer's definition of his purpose. This Professor Simpson does not do. He writes

Had Hope (1971) begun by carefully examining the impact of movement in and out of the specific pairs of classes and had he carefully looked at the relative effects of different kinds of mobility: income, occupational, educational, and geographic, we would be in a better position to specify mechanisms which link mobility to fertility.

*The following reply points out that the paper (Hope, 1971) which Professor Simpson has attacked explicitly presented a formal, rather than a substantive, analysis of a social mobility effect.

To which I reply that we should indeed, but how could I pretend to derive information on these matters from a 4 x 4 mobility table relating the past and present occupational positions of men to their fertility? The Oxford Social Mobility Group, of which I am a member, has recently completed a survey of 10,000 British adult males and it is hoped that analysis of these extensive data will throw light on some of the questions which Simpson raises.

A reader of Simpson's "Comments," lacking any verbatim statement of the purpose of "Social Mobility and Fertility," might suppose that my purpose in reanalysing Berent's mobility table was to reveal processes or mechanisms which relate mobility to fertility, and that I failed to accomplish that purpose. Let me therefore repeat my opening remarks.

In this paper data which have previously been held to satisfy the additive hypothesis are re-examined to see whether in fact they satisfy that hypothesis, either in its original form or in a modified form. The analysis is carried out entirely within the terms laid down by the preceding work. These limitations have been observed quite deliberately in order to ensure that the two sides of the argument come adequately to grips with one another.

There is nothing here about processes or mechanisms. I could not have been more explicit in renouncing any intention to step outside the bounds of the work I was discussing, and that work was not Berent's original analysis of his data but the reanalyses and reinterpretation by Duncan (1966) and Blau and Duncan (1967).

I have in fact addressed myself elsewhere to the two questions which I am accused of ignoring: (1) how adequate are the occupation categories used by British research workers as guides to the life-styles, values and other social characteristics of their members?¹ (2) how constant are occupational mobility effects over the occupational scale?² But the

question of whether or not I have tackled these and other interesting problems is really irrelevant to any criticism of "Social Mobility and Fertility" since (a) I did not purport to tackle them in that paper, and (b) in order to accomplish my explicit purpose I necessarily had to confine myself within the bounds set by Duncan and Blau. If an author has specifically undertaken to examine, and if possible improve upon, the formal adequacy of his predecessor's work, then any attempt to introduce novel substantive considerations will only confuse the reader and will belie his intent. Simpson's comments confound (a) the analysis of the fit of a model³ with (b) the explanation of the mechanisms which cause the phenomena to fit (or depart from) the model. For example, he writes

Hope immediately assumes that mobility between category I and II is moderated [sic] by the same process as mobility between I and III or II and III. Neither Hope nor his predecessors discuss the possibility that no universal or unitary mobility effect up or down is in force and that effect of mobility is dependent upon or conditioned by the characteristics of each combination of classes.

In fact I clearly distinguished between the formal assumptions of the mobility models and the social processes behind the phenomena when I wrote, "Sociologists might feel that the establishment of a simple additive law would have a miraculous quality which would cry out for intensive investigation of the mechanisms which bring it about. Indeed, the fact that the additive hypothesis is even approximately true leads one to wonder how it is that the values and life-styles of former and newly-encountered social aggregates lawfully modify conception-decisions, when movement between those aggregates appears so various in its abruptness, its finality, its extent, its salience and its temporal relations to the childbearing period."

Simpson's contribution to the discussion of social mobility and fertility is to apply an adaptation of Goodman's "ransacking" tech-

¹ See the editorial introduction to *The Analysis of Social Mobility: Methods and Approaches*. The point is further examined in a forthcoming monograph on the grading of occupations by John H. Goldthorpe and K. Hope.

² See the paper "Quantifying Constraints on Social Mobility" in *The Analysis of Social Mobility: Methods and Approaches*.

³ There is a defect in Simpson's formulation of the results of my analysis where he states that "there is a lower mobility effect (m)." In fact I said that "the sum of squares attributable to m has been shown to be very small indeed."

nique to Berent's data. Goodman's method is less explicitly theoretical than either the model employed by Blau and Duncan or the models which I presented as formulations of the "Halfway hypothesis." It is useful when one is not sure where to start in analysing a body of data. It is not, however, wholly atheoretical, since any particular application of it must involve choosing some possible analyses and rejecting others. It may, therefore, fail to detect real effects while finding spurious "effects" which are compounds of real effects and non-effects. Simpson's employment of the method illustrates these points, since he extracts five 2 x 2 subsystems from Berent's data and subjects each to a Husband's Origin x Present Social Class factorial analysis. Why does he choose this particular set of five? Each set is a pairing of classes; and with four classes, there are six possible pairings. Why is the pairing of class I with class IV omitted?⁴ The answer is that, quite rightly, Simpson terminates his analysis when he has used up all fifteen degrees of freedom in the data. It is however clear that the variance which might have been extracted by the analysis of classes I and IV must be distributed somewhere among the five existing analyses. This illustrates the difficulty of data analysis in the absence of explicit theory. Even a small body of data may be handled in various ways, and the interpretation of the results of any one mode of organisation must be uncertain.

It would, of course, be unwise to construct theories and hypotheses merely to simplify the task of the data analyst. If, however, as Duncan and I did, the analyst has set out to test the fit of a hypothesis, the only effective way to attack him is to demonstrate that an equally good or better fit may be obtained on the basis of a different (and, perhaps a priori less probable) hypothesis. If it has been shown that a four-dimensional model gives a good fit to fifteen-dimensional data, it is a step backwards to illustrate the obvious mathematical

fact that it is possible to perform five three-dimensional analyses. As I wrote, "if results of any generalizability are to be achieved, it is desirable to start by postulating effects which (a) have a basis in sociological theory and (b) span as many of the data as possible, so that their fit is to the generality of the available evidence rather than to certain aspects only."

Simpson criticizes the models of my paper for ascribing the same constant to several cells of the data. He fails to notice that, when I move from a model which makes few distinctions among cells (Model 2 with its alternative, which has 5 degrees of freedom) to one which makes more distinctions (Model 3 with its alternative, which has 9 degrees of freedom) no further significant effects are detected. This evidence presents Simpson with a choice of courses: either to provide a distinct hypothesis which will direct our attention to significant dimensions in the data which my models have ignored or confounded with other factors, or to declare that no two cells should be conflated; each should be allowed to feature uniquely and peculiarly in the analysis. The consequence of adopting the second course is a model with as many degrees of freedom as there are possible dimensions in the data. Pursuing it we achieve a perfect and perfectly uninteresting fit.

In closing I would like to make a general point which is of some importance for the future of sociological research. The research worker who adopts the stance: let us accept a given statement of a position, see what flows from it formally, and how its essential features can be rendered into a model, is often attacked by critics whose penchant is for the assertoric rather than the conditional, the experiential rather than the conceptual, and the particular rather than the general. It is always open to such a critic to denounce the truth, usefulness or relevance of the proposition which forms the starting point of a conditional analysis, in which case the attack will be primarily directed at the original author of the proposition. The critic can also always claim to show that the proposition has been inadequately represented by the man who has tried to schematize it in a model. What the critic cannot legitimately do, however, is to treat a conditional analysis as if it were a free-ranging exploration of all important aspects of an area of enquiry. Such explorations have their place, indeed they preponderate in the

⁴This is not by any means the only possible variant of Simpson's analysis. Each of his five analyses, for example, could be rotated through 45° and treated as the kind of mobile versus non-mobile analysis which I illustrated in my paper. An ingenious sociologist might go further and reject the principle of pairing origin and destination classes as a means for producing the subsystems to be analysed. Conformity to this principle implies a choice among theoretical positions, however ill-defined.

literature. But the future of macro-sociological explanation must lie at least as much with the relatively limited, painstaking task of formal conceptual analysis and the translation of theory into models.

A further as yet unpublished paper "Models of Social Mobility and Status Inconsistency" steps outside the self-imposed restrictions of "Social Mobility and Fertility" and uses formal analysis of the additive model to assess its relevance to the theory which it is supposed to test.

Keith Hope,
Nuffield College,
Oxford,
England.

REFERENCES

- Blau, P.M. and Duncan, O.D.
1967 *The American Occupational Structure.* New York: Wiley.
- Duncan, O.D.
1967 "Methodological issues in the analysis of social mobility." Pp. 51-97 in N. J. Smelser and S. M. Lipset (eds.), *Social Structure and Mobility in Economic Development.* Chicago: Aldine.
- Hope, K.
1971 "Social mobility and fertility." *American Sociological Review* 36 (December):1019-32.
"Models of social mobility and status inconsistency." Unpublished paper.
- 1972 "Quantifying constraints on social mobility." Pp. 121-90 in K. Hope (ed.), *The Analysis of Social Mobility: Methods and Approaches.* Oxford: Clarendon Press.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Two categories of ASA membership (Members and Student Members) are eligible to submit manuscripts (100 to 300 typed pages; three copies) for publication in the ASA Arnold and Caroline Rose Monograph Series in Sociology to the Series Editor, Professor Ida Harper Simpson, Department of Sociology, Duke University, Durham, North Carolina 27706.

and 2) the variation in the size of the "death-dip" by sex, age, race, and geographic location. He is also investigating the degree to which suicide statistics are underreported. Co-author with Theodore M. Newcomb of *The Impact of College on Students* and editor of a recently published book of readings on the social psychology of higher education, Kenneth Feldman's research interests continue to focus on the formation, maintenance, and transformation of identities in college and other settings.

■ **WILLIAM H. FORM** is Professor of Sociology at the University of Illinois. He is also associated with the university's Institute of Labor and Industrial Relations. A monograph (*Technology and Social Behavior*) is in preparation, focusing on the impact of technology on the integration of industrial workers in various social systems. A monograph (*Income and Ideology*) has recently been completed and another (*Political Stratification*) is in preparation, both with Joan Huber.

■ **CARL A. SHEINGOLD** is Assistant Professor of Sociology at Cornell University. Along with a continuing interest in social networks and voting, his major work at the moment is concerned with developing explanatory models of the pattern of American political history.

■ **CLARK MCPHAIL** is Assistant Professor of Sociology at the University of Illinois-Urbana. He is currently examining data on marches, street actions and other types of collective locomotion within assemblages. He is also preparing a monograph on theoretical and methodological problems and strategies in the study of collective behavior. His collaborator, **DAVID MILLER**, is a Ph.D. candidate in Sociology at the University of Illinois-Urbana. He is also a lecturer in the Department of Sociology at Western Illinois University. Like McPhail, his major research interests concern different forms of collective behavior.

■ **SANDRA BALL-ROKEACH** is Assistant Professor of Sociology at Washington State University. While a staff member of the National Commission on the

Causes and Prevention of Violence, she conducted research on the effects of televised violence and co-authored the volume, *Violence and the Media*. She is presently conducting research on the determinants of receptivity to sex-role change and the impact of ideology, perceived competence, and perceived morality on political decision making.

■ **JOHN HENRY FREEMAN** is Assistant Professor of Sociology at the University of California, Riverside. His central interests are in the human ecology of formal organizations and methodology. He is conducting research on the structure of military organizations and on the growth and decline of California school districts.

■ **JOHN L. HAMMOND, JR.** is Assistant Professor of Sociology at Columbia University. His interest in problems of ecological correlations, the subject of his current paper, arose out of his research on revivalism and politics in nineteenth century America, the subject of a paper to appear in a future issue of ASR. This and similar studies of contemporary politics relate to his broader interests in the relation between ideologies and political cleavages.

■ **DAVID L. KLEMMACK** is Assistant Professor of Sociology at Virginia Polytechnic Institute and State University. His current research activities include an examination of the determinants of female occupational selection and a comparison of productivity potential across disciplines. **THOMAS LEGGETTE** is a graduate student in sociology at VPI&SU. He is studying the judicial sentencing process. **LAWRENCE S. MAYERS** is Assistant Professor of Statistics at VPI&SU. His primary interest is in exploring the applicability of mathematical and statistical methods and models to sociological problems.

■ The issue closes with provocative exchanges between authors of previously published articles and their critics. Happy holidays to all — authors, critics, and readers. Enjoy, enjoy.

J.F.S.

INDEX

AMERICAN SOCIOLOGICAL REVIEW

Published Bimonthly by the American Sociological Association

CONTENTS OF VOLUME 38, NUMBERS 1-6

ARTICLES

Anderson, James G. Causal Models and Social Indicators: Toward the Development of Social Systems Models	285
Babchuk, Nicholas. See Williams, J. Allen, Jr.	637
Bailey, Kenneth D. Monothetic and Polythetic Typologies and Their Relation to Conceptualization, Measurement and Scaling	18
Ball-Rokeach, Sandra J. Values and Violence: A Test of the Subculture of Violence Thesis	736
Blume, S. S. Chemists in British Universities: A Study of the Reward System in Science	126
Bonacich, Edna. A Theory of Middleman Minorities	583
Brannon, Robert. Attitude and Action: A Field Experiment Joined to a Natural Population Survey	625
Browning, Harley L. Income and Veteran Status: Variations Among Mexican Americans, Blacks and Anglos	74
Cartwright, Bliss C. The Invocation of Legal Norms: An Empirical Investigation of Durkheim and Weber	340
Clark, Terry Nichols. See Morgan, William R.	611
Cohen, Roberta S. See Orum, Anthony M.	62
Cole, Robert E. Functional Alternatives and Economic Development: An Empirical Example of Permanent Employment in Japan	424
Coleman, James S. Loss of Power	1
Cyphers, Gary. See Brannon, Robert	625
Farley, Reynolds. See Hermalin, Albert I.	595
Featherman, David L. See Hauser, Robert M.	302
Feldman, Kenneth A. See Phillips, David P.	678
Fischer, Claude S. On Urban Alienations and Anomie: Powerlessness and Social Isolation	311
Form, William H. The Internal Stratification of the Working Class: System Involvements of Auto Workers in Four Countries	697
Freeman, John Henry. Environment, Technology and the Administrative Intensity of Manufacturing Organizations	750
Goldman, Daniel R. Managerial Mobility Motivations and Central Life Interests ...	119
Gray, Louis N. See vonBroembsen, M. H.	476
Gurevitch, Michael. See Katz, Elihu	164
Hass, Hadassah. See Katz, Elihu	164
Hall, Peter M. See Hewitt, John P.	367
Hammond, John L., Jr. Two Sources of Error in Ecological Correlations	764
Handwerker, W. Penn. Technology and Household Configuration in Urban Africa: The Bassa of Monrovia	182
Hastings, Donald W. Re-Examination of Hernes' Model on the Process of Entry into First Marriage for United States Women, Cohorts 1891-1945	138
Hauser, Robert M. Trends in Occupational Mobility, 1962-1970	302
Heap, James L. On Phenomenological Sociology	354
Hermalin, Albert I. The Potential for Residential Integration in Cities and Suburbs: Implications for the Busing Controversy	595
Hesse, Sharlene. See Brannon, Robert	625
Hesselbart, Susan. See Brannon, Robert	625
Hewitt, John P. Social Problems, Problematic Situations, and Quasi-Theories	367
Hopkins, Andrew. Political Overconformity by Upwardly Mobile American Men	143

Huber, Joan. Symbolic Interaction as a Pragmatic Perspective: The Bias of Emergent Theory	274
Jackman, Mary J. Education and Prejudice or Education and Response-Set?	327
Jackman, Mary R. An Interpretation of the Relation Between Objective and Subjective Social Status	569
Jackman, Robert W. See Jackman, Mary R.	569
Jacobson, Barbara. Education and Mobility: From Achievement to Ascription	439
Johnson, David R. See Williams, J. Allen	637
Katz, Elihu. On the Use of the Mass Media for Important Things	164
Keane, Roberta. See Brannon, Robert	625
Kelley, Jonathan. Causal Chain Models for the Socioeconomic Career	481
Kendrick, John M. See Jacobson, Barbara.	469
Kennedy, Robert E., Jr. Minority Group Status and Fertility: The Irish	85
Klemmack, David L. Non-Random Exogenous Variables in Path Analysis	778
Kohn, Melvin L. Occupational Experience and Psychological Functioning: An Assessment of Reciprocal Effects	97
Komarovsky, Mirra. Some Problems in Role Analysis	649
Kunkel, John H. A Behavioral Model of Man: Propositions and Implications	530
Laumann, Edward O. New Directions in the Study of Elites	212
Leggette, Thomas A. See Klemmack, David L.	778
Lopreato, Sally Cook. See Browning, Harley L.	74
Madron, Thomas W. See Nelsen, Hart M.	375
Mason, Karen Oppenheim. Some Methodological Issues in Cohort Analysis of Archival Data	242
Mason, William M. See Mason, Karen Oppenheim	242
Mayer, Lawrence S. See Klemmack, David L.	778
Mayhew, Bruce H. System Size and Ruling Elites	468
Mazur, Allan. A Cross-Species Comparison of Status in Small Established Groups ..	513
McPhail, Clark. The Assembling Process: A Theoretical and Empirical Examination ..	721
Middleton, Russell. Do Christian Beliefs Cause Anti-Semitism?	33
Miller, David L. See McPhail, Clark	721
Morgan, William R. The Causes of Racial Disorders: A Grievance-Level Explanation ..	611
Movahedi, Siamak. Axiomatic Theory, Informative Value of Propositions, and "Derivation Rules of Ordinary Language"	416
Nagasawa, Richard H. See Kunkel, John H.	530
Nelsen, Hart M. Ministerial Roles and Social Actionist Stance: Protestant Clergy and Protest in the Sixties	375
Ogles, Richard H. See Movahedi, Siamak	416
Orum, Anthony M. The Development of Political Orientations Among Black and White Children	62
Paige, Jeffery M. See Paige, Karen E.	663
Paige, Karen E. The Politics of Birth Practices: A Strategic Analysis	663
Pappi, Franz Urban. See Laumann, Edward O.	212
Phillips, David P. A Dip in Deaths Before Ceremonial Occasions: Some New Relationships Between Social Integration and Mortality	678
Poole, W. Kenneth. See Mason, Karen Oppenheim	242
Pope, Whitney. Classic on Classic: Parsons' Interpretation of Durkheim	399
Poston, Dudley L., Jr. See Browning, Harley L.	74
Ritterband, Paul. Group Disorders in the Public Schools	461
Robinson, J. Gregory. See Hastings, Donald W.	138
Rosen, Bernard C. Social Change, Migration and Family Interaction in Brazil	198
Rosenberg, Florence. See Simmons, Roberta G.	553
Rosenberg, Morris. See Simmons, Roberta G.	553
Roth, Phillip A. See Heap, James L.	354
Schooler, Carmi. See Kohn, Melvin L.	97
Schuman, Howard. See Brannon, Robert	625
Schwartz, R. D. See Cartwright, Bliss C.	340
Sheingold, Carl A. Social Networks and Voting: The Resurrection of a Research Agenda	712
Silberstein, Richard. See Ritterband, Paul	461
Simmons, Roberta G. Disturbance in the Self-Image at Adolescence	553
Sinclair, Ruth. See Blume, S. S.	126
Smith, H. W. Some Developmental Interpersonal Dynamics Through Childhood ...	543
Specht, David A. System Size and Structural Differentiation in Formal Organizations: An Alternative Baseline Generator	479
Viccaro, Thomas. See Brannon, Robert	625
vonBroembsen, Maximilian H. Size and Ruling Elites: Effects of System Growth on Power Structure	476

Webster, Murray. Psychological Reductionism, Methodological Individualism, and Large-Scale Problems	258
Whyte, Martin King. Bureaucracy and Modernization in China: The Maoist Critique	149
Williams, J. Allen. Voluntary Associations and Minority Status: A Comparative Analysis of Anglo, Black, and Mexican Americans	637
Williams, James M. The Ecological Approach in Measuring Community Power Concentration: An Analysis of Hawley's MPO Ratio	230
Winsborough, H. H. See Mason, Karen Oppenheim	242
Wright, Diana. See Brannon, Robert	625
Yokley, Raytha L. See Nelsen, Hart M.	375

COMMENTS

Abbott, Carrell W. Exchange as Symbolic Interaction: For What?	504
Blumer, Herbert. Comment on "Symbolic Interaction as Pragmatic Perspective: The Bias of Emergent Theory"	797
Brower, Milton J. See DeJong, Peter Y.	807
Brown, Charles R. See Abbott, Carrell W.	504
Burke, Peter J. Comment on "Role Differentiation"	801
Crosbie, Paul V. See Abbott, Carrell W.	504
DeJong, Peter Y. Patterns of Female Intergenerational Occupational Mobility: Response to Ramsøy	807
Dieckhoff, Foster. On Hummon's Mathematical Formulation of Blau's Theory of Differentiation in Organizations	387
Featherman, David L. Comments on Models for the Socioeconomic Career	785
Fox, John W. Comment on "Hall's Professionalism Scale: An Empirical Reassessment"	392
Glock, Charles Y. Do Christian Beliefs Cause Anti-Semitism?—A Comment	53
Halaby, Charles N. "Hardship and Collective Violence in France": A Comment	495
Hawley, Amos H. Comment on Williams' "The Ecological Approach in Measuring Community Power Concentration"	390
Hope, Keith. Reply to Simpson	809
Huber, Joan. Reply to Blumer: But Who Will Scrutinize the Scrutinizers?	798
Kelley, Jonathan. History, Causal Chains and Careers: A Reply	791
Lewis, Gordon H. Reply to Burke	802
Macdonald, K. I. Ordinal Regression? A Comment	494
Middleton, Russell. Response	59
Ramsøy, Natalie Rogoff. Patterns of Female Intergenerational Occupational Mobility: A Comment	806
Robin, Stanley S. See DeJong, Peter Y.	807
Simpson, Miles E. Comments on Hope's Mobility and Fertility Paper	509
Singelmann, Peter. On the Reification of Paradigms: Reply to Abbott, Brown, and Crosbie	506
Snizek, William E. Reply to Fox and Vonk	395
Snyder, David. How to Get From Here to There	501
Stark, Rodney. See Glock, Charles Y.	53
Tilly, Charles. See Snyder, David.	501
Vonk, John A. See Fox, John W.	392
Williams, James M. Reply to Hawley	391